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SCIENCE PROGRESS

VERTEBRATE PALÆONTOLOGY IN 1912

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By far the most striking event of the year, so far as vertebrate palæontology is concerned, is the discovery by Mr. Charles Dawson, in a shallow bed of high-level gravel at Piltdown, in the parish of Fletching, Sussex, of portions of a cranium and lower jaw which indicate a being intermediate in many respects between man and the man-like apes. In describing these specimens at the meeting of the Geological Society held on December 18, 1912 (*Abstracts Proc. Geol. Soc.* No. 932, Dec. 28, 1912), Dr. A. Smith Woodward referred to these remains as "human"; but as they are regarded as representing a distinct generic type, it may be a question whether they have any right to that title; it is perhaps better to refer to them as man-like.

Mr. Dawson states that the skull was broken up by the workmen who found it and most of the fragments thrown away. On the other hand, Sir E. R. Lankester, in an article in the *Daily Telegraph* of December 19, 1912, asserts that it was broken when discovered and that the fractured parts had been slightly wôrn before entombment in the gravel. The lower jaw was dug up by Mr. Dawson in an undisturbed patch of gravel a short distance away from the spot where the skull was found. Although certain doubts were expressed at the meeting whether the skull and lower jaw belonged to the same individual, there can be no hesitation in regarding them as associated and there are some reasons for believing them to pertain to a female.

The gravel, which lies at a height of 80 ft. above the Ouse, also yielded more or less imperfect teeth of an elephant, a mastodon, a horse, a hippopotamus and a beaver, as well as a fragment of the antler of a red deer and Palæolithic imple-

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ments of the Chellean type. Messrs. Dawson and Woodward conclude that the gravel-bed is of the same age as the embedded Chellean implements, which are less water-worn than most of the associated flints; but that the teeth of the elephant (which is of a Pliocene type) and mastodon are derived from older (Pliocene) gravels, while the skull and jaw belong to the period of the bed in which they were found. The remoteness of that period is indicated by the subsequent excavation of the Ouse valley to a depth of 80 ft. On the other hand, Sir E. R. Lankester, after first committing himself to the statement (*Daily Telegraph*, Dec. 19, 1912) that the skull and jaw "were probably embedded for the first time in the existing gravel and not washed out of a previous deposit," subsequently shifted his ground and asserted (*op. cit.* Jan. 6, 1913) that the specimen "was undoubtedly washed into the gravel where it was found from a previous deposit."

The skull, which lacks the bones of the face, and is otherwise imperfect, is stated by Dr. Smith Woodward to exhibit all the essential features of that of modern man (*Homo*) and has a brain-capacity of at least 1070 c.c. It is, however, remarkable for the excessive thickness of the bones of the roof, which averages 10 mm. and in one spot reaches 12 mm. The forehead is steeper than in skulls of the Neanderthal type but shows only slight development of the brow-ridges and also affords evidence that the plate of bone (tentorium) dividing the cerebral hemispheres from the cerebellum occupies the same relative position as in modern man. Viewed from the back, the skull is remarkably low and broad, with relatively small mastoid processes.

Of the lower jaw, the right half or ramus is nearly complete, with the exception of the loss of the articular condyle, as far forward as the middle of the bond of union or symphysis with its fellow of the opposite side. Unfortunately, however, only two teeth, the first and second molars, remain, although the socket of the third is preserved. In place of the thickened and rounded posterior border of the symphysis and the prominent chin of man, this portion of the jaw slopes regularly upwards towards the position which would be occupied by the bases of the front teeth. In fact, whereas a modern human jaw, when viewed from below, has the appearance of a horseshoe-like arch, the Sussex jaw has a contour recalling that of a pair

of pliers when closed. In these respects the jaw is essentially that of a chimpanzi. The two molars, which are essentially human in structure, " have been worn perfectly flat by mastication, a circumstance suggesting that the canines resembled those of man in not projecting sensibly above the level of the other teeth." Thus writes Dr. Woodward. On the other hand, Sir E. R. Lankester expresses the opinion (*D.T.* Dec. 19, 1912) that the Sussex jaw " had almost certainly great canines and large front teeth." It should be added that in the shallowness of the notch separating the articular condyle from the coronoid process the Sussex jaw approximates to the Pleistocene Heidelberg jaw, which, however, is of a much more massive type, and, although lacking a prominent chin, has a comparatively short symphysis.

Perhaps the most remarkable feature of the Sussex " man " is the association of a distinctly human type of cranium with an equally marked simian form of lower jaw. This, however, according to Dr. Elliot Smith, who contributed an appendix to the original description, is no matter for surprise, as increasing brain-development in the forerunners of man must have involved more rapid growth and change in the cranium than in other parts of the skeleton. Special interest also attaches to a remark by the same observer that the region of the brain believed to be associated in man with the power of speech is but poorly developed in the Sussex skull. Not improbably, therefore, the half-man and half-ape of the Sussex Weald was devoid of the power of articulate speech.

Be this as it may, it is evident, to quote the words of Sir E. R. Lankester, that these remains, in spite of their imperfection, " are of extreme importance, and constitute a new step in the acquirement of solid, tangible knowledge as to the development of man from ape-like ancestors. This half of a lower jaw from Sussex furnishes . . . evidence of a man-like creature really intermediate between man and ape. It comes nearer to the realisation of ' the missing link' than anything yet discovered."

In the published abstract of the original description no scientific designation was given to this missing link; but in the full text of the paper, published in vol. lxix. of the *Quarterly Journal* of the Geological Society (pp. 117–51), the new generic and specific title of *Eoanthropus dawsoni* is proposed.

Compared with the foregoing, the rest of the year's work on fossil mammals appears insignificant; as a matter of fact, it is distinctly below the average in interest and importance.

As standing on the border-land between zoology and palæontology, brief reference may be made to the handsome volume by Messrs. Rio, Breuil, and Sierra on the mural sketches of animals from Spanish caves, published under the auspices of the Prince of Monaco. In connexion with this may be noticed the identification by Mr. E. P. Newberry (*Klio*, vol. xii. pp. 397 *et seq.*) of "the animal of Set" or Typho, so frequently represented in ancient Egyptian frescoes, with the wart-hog (*Phacochœrus africanus*). Many previous attempts at the identification of the animal in question—which has been considered to represent the okapi—have been made, but the controversy now seems to be finally decided.

Hitherto there has been a gap in our knowledge of the forms of the horse existing between the modern period and the early metal age. This is to some extent filled by the discovery of a skeleton in the superficial formations of Neukölln (formerly Rixdorf), near Berlin. According to Dr. Max Hilzheimer (*Zool. Anz.* vol. xl. pp. 105–17), this skeleton indicates a small but well-formed breed of the western type akin to the existing so-called " Reitpferd."

The same author also describes (*Zeits. Morph. u. Anthrop.* vol. xv. pp. 229–46) remains of a dog and other domesticated animals from a stratum of the third or fourth century at Paulinenaue, Mark.

The Vienna University recently sent an expedition to collect fossil mammals from the well-known deposits of Pikermi, Attica; a report on the results is contributed by Dr. O. Abel in the *Verh. Zool.-Bot. Ges. Wien*, vol. lxii. pt. 2, pp. 61–3

The same author, it may be mentioned here, has published (*Zool. Jahrb.* 1912, suppl. 15, Bd. i. pp. 597–609) notes on adaptation in extinct animals.

The Miocene mammalian fauna of Venice is reviewed, with a number of illustrations, by Mr. Stefanini in the first part of a new serial, *Mem. Ist. Geol. Padova*, vol. i. pp. 267–318.

In Australia Mr. Glauert (*Rec. W. Austral. Mus.* vol. i. pp. 37–46) gives a list of the fossil mammals found in the miscalled Mammoth Cave; while Mr. J. Mahony (*Victoria Naturalist*, vol. xxix. pp. 43–6) records the occurrence of

remains of the Tasmanian devil (*Sarcophilus ursinus*) on the mainland in association with those of various extinct marsupials.

The latest of Dr. Stehlin's valuable contributions to our knowledge of the extinct mammalian fauna of Switzerland (*Abh. schweiz. pal. Ges.* vol. xxxviii. pp. 1165–1298) relates to the osteology and dentition of the lemuroid genus *Adapis*, of which a new species is described. As the result of his studies, the author confirms the opinion that *Adapis* should be included in the Lemuroidea and that its affinities are probably nearer to the *Lemurinæ* than to either the *Indrisinæ* or *Chiromyinæ*. The genus cannot however be regarded as ancestral to any of the existing or Pleistocene representatives of the group but represents a completely extinct collateral branch.

The cave-lion (*Felis leo fossilis*), as exemplified by remains from the neighbourhood of Heidelberg, forms the subject of a memoir by Mr. A. Wurm in the *Jahresber. oberrhein. Geol. Ges.* ser. 2, 1912, pp. 77–102.

Two other noteworthy papers on fossil Carnivora have appeared during the year. In the first of these Prof. Sidney Reynolds reviews the British Pleistocene *Mustelidæ* in the Palæontographical Society's volume for 1911, published in February 1912. No new forms are described.

In the second Dr. J. Merriam (*Mem. Univ. California*, vol. i. No. 2) describes the skeletons and teeth of wolves and other *Canidæ* from the Pleistocene asphalt-beds of Rancho La Brea, California. Many of these belong to the great extinct wolf for which the late Prof. Leidy proposed the name of *Canis dirus*; they serve to show that this species, although near akin to the existing so-called timber-wolf, was bigger than any other known member of the group, not even excluding the great black Alaskan wolf (*C. pambasileus*). Two other species, *C. milleri* and *C. andersoni*, are likewise regarded as extinct; the former being related to the timber-wolf, which is stated to present certain resemblances to the coyote or prairie wolf. Yet other kinds are regarded as representing extinct races of existing American *Canidæ*.

In another article (*Univ. California Pub., Bull. Dep. Geol.* vol. vii. pp. 37–46) the writer last mentioned records the occurrence in the La Brea asphalt of bears referable to the extinct genus *Arctotherium* and the modern *Ursus*, as well as of

of the Austrian Geological Survey, vol. lxii. pp. 87–182. The same subject, as exemplified by the affinities of the Pleistocene European *E. antiquus*, forms the subject of an article by Mr. Zuffardi in *Atti. R. Ac. Lincei*, ser. 2, vol. xxi. pp. 298–304.

In 1911 Dr. Schlesinger provisionally referred an elephant's tooth from Lower Austria to the Siwalik *E. planifrons*; this determination he confirms in a later paper published in *Verh. Zool.-Bot. Ges. Wien*, vol. lxii. pt. 2, p. 55. Two unusually fine skeletons of the mammoth have recently been placed on exhibition. The first of these, which is in the Museum at Stuttgart, is reported to be the largest known, and was found at Steinheim, in Swabia, in the summer of 1910. The tusks are of no very great size, measuring 7½ ft.; but the skeleton is remarkable for the great relative length of the legs, especially the front pair, as well as for the unusual width of the molars. The second skeleton, which has been set up in the Völkerkunde Museum at Leipzig, is nearly complete and has been described by Dr. J. Felix in the *Veröffentlichungen der Städt. Mus. für Völkerkunde* for 1912. In was discovered in December 1908 under a considerable thickness of sand and clay, near Borna, its presence being revealed by the tip of one of the tusks. This skeleton stands 3·20 metres in height.

Brief notice will suffice for a paper by Dr. A. Andreuxi (*Riv. Ital. Pal.* vol. xviii. pts. 2 and 3, pp. 88–90) on remains of *E. meridionalis* from the Italian Pliocene; and to a second, by Dr. Pohlig (*Bull. Soc belge Géol.* vol. xxvi. *Proc. Verb.* pp. 187–93), on a lower jaw of the American *Mastodon americanus* with the left permanent tusk *in situ*. Dr. Pohlig appears to be of opinion that this specimen is unique in this respect; but an example with the right tusk was recorded in 1886 by the present writer (*Cat. Foss. Mamm. Brit. Mus.* pt. iv. p. 21).

Another mummified carcase of a rhinoceros has been discovered in the ozokerit beds of Starunia, Galicia, which has been described by Dr. Abel in the *Verh. Zool.-Bot. Ges. Wien*, vol. lxii. pts. 2, 3, pp. 79–82; the species in this instance being the woolly *Rhinoceros antiquitatis*.

During the year Mr. Ivar Sefve has made a further contribution to our knowledge of the extinct *Equidæ* of South America, in a memoir published in the *K. Svenska Vet.-Ak. Handlinger* (vol. xlviii. No. 6). Among the groups recognised are *Hyperhippidium* and *Parahipparion*; a new species of the latter being named in

honour of the late Prof. Burmeister, the pioneer of Argentine palæontology.

The titanotheres of the Uinta beds of Utah have engaged the attention of Mr. E. S. Biggs, who, in addition to naming a new genus and several species (*Field Mus. Geol. Publ.* vol. iv. pp. 17–41), comments on the rapid evolution and short life of some of the groups of these perissodactyles.

Turning to marine mammals, it may be mentioned that in the group of Sirenia the scapula of *Halitherium schinzi* was described in 1911 by Mr. O. Schmidtgen (*Centralblatt für Mineral,* 1911, pp. 221–3); and also that during the year under review Dr. R. Issel (*Mem. R. Ac. Lincei,* ser. 5, vol. ix. pp. 119–25) has contributed a note on the corresponding bone of the allied genus *Felsinotherium.* The first-named writer has likewise recorded (*Zool. Jahrbuch,* 1912, suppl. 15, vol. ii. pp. 449–95) some new observations with regard to the structure of the pelvis and hind-limb of *Halitherium.*

Fossil whales akin to the modern rorquals and finners form the subject of an article by Prof. F. W. True in vol. lix. No. 6 of the *Smithsonian Miscellaneous Collections,* which mainly consists of a summary of a paper in Danish by Dr. H. Winge. Both writers consider that among a multitude of generic divisions which have been proposed, *Aulocetus, Cetotherium, Herpetocetus,* and *Plesiocetus* are valid; and of these, as well as of the two allied existing genera, *Balænoptera* and *Megaptera,* diagnoses based on osteological characters are appended.

It is gradually becoming evident that the South American freshwater dolphins of the family *Iniidæ,* now represented by the genera *Inia* and *Pontoporia,* each with a single species, had numerous forerunners during Tertiary times. The latest addition to the list is *Hesperocetus californicus,* a genus and species established by Prof. True (*Smithson. Misc. Collect.* vol. lx. No. 11) on the evidence of an imperfect lower jaw, with teeth, from the Californian Tertiaries. This genus, which is provisionally referred to the *Iniidæ,* is remarkable for the length of the symphysis of the lower jaw and the large size of the teeth, which recall those of the extinct *Delphinodon,* classed by the author with the *Delphinidæ.* Other extinct *Iniidæ* are *Saurodelphis, Pontoplanodes* and *Ischyrorhynchus,* all exclusively American.

In a second article, Dr. True (*Journ. Ac. Nat. Sci. Philadelphia,*

Chicago, for 1911 (vol. xix. pp. 696–745) but not noticed in my review of the work of that year. This same paper also contains a restoration of *Nyctosaurus*.

In connexion with the above may be noticed a very interesting article by Messrs. E. and A. Harlé, published in *Bull. Soc. Géol. France*, vol. xi. pp. 118–21, on the means by which the giant pterodactyles of the American Cretaceous were enabled to fly. For permission to reproduce, in a somewhat condensed form, the following abstract of this most interesting article, I am indebted to the editor of *The Field*.

Some of these pterodactyles had a wing-expanse of at least from 21 to 24 ft., whereas the largest flying birds of the present day, such as the albatrosses, condors, lammergeiers, and marabout storks, have not more than about half the bulk of the former, although they have probably attained the maximum size compatible, under present physical conditions, with the power of flight. For studies of the flight of birds and insects in connexion with the theory of aeroplanes have demonstrated that the power necessary to propel animals through the air varies per unit of weight approximately as the sixth root of the weight; that is to say, as the square root of their dimensions. Accordingly, the power required increases more rapidly than the weight and the dimensions. If, for instance, the dimensions be quadrupled a power is required per unit of weight equal to that originally sufficient multiplied by the square root of four; that is to say, the power must be doubled per unit of weight. From this it is evident that a limit to the weight, and consequently to the size, of animals capable of flight must be reached. But the pterodactyles of the Cretaceous greatly exceeded these limits, yet, from the situations in which their skeletons are found in Kansas, it is evident that they were able to fly distances of at least 100 miles from the shore. Probably they performed skimming flights above the waves in pursuit of surface-swimming fish, for the capture of which the structure of the skull and beak seems adapted. Again, if we go back in time to the Carboniferous period of France, we find gigantic dragon-flies with a wing-expanse of from 28 to 32 in., which it is certain would be unable to fly, from lack of sufficient propelling power, under present physical conditions, as they are fully three times the size of the biggest of their existing relatives,

How, then, were these giants capable of flight? One suggestion is that the attraction of gravity, owing to the diameter of the earth having been greater, was less in past epochs than at the present day. But an increase in the earth's radius of some 60 miles, which is the maximum that could be allowed, would cause but slight diminution in the pull of gravity. On the other hand, an increase in atmospheric pressure would have much more effect on the flying capacity of animals. Suppose, for example, an animal flying by wing-beats (and it is certain that pterodactyles did not glide from trees or cliffs in aeroplane-fashion), in which the wing-expanse was double that of the largest modern birds. From the formula given above it will be evident that under existing conditions such an animal would require, per unit of weight, a power equal to that of our largest birds multiplied by the square root of four (in other words, doubled), which would manifestly be impossible to realise. But if the atmospheric pressure were four times as great as at present, flight would be possible with the power diminished by one-half. And, as a matter of fact, the necessary power, per unit of weight, being doubled in one way and halved in another, would remain the same and be no greater than in the case of existing birds. Accordingly, an augmentation in atmospheric pressure in the proportion of one to four would compensate a similar increase in the size of the animal. So that we have the general rule that all increase in the size of the animal would be compensated by a proportional augmentation of pressure. Thus in the case of the largest known pterodactyles, of which the wing-expanse was about double that of the biggest living birds, the impossibility of flight on account of their size would be annulled by a double atmospheric pressure. If the temperature were higher than at the present day there would be a further slight increase in the pressure. The fact, then, that giant reptiles which could not fly under present conditions did do during the Cretaceous, coupled with the similar case presented by the giant dragon-flies of the Coal period, leads the authors to regard (so far as conclusions of this kind have any value and always bearing in mind the possibility that nature may have utilised means of which we have no cognisance) an increased atmospheric pressure during geological time as the most plausible and probable explanation of the problem.

During the past few years the dinosaurian quarries of Tenda-

guru, German East Africa, have been worked with great energy and a vast number of gigantic bones transported to Berlin. An account of the excavations and descriptions of some of the bones, by Mr. Janensch and others, will be found in *Sitzber. Ges. natfor. Freunde* for 1912. According to this, the biggest of the Tendaguru dinosaurs is remarkable for the huge dimensions of the scapula and humerus, which are proportionately much larger than in other species and actually bigger than any other known specimens. The biggest humerus measures rather more than 6 ft. 6 in. in length. Of this enormous bone a cast has been acquired by the Natural History Museum. The dinosaur to which this great bone belonged is believed to be near akin to *Diplodocus* but with a relatively as well as actually larger scapula and fore-limb. Another paper, by Dr. E. Hennig, on the possible occurrence of the Tendaguru deposits in other districts appears in the same journal (pp. 493–7).

To the *Memoirs of the American Museum of Natural History*, ser. 2, vol. i. pt. 1, Prof. H. F. Osborn contributes an illustrated account of the skull of the gigantic theropod dinosaur *Tyrannosaurus rex*, from the Upper Cretaceous of Montana, together with notes on the skulls of *Allosaurus* and the Theropoda in general. The skull of *Tyrannosaurus*, which is furnished with a formidable armature of teeth of the megalosaurian type, is not only the largest in the theropod order, but also the most powerful and massive among reptiles as a whole; as may be verified by the inspection of a cast exhibited in the Natural History Museum. A noteworthy feature of the skull is the fusion of the vomers into a single diamond-shaped plate, articulating posteriorly by a long style with the pterygoids, since a practically identical structure exists in the ostrich and its relatives. As an adaptive modification correlated with the powerful dentition, attention is specially directed to the antero-posterior shortening of the skull and the reduction of the number of pairs of teeth from twenty (in *Allosaurus*) to sixteen. This abbreviation of the skull is paralleled among modern cats and certain extinct dog-like carnivores. The homology of certain bones of the theropod skull is also discussed. A second article in the same issue is devoted to the description, by Prof. Osborn, of the "mummified" skin of *Trachodon annectans*, an iguanodont

dinosaur from the Upper Cretaceous of Wyoming. As this wonderful specimen was noticed and an illustration of 'a portion of the skin was given in my last year's article, further mention is unnecessary.

The structure of the fore-foot of the genus *Trachodon* is discussed fully in the *Bull. Amer. Mus. Nat. Hist.* vol. xxxi. pp. 105-7, by Mr. Barnum Brown, who shows that there are four toes, of which the two corresponding with the second and third in the typical pentadactyle series are furnished with hoofs. Unlike the European *Iguanodon* and its American representative *Champtosaurus*, the trachodonts were unable to make any use of their fore-limbs in progression.

In a second article in the volume last quoted (pp. 131-6) the same author gives a preliminary description of a new genus and species of trachodont dinosaur (*Saurolophus osborni*) from the Cretaceous of Edmonton, Alberta, characterised by the development of a tall crest immediately above the eye-sockets. It is also shown that, in common with other members of the trachodont group, these dinosaurs had a ring of bones in the sclerotic of the eye.

Bare mention will suffice for an article by Prof. R. S. Lull on a restoration of the skeleton and external form of the armoured dinosaur *Stegosaurus*, published, during the year under review, in *Verhandlungen des VIII. Internat. Zool. Kongress zu Graz* of 1910. In connexion with this may be mentioned an article by Prof. G. R. Wieland (*Science*, vol. xxxvi. pp. 287-8) on the analogy between the bony plates of the armoured dinosaurs and the shells of the chelonians; an analogy first suggested by the same writer in 1911. In the present article this idea is further developed, the author expressing the opinion that "dinosaurs, instead of eventually confining extensive dermal development to a single nether layer covering the body-region only, as in the turtles, tended to develop both the nether and outer layers in the body or skull or both. And this is only another but definite way of saying that dermal armature was variously developed in the Dinosauria or that it tended to assume bizarre patterns."

An armoured dinosaur, *Stegopelta landerensis*, from the Cretaceous of Wyoming, forms the subject of a short paper by Dr. R. S. Moodie published in 1911 in the *Kansas Science Bulletin*, ser. 2, vol. v. pp. 257-73.

Finally, a general review of the distribution of Cretaceous dinosaurs by Dr. Lull in the *Bull. Geol. Soc. America*, vol. xxiii. pp. 208–12, contains a considerable amount of new and interesting information on this subject.

Two new South African genera and species referred to the Parasuchia (or Thecodontia), as typified by the European *Phytosaurus* (*Belodon*), are described by Mr. D. M. S. Watson in the second volume of the *Records of the Albany Museum*, under the names of *Mesosuchus browni* and *Eosuchus colletti*. They appear to be more or less nearly related to the gigantic *Erythrosuchus*, which, like the two new forms, occurs in the South African Karu formation.

In an article on the remains of crocodilians from the Upper Tertiaries of Parana, published in vol. xxi. of *Anales del Museo Nacional de Buenos Aires*, Mr. C. Rovereto refers two out of three species to *Alligator*, with the proviso that they may belong, as they almost certainly do, to the South American genus *Caiman*. The third species, which was described by Burmeister as *Rhamphostoma neogæum*, is referred to the existing Indian genus *Garialis*, a reference which is less remarkable than it might appear, seeing that crocodilians of the same generic type occur in the Cretaceous and Eocene of Europe.

From a distributional point of view considerable interest attaches to the description by Prof. L. Dollo, in the science section of the *Bull. R. Ac. Sci. Belge*, 1912, No. 1, pp. 8–9, of a freshwater tortoise of the genus *Podocnemis*, from the Lower Eocene of the Enclave de Cabinda, Congo State. Although now restricted to tropical South America and Madagascar, the genus is represented in the Eocene of England, India, the Fayum, and the Congo.

The paddles and other remains of certain North American Jurassic plesiosaurs form the subject of an article by Mr. M. G. Mehl in the *Journal of Geology*, vol. xx. pp. 344–52. One remarkably fine limb is tentatively assigned to the European genus *Murænosaurus*, under the name *M. reedii*. Possibly the imperfect specimen described by another writer as *Plesiosaurus shirleyensis*, which certainly does not belong to the genus to which it is referred, may represent an allied type. Finally, the so-called *Cimoliosaurus laramiensis* is considered to be not improbably referable to *Tricleidus*, a genus established by

Dr. C. W. Andrews for a plesiosaur from the Oxford Clay of Peterborough.

The lower jaw of a gigantic ichthyosaur discovered in the Trias of Aust Cliff in 1877 and preserved in the museum at Bristol is discussed by Prof. von Huene in *Centralbl. für Min. Geol. u. Pal.* 1912, pp. 61-3, by whom it is considered to be related to *Mixosaurus* and *Merriamia.*

At the conclusion of a memoir on the structure of the skull of that very remarkable Triassic reptile *Placodus*, whose bean-like teeth seem evidently adapted for crushing the shells of molluscs or crustaceans, Mr. F. Broili (*Palæontographica*, vol. lix. pp. 147-55) remarks that the skull-roof possesses no sign of those bony ridges and rugosities seen on the skull of *Placochelys* but is, on the contrary, entirely smooth. From this it is inferred that *Placodus* probably did not possess a bony carapace, as such a structure was very likely associated with a roughened skull; this being confirmed by the absence of direct evidence that bony plates have been found in association with the skeleton. On the other hand, there may have been a horny plastron. The alleged relationship to *Placochelys* is therefore not borne out by the available evidence.

Passing to the mammal-like groups of early reptiles reference may be made first to an article by Prof. S. W. Williston (*Amer. Journ. Sci.* vol. xxxiv. pp. 457-68) on the restoration of the cotylosaurian genus *Limnoscelis*, from the Permo-Carboniferous of New Mexico. To repeat the author's summary of the osteological characters of the genus would be out of place and it must suffice to mention that this primitive reptile, which attained a length of about seven feet and had remarkably short limbs, probably frequented the borders of swamps and marshes.

To the *Journal of Morphology* for 1912 the same writer contributes an account of the Cotylosauria, the group to which *Limnoscelis* belongs. In a third communication, published in the *Journal of Geology* for 1911 (vol. xix. pp. 233-7), Dr. Williston gives a restoration of *Seymouria*, a relative of *Limnoscelis*, although regarded as typifying a family by itself. It may be added that much interesting information with regard to these and kindred forms may be found in an article by the same palæontologist in the *Journal of Morphology*, vol. xxiii. pp. 637-66, on primitive reptiles in general.

European Triassic Cotylosauria are discussed by Prof. von Huene in the *Palæontographica*, vol. lix. pp. 69–102, pls. v.–ix. *Telerperton* and *Sclerosaurus* are referred to this group; the former, which has very generally been classed among the Rhynchocephalia, being regarded as a near relative of the South African *Procolophon*. A new genus and species, *Koiloskiosaurus coburgense*, is established on the evidence of a skeleton from the Bunter of Coburg.

Here may be appropriately noticed a paper by Mr. D. M. S. Watson (*Ann. Mag. Nat. Hist.* ser. 8, vol. x. pp. 573–87) on the homology and relationships of the elements of the lower jaw in the mammal-like reptiles. However, the subject is one of an extremely technical nature, which it would be useless to attempt to review without the aid of diagrams.

Mention may likewise be made of a second paper by the same author (*op. cit.* vol. viii. pp. 294–330), published in 1911 but not referred to in my review of that year's work, on the skull of the South African *Diademodon*, with notes on the same part of the skeleton in other members of the cynodont group.

A nearly complete skeleton of the South African dicynodont genus long known as *Ptychognathus* but now termed *Lystrosaurus* forms the subject of an article by Mr. Watson in the *Records of the Albany Museum*, vol. ii. pp. 287–95. *Lystrosaurus* appears to have been of aquatic habits and also to have used its powerful pair of upper tusks for digging. It probably dug with its mouth open, scooping up food with the lower jaw. "It is natural to suppose that Lystrosaurus was a vegetable-feeder, as the absence of [cheek] teeth and the presence of a horny beak are more adapted to such a diet than to a carnivorous one. The extraordinary massiveness of the jaws, however, is rather difficult to reconcile with the softness of most aquatic plants and suggests some additional food."

To the *Annals of the South African Museum*, vol. vii. pt. 5, Dr. R. Broom contributes no less than five articles on reptiles of the Trias and Permian of South Africa. In the first of these, after describing a new species of *Propappus*, he gives reasons for believing that its well-known bigger relative *Pariasaurus* stood higher on its limbs than is generally supposed. Both these reptiles appear to have been tortoise-like in habits and probably protected themselves by digging in the ground. In the second paper the author describes a new mosasaurian of the genus

Tylosaurus and in the third a new cynodont from the Stormberg beds. More important are certain observations in the fourth paper on the structure of the dicynodont skull, where it is stated that the bone in which the pineal foramen (that is to say, the aperture for the pineal eye) is pierced is probably a special development in this group, the paired bones behind this representing the parietals.

These early South African reptiles form the subject of another paper by Dr. Broom, published in the *Proceedings of the Zoological Society* for 1912 (pp. 859–76). The remains described are referred to no less than seven new generic types as well as to a number of species included in previously known genera. Although several of these are of considerable interest, none requires special notice on the present occasion.

In an earlier portion of the Zoological Society's *Proceedings* (pp. 419–25) Dr. Broom discusses the structure of the internal ear in dicynodonts and the much-disputed homology of the mammalian auditory ossicles. As regards the latter, he reverts to the old view that the incus corresponds to the reptilian quadrate; the removal of that element from the mandibular articulation being foreshadowed in the Permian African genus *Cynognathus*, in which it has partially slipped out from the joint.

Next to *Eoanthropus*, perhaps the most important discovery of the year in the branch of science under discussion is the identification of a toad from the Jurassic of Wyoming. So long ago as 1887 the late Prof. O. C. Marsh announced that he had evidence of the occurrence of a tailless batrachian in the Como beds of the Montana Jurassic and proposed for it the new generic and specific designation *Eobatrachus agilis*. The two specimens were never properly ˌdescribed or figured and the genus has consequently been ignored by palæontologists. Recently the types have come into the hands of Dr. R. L. Moodie, who expresses himself perfectly satisfied (*Amer. Journ. Sci.* vol. xxx. pp. 286–8) as to the general correctness of the original diagnosis and raises no doubts with regard to the horizon from which the specimens were obtained. He adds that the Como batrachian appears to be a toad, probably referable to the family *Bufonidæ* and possibly even to the existing genus *Bufo*. In stating that the earliest tailless

batrachians hitherto known date from the Oligocene or Eocene, the author has overlooked the description in 1902 by Mr. L. M. Vidal (*Mem. R. Ac. Cienc. Barcelona*, ser. 3, vol. iv. p. 203) of a frog from the reputed Kimeridgian of Montsech, north-eastern Spain, under the name of *Palæobatrachus gaudryi*; the genus being typically from the European Miocene and Oligocene. This putting-back of the clock in regard to the geological age of frogs and toads upsets current ideas on the subject of batrachian evolution.

In a communication on the skulls of large Coal Measure labyrinthodonts preserved in the Museum at Newcastle (*Manchester Mem.* vol. lviii. No. 1), Mr. D. M. S. Watson records a morphological observation which, although somewhat technical, is of such importance as to deserve quotation in full :

"Examination of these primitive and extremely well-preserved skulls seems to show that the ordinary idea of the autostylism of the Tetrapoda is incorrect in postulating a connexion between the pterygo-quadrate cartilage and the otic region. It is, I think, quite certain that there never was such a connexion in primitive forms, except through the dermal bones of the temporal region. The lower attachment with the basisphenoid I have shown to exist in crossopterygians, which are hence ' amphistylic ' in a different way to *Notidanus.*"

In this connexion may be noticed a long paper by Dr. J. Versluys (*Zool. Jahrb.* 1912, suppl. xv. 2nd vol. pp. 545–719) on the problem of streptostylism and the mobility of the palate in extinct and living reptiles. The subject is, however, of such a complicated nature that it would be impossible to do justice to it in the space at my disposal.

Reverting to the Stegocephalia, it has to be added that Prof. von Huene has communicated to the *Anatomischer Anzeiger*, vol. xli. pp. 98–104, an article on the skull of the American genus *Eryops*, in which the relationships and homology of the constituent bones of the occipital and basicranial regions are clearly indicated.

In this place reference may be made conveniently to an article by Dr. Moodie in the serial already quoted (pp. 277–85) on the amphibian fauna of the Permian shales of Mazon Creek, Illinois. Ten species, referred to eight genera, are now known from this horizon; their systematic positions being indicated in a table of classification.

In regard to literature relating to fossil fishes the writer may take the opportunity of mentioning that authors do not send him copies of papers on this subject to nearly the same extent as they do those on higher vertebrates. Consequently his reviews on this section contain more omissions than is the case in other groups.

Such notice as I can give may commence with mention of an article by Dr. C. R. Eastman on Mesozoic and Cænozoic fishes published in the *Bulletin* of the Geological Society of America, vol. xxiii. pp. 228–32. After alluding to the general lines on which piscine evolution appears to have taken place during past epochs, the author raises the question whether the fish-fauna of the ocean abysses has been driven to its present haunts as a refuge against foes and competition. The question is answered in the affirmative, the author remarking that, according to palæontological evidence, this "refuge was not inhabited to any great extent by fishes prior to the latter part of the Cretaceous. But, beginning during this period and steadily proceeding until the present day, a gradual migration of certain groups of fishes into great depths of the ocean has been in progress, coincident with remarkably striking changes in the anatomical structure of the emigrant outcasts. As a result of recent researches, more especially of the late Cretaceous and Eocene deep-sea fish-faunas, we are enabled to note the gradually changing constitution of these abyssal assemblages from the close of the Mesozoic onward to our own day." The paper concludes with notices of recent work on fossil fishes.

A large series of remains of fishes from the Upper Tertiary and Secondary deposits of France form the subject of two papers by Mr. F. Priem, published in *Bull. Soc. Géol. France*, ser. 4, vol. xii. pp. 213–45 and 250–71 ; a few species being described as new. In a third article the same author (*op. cit.* pp. 246–9) describes and figures certain fish-otoliths from the French and English Eocene. A supplement to his account of the fishes of the Paris Basin was also published by Mr. Priem in 1911 (*Ann. Palæont.* vol. vi. pp. 1–44).

In the *Mem. Soc. ital. Sci.* ser. 3, vol. xvii. pp. 182–245, Messrs. Bassani and d'Erasmo discuss the Cretaceous fish-fauna of Capo d'Orlando, near Naples ; all the specimens being referred to previously known species. Another paper on the Italian fish-fauna, namely that of the Pliocene of Imolese, by Mr. G. de

Stefano, appeared in *Boll. Soc. Geol. ital.* vol. xxix. pp. 381–402, 1911.

Two memoirs on fossil fishes have been issued during the year by the Palæontographical Society (in the volume for 1911). In the first of these Dr. R. H. Traquair—whose recent death is a great loss to fossil ichthyology—continues his account of the British Carboniferous *Palæoniscidæ*, describing one species of *Canobius* as new. In the second Dr. Smith Woodward completes his account of the fishes of the English Chalk, dealing, apart from a supplement, with the well-known genus *Ptychodus*, of which he figures a remarkably fine series of associated teeth obtained by Mr. Willett near Brighton. The author concludes with the remark that the English Cretaceous fish-fauna is of a much more modern type than the contemporary reptilian and mammalian faunas, thereby indicating, at any rate in the case of the acanthopterygian teleosteans, a remarkably rapid process of evolution. The distribution of *Ptychodus* teeth in the English Chalk, as well as the teeth themselves, form the subject of a paper by G. E. Dibley in the *Quart. Journ. Geol. Soc.* vol. lxvii. pp. 263–77, 1911.

The following papers by Mr. M. Leriche published during 1911 may also be mentioned here: Note sur les Poissons stampiens du Bassin de Paris, *Ann. Soc. Géol. Nord*, vol. xxxi. pp. 324–36; Sur quelques Poissons du Cretacé du Bassin de Paris, *Bull. Soc. Géol. France*, ser. 4, vol. x. pp. 455–71 ; Note sur les Poissons Néogènes de la Catalogue, *ibid.* pp. 471–4; and Un Pycnodontoïde aberrant du Sénonien du Hainault—*Acrotemnus splendens*, de Kon., *Bull. Soc. Belge géol.* vol. xxv. *Proc. Verb.* pp. 162–8.

Brief notice must also suffice for two papers on Cretaceous fishes, of which the first, by Dr. G. d'Erasmo (*Riv. ital. Paleont.* vol. xviii. fasc. 2 and 3), deals with certain species from Monte Libano. In the second Dr. E. Henning (*Sitzber. Ges. natfor. Freunde*, 1912, pp. 483–93) discusses the rapid evolution of teleostean fishes in the short period between the Upper and Middle Cretaceous and the question whether this implies polyphyletic origin from several distinct groups of ganoids. The fish-faunas of a number of Cretaceous horizons are contrasted with one another.

Of more general interest is certain new evidence as to the community of type existing between the Tertiary faunas of

Western Africa and Eastern South America furnished in a paper by Dr. Eastman (*Ann. Carnegie Mus.* vol. viii. pp. 376–8) on remains of freshwater fishes from Guinea. The most important of these are referable to a species of double-armoured herring belonging to the Tertiary genus *Diplomystus* and closely allied to one from the Brazilian Tertiaries. " It is an interesting and significant fact," remarks the author, "that species of the same genus, or at least of very closely allied genera, should occur respectively in the freshwater deposits of the eastern coast of South America and western coast of Africa, the presumption being that the strata are approximately contemporaneous—that is to say, early Tertiary. This coincidence points to a similarity of the freshwater fish-faunas of the two continents extending as far back as the dawn of Tertiary time and also suggests a correspondence of geological history between the land-masses on either side of the Atlantic."

The author then proceeds to discuss the bearing of the discovery on the theory of a land-connexion, by means of " Helenis," between Africa and South America ; such hypothetical continent having been regarded as the original home of the *Lepidosirenidæ, Characinidæ, Cichlidæ,* and *Siluridæ.* As the genus *Diplomystus* also occurs in the Lower Tertiaries of Europe and Western Asia, its distribution is not very dissimilar to that of the Chelonian genus *Podocnemis (supra)*, which may have followed the same lines of migration, whatever these may have been

In a second communication (*op. cit.* pp. 182–7) Dr. Eastman describes the skeletons of two European Jurassic fishes within the ribs of each of which are contained the remains of a lizard. In one case the reptile, which had doubtless been swallowed as food, appears to be a species of the contemporary rhynchocephalian genus *Homœosaurus*, whilst in the second instance the prey may have belonged to the same or a nearly allied genus.

" The Soft Anatomy of Cretaceous Fishes " appears a somewhat strange title for a palæontological paper, but Dr. Moodie (*Kansas Sci. Bull.* ser. 2, vol. v. pp. 277–87, 1911) has obtained material which enables him to record certain details on this point. In the same article he also describes a new species of *Thrissopater* from the Cretaceous of Texas.

The affinities of *Saurorhamphus freyeri*, a fish first described by Heckel in 1849 from the Cretaceous bituminous schists of

Carso Triestino, are discussed by Dr. d'Erasmo in *Boll. Soc. Adriat. Sci. Nat.* vol. xxvi. pp. 45–88, in a manner chiefly interesting to systematists. The same remark applies in an even greater degree to a paper by Mr. L. Neumayer in the *Palæontographica* (vol. lix. pp. 251–88) on the comparative anatomy of the skull in Eocene and modern *Siluridæ*.

Two papers have been published during the year on the nature of those remarkable flat spiral structures, armed on the convex border with powerful teeth, described under the names of *Edestus*, *Helicoprion*, etc., which have long been a puzzle to ichthyologists, some of whom have regarded them as highly modified dorsal spines of sharks, whilst others consider that they pertain to the mouth. The first of these is an English translation of a paper by Mr. A. Karpinsky in *Bull. Ac. Sci. St. Pétersbourg*, 1911, pp. 1105–21, briefly mentioned in my review for that year. The main object of this paper, of which the translation is published in *Verh. K. Min. Ges. St. Pétersbourg*, vol. xlix. pp. 69–94, is to show that the view held by Dr. O. P. Hay and others that these organs are dorsal spines is untenable and that they are really appendages of the mouth. To this view Dr. Hay (*Proc. U.S. Nat. Mus.* vol. xlii. p. 31) is, however, himself a convert, as the result of the examination of a specimen discovered about eighteen years ago in the Coal Measures of Iowa. This specimen, which is double, comprises an upper and a lower element, both of which are bilaterally symmetrical and appear to have been produced in front of the mouth of the shark in such a manner that one worked against the other. Their shafts seem to have been developed by the consolidation and fusion of a median row of teeth, which gradually become worn away in the fore part of the series in the usual shark-fashion but the bases of which form the shaft.[1]

NOTE: GIANT TORTOISES AND THEIR DISTRIBUTION

IN my article on "Giant Tortoises and their Distribution," SCIENCE PROGRESS, October 1910, vol. v. pp. 302–17, reference

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[1] Since this article was set up, several other palæontological papers and memoirs have come to hand (notably a continuation of Prof. W. B. Scott's description of the Santa Cruz fauna), which it was found impossible to notice.

was made to a giant land tortoise then living in Ceylon, at Matara, near Galle. At the time of writing I had some doubt as to whether this specimen was distinct from the Colombo tortoise referred to in Dr. Günther's *Catalogue of Gigantic Tortoises in the British Museum* as having been living at Uplands, in Mutwal, near Colombo, in 1870. From a letter communicated to *Spolia Zeylanica* for December 1910 by Mr. Joseph Pearson, director of the Colombo Museum, I learn that the Colombo tortoise was found in Ceylon when the island was taken over by the British in 1796, and that it died in 1894, within a week of its removal from Uplands to Victoria Park, Colombo. It is now preserved in the museum at Colombo, and is referred by Mr. Pearson to *Testudo gigantea*. Its shell measures, in a straight line, 40 inches in length.

This being so, it is clear that the Matara tortoise, of which a photograph appeared in my article, represents a second giant tortoise imported into Ceylon; as, indeed, is indicated at the close of Mr. Pearson's letter. This tortoise I have referred to *T. gigantea*; and it may be that the measurement given in my article may refer to that specimen. I regret, however, that I cannot recall where I obtained this measurement, or the information as to a tortoise having been imported into Ceylon from the Seychelles in 1797 or 1798. Mr. Pearson states that he is endeavouring to obtain further information with regard to the Matara tortoise, of which the very existence might apparently have remained unknown to naturalists had it not been for the photograph by Mr. Stanley Mylius, published in *Country Life* of July 9, 1910.

<div style="text-align: right">R. LYDEKKER.</div>

TEMPERATURE AND THE PROPERTIES OF GASES

By FRANCIS HYNDMAN, B Sc.

IN every branch of human activity there are distinct periods which are marked either by some new discovery or by the termination of a definite line of work. The study of the thermodynamic properties of gases and the relation of the gaseous to the other states of matter has now reached the conclusion of such a period. It may be said that the modern study and theory of gases dates from the publication at Leiden in 1873, by J. D. van der Waals, of his famous treatise on the continuity of the liquid and gaseous states. Since that time a large army of workers have been occupied in striving to reduce the then unliquefied gases to the liquid and ultimately to the solid state, and in determining the various constants which define them. The honour of conquering the last of the known gases which remained unliquefied owing to the extremely low temperature required has fallen to Prof. H. Kamerlingh Onnes of Leiden, who has been for twenty-five years building up the most perfect and efficient cryogenic laboratory in the world.

This gas, helium, which was unknown fifteen years ago except spectroscopically in the sun, has now been found to occur in minute quantities in every radioactive portion of the earth's crust which has been tested, with one or two trifling exceptions. Its presence is closely connected with the radioactivity which nearly all substances possess, and it appears to be one of the decomposition products of radium and of similar substances. It occurs in the atmosphere but in very small quantity, and is obtained in practice by heating certain minerals, preferably monazite sand. It is hence a very remarkable substance, besides being the gas which is the most difficult to liquefy. As all the known gases have now been liquefied, this line of work must stand still until the chemists discover some other and possibly even more refractory gas.

It is interesting to note that Prof. van der Waals retired

from his chair at Amsterdam just at the time when this result was obtained, which so brilliantly confirmed his prediction that all substances which do not decompose could be brought under suitable conditions of pressure and temperature into the states of solid, liquid, vapour, or gas respectively.

With the latter states, and probably with solids also, the whole thermodynamic condition of a substance is known with the determination of two sets of data. One, the relation between the volume and the pressure at any possible temperature, is commonly spoken of as the determination of the equation of state for the substance. The second, the relation between the change of temperature of the substance and the amounts of heat required to produce that change of temperature under different conditions, is known as the determination of the specific heat. It may be said at once that with no substance is there a complete knowledge of the equation of state or of the variations of the specific heat covering even two out of the four states of matter mentioned above. On the other hand, small ranges are known for various substances with more or less accuracy, and these can be pieced together, by the aid of a principle which will be considered later, into equations of state which represent an ideal substance which occupies an average position among the variations of actual substances.

This subject has to be attacked from two different sides, one that of thermodynamics, which enunciates general propositions to which all substances in any state must agree, but which is sometimes only applied to actual substances with difficulty owing to the want of knowledge of some of the data which are requisite. On the other hand, an attempt can be made to build up a theory which will account satisfactorily for the behaviour of matter by considering its constitution and attempting to arrive, by as nearly strict mathematical paths as possible, at the probable behaviour of matter with the constitution which has been supposed. We will not consider the various constitutions which have been suggested, nor any one in detail, but shall merely outline the fundamental conceptions on which the one most commonly used—the kinetic theory—has been based. The conceptions on which this theory are grounded have enabled progress to be made in other branches of science also, and the assistance derived from these in return has helped to confirm the validity of the conceptions of the kinetic theory.

As we assume that any actual gas can be ultimately brought into the solid state through all the others, we may discuss the relations of quantities in the gaseous state as the least complicated, without any loss of generality. The kinetic theory assumes that all pure gases consist of a vast number of particles which are exactly similar in volume (s), shape, and mass (m) to one another, and that they are all striving to move in straight lines with velocities which are continually varying about a certain mean value (u). These particles or molecules are known to be exceedingly small; so, where the gas is in a comparatively rarefied condition and the size of the molecules is very small compared with the average distance between them, it is possible, as a first approximation, to neglect the size of the molecules and to treat them as if they were only mathematical points without any action on one another, and merely endowed with mass and velocity, that is, with kinetic energy. The molecules are continually striking against the walls of the containing vessel with blows the force of which depends upon their kinetic energy, and hence on their velocity. If we call the combined effect of these blows the pressure, and measure it as a distributed force applied to every square centimetre of the wall, it is easily found that

(1) . . . . $p = \frac{2}{3} . n . \frac{1}{2} m u^2 = \frac{1}{3} d u^2$

where (n) is the number of molecules per cubic centimetre, (u) has the value given above, and d is the density $= 1/v$, where v is the volume of the gas. Hence we have $pv = \frac{1}{3} u^2$.

Comparatively rough experiments with air or similar gases under moderately small pressures made by the early experimenters, or more accurate experiments made more recently at really small pressures, have shown that as a first approximation the relation

(2) . . . . $pv = Rt_0(1 + at) = RT$

holds for gases, where R and a are constants, and t is the temperature centigrade. The value of T is then clearly determined when the value of a is known.

This relation is known as the Boyle-GayLussac-Avogadro law, and is the most simple equation of state. By comparing the two values for pv, it will be seen that $\frac{1}{3}u^2 = RT$, and hence that the temperature and the mean velocity are very closely related. Also that, where $T = t_0(1 + at) = 0$, u would be zero, and

hence there would be no motion. If we could suppose this equation to hold until that condition were reached, the state of no motion, beyond which it would logically seem impossible to go, would be reached at a temperature $t = 1/a$.

From the comparatively rough measurements mentioned above, a was found to have a mean value of $\frac{1}{273}$, and hence, with the limitations stated above, 273° would be the temperature below zero centigrade at which all motion would cease and matter would be quiescent. This point has been called the absolute zero, and although the value given to it now is not exactly — 273° C., it is sufficiently near this for any difference to be considered in the light of a correction. It was also shown by Lord Kelvin that the value of the absolute zero could be obtained from a study of the cycle of a perfect engine, that the thermodynamic temperature which enters into this is very nearly $273 + t°$ C., and that its zero value is identical with the temperature at which motion would cease with a perfect gas. In consequence of the great importance of this work, it is common to call temperatures on the absolute scale temperatures Kelvin, so that zero° C. = 273° K.[1]

As indicated above, this ideal gas state is found to exist to a very near approximation when the density of real gases is very small, and it is assumed that it would apply strictly at exceedingly small densities near to zero density. The coefficient of expansion a, found under these conditions, will then be the inverse of the absolute temperature, and this is the principal means by which an estimate is arrived at of the real value of this temperature.

If the molecules of a gas have no attractions for one another, no work will be done on allowing the gas to expand into a vacuum. It was at first thought that air and other similar gases conformed with this, but the experiments of Joule and Kelvin showed that real gases were in general either heated or cooled when allowed to expand in this way, excepting under certain definite conditions of temperature and initial pressure which vary for each gas, and at which there is no change. A perfect gas would, under all conditions, be in the condition so that

$$(3) \quad \ldots \ldots \ldots \quad T\left(\frac{\delta v}{\delta T}\right)_p - v = \left(\frac{\delta T}{\delta p}\right)_E = 0$$

$$E = \text{Total Energy}$$

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[1] As will be explained later, the best value at present is 273·09.

would hold, whereas with real gases the states at which zero values for the Joule-Kelvin effect are found only occur under certain definite relations between pressure and temperature for each gas (see curve D, fig. 1, p. 38).

Some difficulty is found in the application of equation (3) to experimental results, as it is strictly only derived for infinitesimal changes of temperature, and the total energy (E) is supposed to remain constant. This makes its employment to reduce experiments, in which the changes of temperature and pressure are not very small, a somewhat difficult task, which is also increased by the difficulty of excluding other effects which tend to mask the one sought, and which sometimes allow a considerable fall in pressure to take place with no change of temperature.

A gas which at the same time obeys the equation of state (2) and which exhibits no Joule-Kelvin effect may strictly be called a perfect gas, but, as pointed out, a gas may obey one without necessarily obeying the other, at least over a certain range.

Experimental investigation at even moderate accuracies soon showed that gases obeyed these laws to a greater or less degree, and it was noted that the greatest deviations were found with gases such as carbon dioxide, sulphur dioxide, and ethylene, which are comparatively easily liquefied. With the class which Faraday called the " permanent gases " because he was unable to liquefy them, such as nitrogen, oxygen, and hydrogen, the deviations are much smaller. Still greater deviations are found with vapours of liquids such as water, etc., just above their boiling points. The extended kinetic theory as applied to real substances takes cognisance of both the size of the molecules and their attraction to one another, but has not been made to include as yet the internal energy of the molecule and the way in which this changes with temperature and pressure. It is clear that the molecules must have something of the nature of real extension, as shown by the increasing difficulty of compression, as certain limits are approached, and by such phenomena as effusion, and, on the other hand, a real molecular attraction as shown in such phenomena as capillarity. Also these characteristics are even more marked in the solid state. Molecules are known from observations of the density of gases to consist in most cases of two or more separate and distinct atoms, among which there must be a certain amount of internal energy

of motion which can be measured by observations on the specific heats. Without considering the historical development of knowledge in this direction, the modern position may be summed up as follows, leaving out of account considerations of electrons which can only make very small percentage changes in these relations.

(1) All chemically elementary substances, and many compounds, are capable of existing in the conditions of solid, liquid, vapour or gas under specific conditions of pressure and temperature.

(2) All pure gases consist of a very large number (n = about 10^{20} per cubic centimetre under normal conditions) of similar molecules.

(3) These molecules are moving in straight lines for distances depending on the density of the gas and known as the free path, the mean value being of the order of 10^{-4} mm. at ordinary temperature and pressure; they move with velocities which are changing at each collision, but continually varying about some mean value, the square of which is proportional to the absolute temperature. These velocities are of the order of 1 kilometre per second at the ordinary temperature.

(4) All molecules of any given pure gas consist of the same number of one or more atoms, these being the smallest particles of the substance which can exist without loss of identity alone or in combination. Each atom occupies a definite volume under definite conditions of temperature and pressure, and each molecule of more than one atom another volume which is not the sum of the atomic volumes. There is in each case a limiting volume which would only be reached at the lowest temperatures and highest pressures. Each molecule occupies an effective space which is some small multiple of its real volume and is usually denoted by (b).

(5) Complex molecules at any rate have some internal motion ; and possibly atoms also, though to a smaller extent.

(6) The molecules exert an attraction on one another which varies very little with the pressure, but which decreases as the temperature decreases. It is probable that the law of attraction varies with a much higher power than the square (that of gravitation and simple electric or magnetic attraction), some index of the order of 6 being indicated, and hence it is only effective when the molecules are very close together.

A very slight consideration of the above conditions which would have to be satisfied by an equation of state show that it must necessarily be very complex if it is to express them exactly. Suitable equations can be obtained as the result of careful experiment under known conditions and over a definite range for certain given substances, but such measurements are difficult and lengthy, and the values found are only strictly applicable to the conditions under which they are made.

These measurements, although of the utmost importance in special cases, would be of little assistance in the general question without a guiding principle. The utility of this can be best illustrated by an example. Consider some hydrogen and some carbon dioxide at the ordinary temperature and under the atmospheric pressure. For small changes of pressure and temperature, both will behave very similarly. Suppose, however, that they are strongly compressed. It will be found that at 15° C. the carbon dioxide will become a liquid under a pressure of 51 kilogrammes per sq. cm., whereas the hydrogen will become very dense, but will still remain a gas even under the enormous pressure of 5,000 kilogrammes per sq. cm. as found by actual experiment, and as we know now under *any* pressure which could be applied at this temperature. The former is called a vapour, the latter a gas at this temperature, and to bring hydrogen into the condition of a vapour it is necessary to go down to the temperature of about − 240° C.

It is found that there is some particular temperature for every gas, below which it must be cooled before it can be liquefied, and which is known as the critical temperature (T_c) while the necessary pressure to liquefy at this temperature is the critical pressure (p_c). The significance of this point will be further illustrated by a consideration of the result of heating a liquid and the vapour above it in a space where pressure can be applied. At any temperature there is a definite vapour pressure under these conditions which is independent of the volume of liquid and vapour until there is either all liquid or all vapour. As the boiling point is that at which the vapour pressure of the liquid is the same as the pressure above it, it follows that as the pressure on a liquid is reduced from the normal boiling point under atmospheric pressure the liquid will boil at continually lower temperatures until, in the natural course, the freezing point is reached, when it changes to the solid

state. Suppose, however, that the temperature is raised above the boiling point and the pressure increased enough to preserve some liquid. The vapour pressure will rise with the temperature until a point is reached at which the liquid meniscus vanishes suddenly with a very small increase of temperature and cannot be re-obtained by any increase of pressure. The liquid has passed to the gaseous state through the critical point.

The investigation of the exact behaviour of substances at this point and the means of determining the exact values of the constants are questions of great interest, but we are concerned for the moment with the values of these quantities only. Suppose we have these for some series of substances and we divide the pressure volume and temperature of these under any conditions by the critical values. The result is known as the " reduced " pressure (π), volume (ϕ), and temperature (θ), so that $\pi = p/pc$, etc. Thus far everything is the result of experiment, and we may turn to the guiding principle mentioned above. This was enunciated by J. D. van der Waals as the deduction from the theoretical equation of state deduced by him in 1873. This equation will be duly considered, but the great principle deduced from it and known as the " law of corresponding states " is of wider application. It may be said to generalise matter, to reduce everything to one substance under different conditions, as it states that " *All substances have the same properties at the same reduced pressure, volume, and temperature.*"

When one takes into consideration the great complexity of many molecules and the extraordinary range of properties exhibited, from helium with a melting point of less than 3° K. to such a substance as iodobenzene, which is one of those which have a high critical point which has been determined with some accuracy ($Tc = 721$ K.), it is remarkable that the coincidence should be as good as it is. However, even with substances which are chemically elementary and in which there is no association of vapour molecules on approaching the liquid state, there are many differences which appear to be connected with chemical properties, as substances of similar chemical characters fall into classes in which the divergences may be exceedingly small. In most cases the divergences are unexplained : probably there are not at present sufficient accurate data on which any more comprehensive generalisation could be based. The successful solution of this further prob-

lem awaits some one who is able to systematise the enormous mass of data which is being obtained. Something in the shape of a further generalisation has been obtained by the application of the thermodynamic reasoning of J. Willard Gibbs to the relations of the solid to the other states ; but this rather extends the former results of van der Waals to states which he did not consider, than increases the general accuracy with which the experimental data are systematised and new relations deduced.

One of the main difficulties in this subject is the great experimental difficulty which is encountered directly really accurate data at any other temperatures than the normal are required. Even at the normal temperature it is only by the very greatest care at every step that values are obtained, which are more accurate than to 0·02 per cent. The vast majority of measurements of compressibility at constant temperature, the determination of isothermals, are hardly accurate to 0·2 per cent., while very few critical data are accurate to 1 per cent.

It is very rarely that the same observer makes measurements on the three critical data, so that the results are often not very comparable, and in any case the values given are in units which are not always self-evident. It is unfortunate that a really strict system of units has not been generally recognised, as all three units of pressure, volume, and temperature are liable to some ambiguity. Pressure is usually expressed in atmospheres, the value of which depends upon the latitude of the experimental station at which the determinations are made, but which are sometimes mean atmospheres reduced to latitude 45°. If all observers deduced their results to the C.G.S. unit of a megadyne per sq. cm., which is very nearly an atmosphere, it would be much clearer. The same is true of the volumes which are sometimes given in the unit known as the normal volume, the volume of the quantity of gas under experiment at zero° C. and under the unit of pressure employed. Others express the volumes in terms of the mass of the gas, which is easily converted to the first mentioned, if the law of Avogadro is assumed to hold strictly. However, as will be explained later, this law is not strict, and if a correction is applied so that equal volumes of different gases shall contain equal numbers of molecules, a unit is obtained which is known as the "theoretical normal volume" and which makes results on different gases strictly comparable. There is less ambiguity about the

scale of temperature which is either centigrade or Kelvin, and between which there is a relation which is now almost exactly known. However, temperatures are sometimes given in the scales of a particular gas thermometer.

The difficulties experienced in the determination of the exact values of the critical constants are, as mentioned above, very great, and this from two causes. In the first place their value varies very much with the presence of only small traces of impurities, traces which would hardly affect any other physical constant; and in the second place the critical state is so evanescent and so exact with pure substances that it is absolutely necessary to have the meniscus under view during the whole time until it disappears with a minute rise of temperature while the pressure is kept constant, or still better is increased very slowly, so that no heating due to compression can take place. It is clear that these conditions are not easily attained in practice, and hence the differences between the results given by even the most careful workers can be understood.

However, the attainment of these data to a high degree of accuracy is only a matter of time, and a number are now known to a sufficient accuracy to make deductions drawn from their use right in principle if not in actual value.

In attacking a subject such as this with the desire of deducing some general laws, there are always two main lines of advance open, both of which can be usefully followed as each gives the possibility of arriving at some conclusion which would not have been deducible from the other. Thus the simple relation of equation (2) has been of immense value, and really embodies the results of the deductive and the empirical lines of argument in their simplest form. The next step on the deductive side was made by J. D. van der Waals in 1873, who from kinetic and thermodynamical reasoning obtained the well-known form:

$$(4) \ . \ . \ . \ . \ . \ \left(p + \frac{a}{v^2}\right)(v - b) = RT = (1 + a)(1 - b) T$$

in which a and b are functions of the attraction and of the volume occupied by the molecules respectively, and are supposed to be invariable with temperature and pressure. It is clear from the propositions formulated above that these assumptions are not correct, and many attempts have been made by Clausius, Batelli, Berthelot, Boltzmann, Reinganum, and

others to obtain a closer agreement with the experimental results either by the inclusion of an additional constant or better by making them functions of the temperature and perhaps of pressure. Probably the most satisfactory of these is that due to Reinganum

$$(5) \ldots \ldots \left(p + \frac{a^1}{v^2}\right) \frac{(v - v^1)^1}{v^1} = RT$$

where a^1 and b^1 are functions of both v and T. It is certainly very exact for comparatively small densities, gives a good agreement for higher densities, and is capable of easy manipulation.

The other main line of development is more empirical, although many points have to be considered before the best form is reached. It is clear that the corrections to (2) which are given by (4) or (5) could be covered by a convergent series in powers of the density in which the coefficients of the various terms were determined from experimental data. There is much to be said for expressing the product pv as a series of increasing powers of d or $\frac{1}{v}$. The series developed by H. K. Onnes principally from the experimental results of Amagat is

$$(6) \ldots \ldots pv = A + B/v + C/v^2 + D/v^4 + E/v^6 + \text{etc.}$$

in which p and v are most conveniently expressed in megadynes and theoretical normal volumes, at constant temperature. It is found that with the highest pressures used by Amagat (about 3,000 At) when the density is about 10^3 the F term is the last that is necessary.

For every substance it is clearly possible to obtain such a series with some accuracy, if the measurements cover a sufficiently wide range, thus enabling the relations between p and v to be known at certain given temperatures. To obtain the change with temperatures a number of isothermals at different temperatures are required, the change of coefficient between any two being sufficient to give the relation over that particular range.

However, it is the combination of these relations with the principle of corresponding states which makes their use particularly instructive. The equation (4) can be put into the reduced form in which the pressure volume and temperature are generalised and a and b vanish by noting that, as it is a cubic equation in v, it will have three roots, which must all

coincide at the critical point. Without going through the process it follows from this that

$$(7) \quad \ldots \ldots \ldots \left(\pi + \frac{3}{\phi}\right)(3\phi - 1) = 8\theta$$

is the reduced equation.

On the other hand equation (6) can only be put into the reduced form by making some general assumption with regard to the relations of the critical data. One which is very nearly true in a large number of cases, and may be found to be strictly true in some, is that $\frac{p_c v_c}{T_c} = $ a constant $= \lambda$ say, and then the equation will appear as

$$(8) \quad \ldots \ldots \quad \lambda\pi\phi = A' + B'/\phi\lambda + C'/\phi^2\lambda^2 + D'/\phi^3\lambda^4 + \text{etc.}$$

in which A' B' etc., are functions of the reduced temperature θ of the form

$$(9) \quad \ldots \ldots \quad B' = b_1\theta + b_2 + b_3/\theta + b_1/\theta^3 + b_5/\theta^5. \quad •$$

In using this equation it is not necessary to give a value to λ, if, as is very useful, the values of pv/T at given values of p, v and t are wanted, for we get $\lambda \pi\phi/\theta = A'' + B''/\phi\lambda + C''/\phi^2\lambda^2 + \text{etc.}$, and hence $pv/T = A'' + B''d\left(\dfrac{Tc}{pc}\right) + C''d^2\left(\dfrac{Tc}{pc}\right)^2 + \text{etc.};$ where B'' etc. $= b_1 + b_2/\theta + b_3/\theta^2 + b_1/\theta^4 + b_5/\theta^6$. The value of Tc/pc is much more accurately known than λ and is usually between 2 and 4 (see, however, Table III.). For general deductions a value of λ can be taken and the reduced form $\pi\phi$ obtained for some special values of ϕ and θ.

Either from (7) or (8) or any other reduced equation it is hence possible to calculate relations between π, ϕ and θ which apply, at any rate up to the practical limits of these, to a fair approximation for any given substance, when the values of the critical constants are inserted.

In fig. 1 the system of values obtained from equation (7) by plotting $\pi\phi/\theta$ against $1/\phi = \delta$ as rectangular co-ordinates is shown, but it must be clearly understood that the numerical values can only be taken as an approximation to the results of experiment, although the main principles are correct.

It will be noticed that there are two clearly defined limits, where $\delta = 3$ and at high temperatures. As far as the first is concerned, Amagat found at his highest pressures values of

δ of about 2 and the compressibility was very sensibly decreasing. On the other hand the lowest critical temperature known being that of helium at say 5° K., it follows that a value of θ = 1000 makes T = 5000° K., a value far above any at which iso-

FIG. 1.

thermal measurements are possible under present conditions. As the critical temperature of hydrogen is about six times as great, it follows that even with θ = 100 the practical limits are reached with this gas and hence with all others. There are

two special points, A where $1/\phi = 0$ and $\pi\phi/\theta = 8/3$, at which all the isotherms converge, and B which is the critical point.

This diagram of $\pi\phi/\theta$ is particularly interesting and convenient for showing the whole range in consequence of these limits. It is to be noticed that the change of $\pi\phi/\theta$ obtained in passing along an isotherm is a change of entropy with change of density $(- d\psi/dv)$, which is very important in many theoretical discussions.

By the usual process of finding minimum values it will be found that the minima of $\pi\phi/\theta$ are given by

$$(10) \ . \ . \ . \ . \ . \quad \phi^3(27 - 8\theta) - 18\phi + 3 = 0$$

for various values of θ, the limiting values to give real solutions being $\theta = \frac{27}{8}$ and $\frac{27}{32}$ where $\pi\phi/\theta = \frac{8}{3}$ and zero respectively (fig. 1, curve c). We shall see later also that values of $\pi\phi/\theta = pv/T\lambda$ are of considerable interest in the treatment of the variability of certain quantities such as the specific heats.

It has been mentioned above that one of the criteria of a perfect gas is that it shall not be heated or cooled in expanding through a small orifice under a small difference of pressure. Now it is found in practice that nearly all gases are cooled on expansion and that at the ordinary temperature only helium and hydrogen will be heated among the known gases. The effect with helium has not yet been observed directly, that with hydrogen being measured with some uncertainty by Joule and Kelvin in their famous experiments. All that one can justly deduce from their results with hydrogen is that the change was very small, but towards a heating rather than a cooling effect.

The general equation given by Lord Kelvin reduces when there is no heating to (3), and if this is applied to the reduced equation (7) the following relation is obtained

$$(11) \ . \ . \ . \ . \ . \ . \quad \phi^2(27 - 4\theta) - 18\phi + 3 = 0,$$

which is the same equation as (10) if θ has twice the value it has there.

The values obtained from this are shown in curve D, fig. 1. Hence at all values inside the curve there will be cooling and at all values outside heating. The maximum temperature according to equation (11) at which the inversion will take place will be $\theta = 6.75$, at which it will occur at zero density. Considering the case of hydrogen and assuming $Tc = 30$ K., $pc =$

15 At., and taking $\delta = 0.1$ and hence $\pi = 1.5$ atmospheres, then $\theta = 6.31$ which makes T = 189.3 K. From this to about $\theta = 2$ the inversion occurs at nearly the same values of $\pi\phi/\theta$, the pressures rising to $\pi = 8.95$, which with hydrogen = 134 At. This is not very different from the results found by Olszewski at Cracow using a method which is not strictly carried out on the principles on which the Kelvin equation is deduced. Also it is known from practical experience that hydrogen experiences a sensible cooling when expanded through a fine jet at pressures of about 100 atmospheres at the temperature of liquid air, which is about 83 K., as this has been used to effect the liquefaction of hydrogen in combination with the regenerative process as used by Linde originally for air.

This limiting value for helium, with a Tc = 5.1 and pc = 2.3 about, will be T = 32.2 K. with $\delta = 0.1$. This result is again to some extent substantiated by experiment, as the isothermal determinations of H. K. Onnes at Leiden showed that the minimum value pv/T would be at about 18' K. for very small densities, and, as has been pointed out above, the relations expressed by equations (10) and (11) make this temperature just half that of the inversion point for the same density.

By using a temperature of 15" K. obtained by means of liquid hydrogen boiling under reduced pressure, H. K. Onnes was able to liquefy helium with ease. As a contrast is the case of oxygen, in which Tc = 155° K. and pc = 50 At. at a density of 0.02, which would be equivalent to a pressure of about 1 atmosphere $\theta = 6.6$, whence T = 1023 K. = 750° C., while, where $\delta = 1.5$; $\pi = 5.9$; so that at a temperature of $-40°$ C., the pressure at which inversion would occur would be about 300 kg. and hence quite within measurable limits.

It should again be emphasised that the results obtained by the use of equation (7), or indeed any other theoretical equation, are not to be taken as numerically accurate, but only as indicating the probable course of the relation. If anything were wanted to make this clear, it would be a consideration of the limiting temperatures found by the use of the various equations of state and equation (8). Some of the more important are

Clausius $3.182 \sqrt{1 + \dfrac{\beta}{a}} \, tc$, Berthelot $4.24 \, tc$, Reinganum $5.36 \, tc$

in place of the $6.75 \, tc$ found with the v.d. Waals equation. On the other hand, the empirical equation (8) gives a value just

under 5 when the density is taken as vanishingly small, and in this case it is not necessary to make any assumptions about the value of λ, so it is probably not very far from the truth. It is hoped that a more detailed consideration of this relation will be published shortly elsewhere; but the subject is painfully lacking in data, those of Thompson and Joule made in 1854 being almost the only series available, although there are a few other measurements by Olszewski, as mentioned above, and others which are more or less capable of mathematical treatment over a small range.

It would be exceedingly important for the whole gas theory to have a series of accurate measurements on one or more gases for considerable ranges of temperature and determining not only the sign but the value of the Joule-Kelvin effect, as a function of initial temperature and of initial density.

One of the most important applications of the study of the isothermals of gases is in the corrections to be applied to the gas thermometer to give temperatures on the absolute scale. This involves two problems—the evaluation of the difference between the centigrade and Kelvin scales, which depends partly on strictly thermodynamic reasoning and partly on the deductions to be drawn from the properties of various gases. For ordinary thermometric purposes, however, it is more important to know the point-to-point differences between the scales of any given gas used for thermometric purposes and the absolute scale, that is, the correction which must be applied to the temperature as read by the thermometer to get the real temperature at any point of the scale.

Until helium became known and reasonably obtainable, standard thermometry may be said to have been confined to the use of two gases, as no one gas is practically available over the whole range of temperatures measurable by the gas thermometer.

For temperatures from 100° C. upwards to the highest point which the reservoir will stand, nitrogen is still the most suitable gas, as the corrections are comparatively small; it does not penetrate the walls of the reservoir like hydrogen, or still more helium, nor attack mercury like oxygen at high temperatures. There is every reason to suppose that argon will be a still more suitable gas when its thermodynamic properties are sufficiently well known. For temperatures below 100° C.

hydrogen has been up to quite recently the standard, as its very low critical point (30 K.) makes the corrections quite small until temperatures only obtained by liquid hydrogen are reached. Now that helium is available with a critical point of about 5·1 K. and a small very simple molecule, which makes divergences extremely small, there is no doubt that it is the most suitable gas for low temperatures, as the corrections are even small at the temperature of solid hydrogen, the lowest temperature obtainable without the aid of helium itself. Thus quite shortly we may expect standard gas thermometry to be confined to helium thermometers up to 100° C. and argon thermometers from about 0° C. upwards, there being a region of 150 to 200° over which the two scales can be compared. However, for practical purposes the hydrogen and nitrogen scales will continue to be used, and, if the absolute corrections are known, readings made with them are as accurate as if made with a standard thermometer with the same care.

The evaluation of the absolute scale is due to Lord Kelvin in 1847 from the theory of heat engines. Heat is taken in at a temperature T and given out at a temperature T^1, and the theory says that the amounts of heat are proportional to the absolute temperatures with a perfect reversible engine. As the most perfect working substance is a perfect gas and as certain actual gases approach very nearly to the standard of perfection, they are clearly the most suitable substances to determine the value of the difference between the Kelvin and centigrade scales.

It is rather remarkable that the original value of − 273·1 C., which was derived from gases whose properties were observed at considerable distances from the absolute zero, should be almost exactly the value which the most recent and careful determinations would indicate. From time to time lengthy papers have been published making estimations of the absolute zero derived from measurements on the Joule-Kelvin effect which are known not to be very accurate. It is not to be wondered at that there should have been a considerable discrepancy between the results obtained, but they at least all indicated that the value of the Kelvin zero on the centigrade scale would be more than −273 and less than −273·5. Much more accurate information is, however, obtained from a strict investigation of accurate isothermals, and it will only be necessary to consider the results furnished by, say, nitrogen, hydrogen, and helium with critical

points at about 127 K., 20 K., and 5 K. respectively, as they practically cover the range of exact measurements $\theta = 1$ to $\theta = 10$. For the first gas the values of Amagat are used, for the second those of Onnes and Braak, for the third those of Onnes, and in each case the empirical expression of actual results bv means of equation (6) will be used, as these coincide with the actual isotherms within the limits of experimental error. The hydrogen results are the most important on account of their accuracy and the wide range of temperature covered, so that both the Kelvin zero and the variations from the Kelvin scale can be obtained from the same set of measurements.

There are two types of standard thermometers used—those at constant volume and constant pressure; but as the latter is less simple, and in most cases the corrections are larger, the constant volume thermometer is used more frequently, excepting at high temperatures. With an initial pressure of 760 mm. or 1 atmosphere at zero C. it is known as the normal hydrogen, helium, or other gas thermometer as the case may be, and with an initial zero pressure of 1,000 mm. as the international thermometer. With these small densities all terms above the third in equation (6) become vanishingly small, and even the third has very small influence, so that obtaining the corrections at these pressures resolves itself into the problem of measuring the value of B as accurately as possible.

The value of the absolute zero is usually obtained by correcting the pressure coefficient at one of the standard temperatures mentioned above to a zero density by the aid of the second and third terms of equation (6), which gives the deviations from the perfect gas state of equation (2). This deduction depends on the assumption that at limitingly low pressures any gas will be in a state where its deviations from the Boyle-GayLussac-Avogadro law expressed by equation (2) may be neglected. With a constant volume thermometer the pressure coefficient is the change of pressure with a given known interval of temperature, which is usually taken to be zero° C. to 100° C., as these points are obtainable with very great accuracy, or rather the exact value of the boiling point of water (although usually not exactly 100° C.) is easily determinable at the time of the experiment. Hence the pressure coefficient

$$a_v = \frac{1}{p_0} \frac{p_t - p_0}{T - T_0}, \text{ and if we obtain this relation with equation (1)}$$

we find that $\dfrac{p-p_0}{T-T_0} = \dfrac{R}{v_0} = \dfrac{p_0}{T_0}$ and hence $a_v = \dfrac{1}{T_0}$. As a consequence, if the perfect gas state can be assumed at very small pressures and densities, the absolute value of zero° C. is given by the inverse of the coefficient of expansion from $0 \cdot 0°$ C. to some temperature which is not only $100°$ C. most suitably for the reason given above, but also because this is the standard interval of the centigrade scale.

In the following table are collected the values for a few gases used for thermometric purposes in which the value of B is known to a sufficiently high degree of accuracy to make the calculation of any real value.

The curvature of the isotherms is so small that the C term does not enter into the result except for the purpose of obtaining the theoretical normal volume.

If the critical data were known with sufficient accuracy, it would be possible to derive these results by substitution in the reduced form, but at present the errors are far too great to make this method of any real value.

TABLE I. ABSOLUTE ZERO

Data.	Helium.	Hydrogen.	Nitrogen.
10^3 B_{100}	$+0 \cdot 673$	$+0 \cdot 86316$	$+0 \cdot 44303$
10^3 B_0	$+0 \cdot 512$	$+0 \cdot 58001$	$-0 \cdot 37215$
10^6 C_0	$+0 \cdot 12$	$+0 \cdot 670$	$+2 \cdot 62170$
a_v	$0 \cdot 0036616$	$0 \cdot 0036629$	$0 \cdot 0036744$
At pressure p_0 . .	1000	1000	1000
a_v limit	$0 \cdot 0036617$	$0 \cdot 0036617$	$0 \cdot 0036618$
$\dfrac{1}{a_v t} = T_0$	$-273 \cdot 10$ C.	$-273 \cdot 10$	$-273 \cdot 09$

According to Berthelot, who has carefully reviewed the whole of the data available, the most probable value for absolute zero is $-273 \cdot 09°$ C., while the above results for hydrogen and helium, which were obtained subsequently, give $273 \cdot 1$. Thus it is probable that the uncertainty has now been reduced to a hundredth of a degree centigrade.

There is a much greater degree of uncertainty in the evaluation of the divergences of the gas scales from the absolute, if the values given by different workers are given an equal weight. However, it is most probable that the values calculated

from actual isotherms by Kamerlingh Onnes and Braak for hydrogen and helium are to be taken with much greater confidence than those obtained by wide extrapolation of experimental values or from theoretical considerations, using some equation of state. In each case the differences between real and absolute may be expressed by means of a series, if the observations are sufficiently numerous and accurate to allow the coefficients to be obtained. For hydrogen this is the case, and in a series of the form

$$(12) \quad . \quad . \quad . \quad A t = a\frac{t}{100} + b\left(\frac{t}{100}\right)^2 + c\left(\frac{t}{100}\right)^3 + d\left(\frac{t}{100}\right)^4$$

the coefficients have the following values in the range $+ 100°$ C. to $- 217\cdot4°$ C.

$$a = - 0\cdot0143307$$
$$b = + 0\cdot0066916$$
$$c = + 0\cdot0049175$$
$$d = + 0\cdot0027297.$$

Similar differences can be obtained for other gases by correspondingly careful measurements.

The following table gives values calculated from the above equation for hydrogen and from experimental isotherms for helium, where, however, the values have been interpolated in the experimental range. Those in square brackets are extrapolated.

TABLE II. CORRECTIONS TO ABSOLUTE SCALE, INTERNATIONAL THERMOMETER

Temperature read.	Helium.	Hydrogen.
100° C.	0·0	0·0
50	—	− 0·0047
0	0·0	0·0
− 150	—	+ 0·0082
− 100	− 0·004	+ 0·0187
− 150	+ 0·0014	+ 0·0337
− 200	+ 0·004	+ 0·0593
− 250	—	[+ 0·1076]

These values appear to be as accurate as it is possible to obtain them at the present time, except by a direct measurement of the values of B and C at the temperature concerned, which is naturally more likely to give a correct value.

It is not the purpose of this article, however, to derive the most accurate corrections or to discuss the relative merits of the various methods by which such corrections have been obtained, but to indicate the most approved modern lines along which such investigations proceed. One very striking fact is the extreme accuracy of such measurements, even at temperatures such as + 500° C. or − 250° C. Tenths of degrees are capable of exact determination, and at the latter temperatures even hundredths of degrees are determinable with certainty, with carefully prepared and calibrated instruments. This accuracy is really necessary at low temperatures, on account of the much higher proportion of the temperature which one-hundredth of a degree has at, say, 50 K. than at 300 K. It is clear that such an accuracy is only obtainable when every possible precaution is taken, and, in particular, when the temperature of the gas which is being measured is kept constant to about one-hundredth of a degree. For all isothermal work at low temperatures the reservoir of gas is immersed in a liquid which is caused to boil at the required temperature by adjusting the pressure on it. The vapour pressure of a pure liquid diminishes with the temperature according to the relation expressed by the border-curve between liquid and vapour, which can be deduced by corresponding states from one accurate series of measurements, or, better, by direct measurement in each case.

As, however, in practice it is impossible to keep gases quite pure, the liquefied gas will be more or less a mixture, and the temperature at which it boils under a given pressure will change as the more volatile component boils away. Such a condition is very well exhibited by the boiling of liquid air. Here the normal boiling points of oxygen, freshly condensed air, and nitrogen are respectively 90 K., 82 K., and 79 K., hence that of air is very nearly obtained by the sum of the proportions of liquid oxygen and nitrogen contained in it. When the air is boiled, the more volatile nitrogen boils away, so that the liquid becomes continually richer in oxygen and the temperature rises until a steady state is reached at which the mixture boils as a simple substance. Hence it is clearly not possible to keep a temperature constant by boiling liquid air at constant pressure, and this is true of all gases used for such work, although, where the amount of impurity is small, the total change of temperature may be small also. It is necessary to have an elaborate system

by which the gas is boiled under a pressure which can be kept constant when required or changed very slowly to coincide with the slow change in temperature, which is indicated by some delicate and sensitive thermoscope, while a thermometer is used to make the actual measurements of temperature when this has been constant for a sufficient time for a steady state to have been reached in the gas reservoir and adjacent parts. Although the gas thermometer is the invariable standard, subject to the corrections considered above, it is hardly ever used for the actual measurements, partly because a standard gas thermometer is a valuable instrument which might be damaged in the course of the experiments, and partly because the work of reading the pressure and volume and of keeping all the conditions suitable for obtaining the best results is so laborious and complicated that the temperatures are better obtained by means of resistance or thermoelectric thermometers which have been carefully calibrated in the neighbourhood of the experimental points by comparison with a standard gas thermometer. These electric methods have also the great advantage that the measurements can take place in another room in quiet.

What has been said about low applies equally to high temperatures, only here the gas reservoir is sometimes immersed in the vapour of a boiling liquid; but very few isothermal measurements have been made at high temperatures except at the comparatively low pressures of gas thermometery.

There is some reason for this, as there are only a few substances where high temperature measurements are likely to give any very important result. Of these, mercury is probably the most manageable, although other substances, such as zinc and cadmium, which also have monatomic vapours would be of great interest. There are at the present time many measurements on vapour pressures, but these give only very meagre information in comparison with that obtained when the volume is measured also. The normal boiling point is only a special vapour pressure which occurs at different reduced temperatures, as the pressure of 1 atmosphere is a varied fraction of the critical pressure. It is not without interest to consider the relation between the boiling point and the critical data of all the mono-, di-, and tri-atomic substances for which reasonably accurate data are available, as collected in Table III.

TABLE III. DATA OF CHANGE OF STATE

Substance.	Molecule	Density	Tc	T_B	T_Γ	pc	Tc/pc	Γ_B/Tc
Helium . . .	He	2	5·1	4·5	<3	2·3	2·22	0·88
Hydrogen . .	H_2	1	35	20	14	11·0	—	0·57
Nitrogen . .	N_2	14	127	77	63	35·0	3·63	0·61
Carbon monoxide .	CO	14	133	83	69	35·5	3·75	0·62
Argon . . .	A	20	153	87	85	51·0	2·95	0·57
Oxygen . . .	O_2	16	155	90	—	50·0	3·10	0·60
Nitric oxide . .	NO	15	179	130	120	71·0	2·52	0·73
Krypton. . .	Kr	41	210	160	121	74·0	3·89	0·76
Xenon . . .	Xe	64	288	164	123	57·0	4·53	0·57
Carbon dioxide .	CO_2	22	304	(195 sublimes 1 At)		77·0	5·34	0 64
Nitrous oxide. .	N_2O	22	312	193	171	77·5	4·02	0·61
Hydrogen chloride	HCl	17 8	325	—	—	83·0	3·92	—
Hydrogen bromide	HBr	40·5	364	—	—	—	—	—
Hydrogen sulphide	H_2S	17	373	190	187	90·0	4·15	0·56
Carbon oxysulphide	COS	30	378	—	-	65·0	5·82	—
Hydrogen selenide	H_2Se	40·5	411	232	209	91·0	4·51	0·57
Chlorine . .	Cl_2	35·5	419	239	171	93·5	4·51	0·57
Hydrogen iodide .	HI	64	424	236	223	[ ]	[ ]	0·56
Sulphur dioxide .	SO_2	32	429	263	200	79·0	5·44	0·61
Carbon disulphide .	CS_2	38	549	319	163	74 0	7·4	0·58
[Fluorbenzene .	C_6H_5F	48	560	358	—	447·0	1·25	0·63]
Bromine . .	Br_2	80	575	336	266	[132]	[4·3]	0·58
Water . . .	H_2O	9	638	393	273	200·0	3·20	0·62
Mercury . .	Hg	100	[1065]	630	234	[95]	[M·2]	[0·59]

The density is that in the vapour state, and is half the molecular weight. Tc, T_B, T_F are the absolute values of the critical, boiling and freezing point temperatures, and pc is the critical pressure in atmospheres. $Tc/pc = v_c/\lambda$ and is seen to increase with increasing density more than with temperature or molecular complexity. Indeed, with more complex molecules of the type of fluorbenzene, which is given as an example, as it is well studied and normal, this ratio appears to be little more than half the lowest value otherwise found in the table, and thus among simple substances. No doubt the meaning is a variation in the critical volume which cannot be satisfactorily investigated for want of sufficient reliable data. The last column is the reduced normal boiling point, and the mean of the values given is 0·62 or very nearly 2/3, which is a rough and useful approximation to this ratio. It may be noted that there is much less regularity in the relation of the freezing point to the others, as would be anticipated from the complex molecular conditions which appear at and near the solid state.

By making use of the principle that equally reduced vapour

pressures correspond to equally reduced temperatures, it is possible to arrive at the values of some of the gaps in the table. A value for the critical pressures of bromine found thus is $pc = 132$. To arrive at approximate values for mercury, it is necessary to make an independent estimate for either pc or Tc. Since the ratio T_B/Tc is also available, and taking this as 0·59, Tc appears as 1065, and then from the known vapour densities the critical pressure comes out at 95 atmospheres only. This is remarkably low, and makes the ratio Tc/pc very large, thus showing probably that v_c is large. However, as has been mentioned above, the data are still wanting to enable any generalisations to be made with elementary substances and simple compounds.

To have complete knowledge of the thermodynamic condition of a substance, it is necessary to know the quantity of heat which will be required to raise a known mass of it a known difference of temperature. In the case of solids and liquids, the mass is kept under constant conditions of pressure and the volume allowed to increase with increase of temperature, so that the applied heat does external work in producing this increase of volume, in addition to that which would be required to change its temperature at constant volume. If we call Cp the atomic heat at constant pressure and Cv that at constant volume, they will mean the number of calories required to raise the atomic weights of any substance one degree centigrade under these conditions. However, to make the definition quite exact, it is necessary to define the calorie used, as there is still unfortunately an ambiguity owing to the existence of several calories which differ by as much as 1 per cent. There is so much in favour of the mean calorie, the hundredth part of the heat require to raise one gramme of water from zero to 100° C., that it is becoming more generally accepted as the standard. We have, then, as p is constant

$$(13) \ . \ . \ . \ . \ . \ . \ Cp - Cv = p(v^1 - v) = \beta p v$$

where β is the coefficient of expansion at constant pressure.

With solids and liquids Cp and β can be measured, and Cv can be deduced from them, as it is exceedingly difficult to measure it direct.

With gases and vapours, however, there is no difficulty in keeping the volume constant, so that the two quantities Cp and

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Cv can be measured independently, or their difference and their ratio can be determined experimentally and their values be thus obtained.

The earlier experimenters, whose values were not very accurate and who mostly used the permanent gases for their measurements, concluded that Cp and Cv varied little with temperature, and that Cv at any rate did not vary with the volume. Such conclusions are quite in accord with deductions to be drawn from a consideration of ideal gases obeying equation (2), with which the difference of the specific heats will be a constant from equation (13).

However, experiment shows that both specific heats not only vary considerably with change of temperature, but with change of density also. There are not many substances on which experiments have been made in several states, but the general trend of change is indicated by what is known.

In the solid state the majority of the elements have atomic heats approximating to 6·5, even hydrogen being 5·88 as deduced from the results of the change in the specific heat of palladium by occluded hydrogen; in the gaseous state it is 3·4 at 0° C. If we assume that the molecular heat is strictly additive, as it appears to be in a large number of cases, we can compare the heats of simple compounds such as water, which is particularly interesting because it is the standard calorimetric substance. It will be convenient to give molecular heats to avoid any question about the atomic heats in the molecule, and to assume the simple molecule in all states for this purpose, although it is certainly more complex in many liquids and solids. We have not always both specific heats at the different temperatures of the vapour and gas, so must assume that the difference is equal to 2 gramme calories, which is very nearly the value for an ideal gas. Taking then the constant pressure value throughout, we find for ice at 0·0° C. 9·36 and decreasing with the temperature, for water at about 16° C., 18; for steam at 100, 6·35 + 2 = 8·35 ; for water vapour at 1,000, 11·52 + 2 = 13·52, assuming that the same law holds as at lower temperatures.

The changes here shown appear to be general. Starting from the minimum at absolute zero, the value grows until it reaches a maximum at some temperature coinciding with the liquid state at moderate pressures, then again decreases to a second minimum at a temperature corresponding with the

vapour state at moderate pressures, again increasing with the temperature very rapidly, and in some cases passing the first maximum at easily attainable temperatures. The molecular heats of gases at constant pressure appear to be given by a formula as follows:

$$(14) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad C_p = 6\cdot 5 + sT$$

where s is a coefficient which increases with the complexity of the gas.

In the above no mention has been made about density, as it is always assumed that the density was small. However, in some of the experiments which determined the best values we have, the density was certainly very high, and we may consider shortly the effect of density; but the measurements are very contradictory, and unfortunately the results which have been deduced as yet from theoretical grounds do not appear to be reconcilable with the best experimental evidence.

If an easily manageable equation of state were to hand, which were true over a large range, there should be no difficulty in deducing the changes of both C_p and C_v from the well-known relations

$$(15) \quad \begin{aligned} \frac{\delta C_v}{\delta v} &= T\frac{\delta^2 p}{\delta t^2} \\ \frac{\delta C_p}{\delta p} &= -T\frac{\delta^2 v}{\delta t^2}. \end{aligned}$$

Putting these equal to zero respectively should then give the temperatures at which the maximum and minimum values of C_v and C_p occur at various densities (pressures). However, with either (2) or (4) C_v appears as a constant, and with (8) it appears to only show maxima between very narrow limits of density. This subject is now under consideration with improved coefficients. It is known that C_v increases with increase of density with all gases, excepting hydrogen, which have been tried. In the case of hydrogen it decreases, and hence, as the reduced temperature of hydrogen at ordinary experimental temperatures is much higher than that attainable with the other permanent gases which were tried, one is naturally led to the supposition that the maximum value will occur for hydrogen at some lower temperature with moderate pressures.

The maximum value of C_v appears to increase with the density, so that at very high pressures it is possible that the

change with hydrogen would be the same as with other gases. If these conclusions are correct the results should be intensified in the case of helium, which has a much lower critical temperature and pressure.

It appears to be probable also that at temperatures below the critical $C_p - C_v$ may be negative, in which case $K = C_p/C_v$ would further be less than unity. If this should be substantiated by investigation it will throw some doubt upon the deductions which are customarily made about the connection between k and the total and external energy of the molecule. Certainly the main conclusions are justified, and the deduction that k would have its maximum value with monatomic molecules has been abundantly demonstrated, first with mercury vapour and subsequently with the gases of the argon group, where the experimental results all show values differing very little from $5/3$. In the liquid state the molecular heat of mercury is about 6·7 and in the solid 6·4, which would appear to indicate that even in the solid state it is monatomic, as this value coincides with the general value of the atomic heat of solid elements.

However, the elements with simple molecules in the gaseous state are still very little studied in the liquid and solid states, partly owing to the low temperatures at which they would have to be observed and partly because the importance of these measurements is not very generally recognised except among those who are fully occupied with these and similar questions. There are three separate lines of experimental research which are all very fruitful and which are at present only connected together in a very imperfect way theoretically owing to the want of sufficient data. The accurate study of isothermals, which is the absolutely necessary foundation for an advance in the theory of coincident condition, and the possibility of arriving at a generally applicable equation of state can receive most important assistance from the study of the Joule-Kelvin effect and the specific heats. It must, however, be emphasised that the preliminary and pioneer stages are past, and that unless measurements are exact they have really very little value or are actually harmful because they form the basis of false conclusions.

In isothermal work it is possible at about the ordinary temperature to arrive at an accuracy of about 0·02 per cent. mean error in the determinations. As lower temperatures are used,

not only does the proportional error become of more importance, but at the same time the difficulties become greater. It may hence be said that an accuracy of 0·1 per cent. is about the limit of usefulness in isothermal determinations even at very low temperatures. Such accuracies can now be attained at the temperature of boiling hydrogen and should be attainable even in boiling helium, so that the properties of helium as a gas and everything else as a solid can be investigated at very nearly the absolute zero, that is, at and about 5° K.

At any temperature where the system of isotherms is accurately known it should not be difficult to determine experimentally both $\delta Cv/\delta v$ and $\delta Cp/\delta p$ by enclosing the gas in a comparatively athermanous envelope and causing a small change of temperature by electrical means in the gas, keeping this at one time at constant volume and at another at constant pressure. The energy, and therefore heat, absorbed would be known, so that all the data would be present to calculate the above values by starting with volumes or pressures which were increased by a small proportion. The isothermals would only be required for correction to standard value and the results would be much more accurate than any deductions from the isotherms themselves, as these involve the second differential coefficients with the temperature (see 15). It would be necessary to have the thermometer in the gas, which might introduce some difficulty in the construction; or, if the isothermals were sufficiently accurately known, the temperature change could be deduced from the changes in p or v when the other variable was kept constant.

From what has been said it will be seen that the subject has reached a stage at which it is clear that much new light cannot be obtained without either many accurate data or some unlooked-for discovery. To obtain the former, lengthy experiments with complicated apparatus are necessary, but the results would well repay the labour, if such labour were possible. However, in spite of the growing importance of the subject from every point of view, it is strictly true that there is only one place in this country where such measurements are at all possible, although they form the only real foundation of a kinetic theory of matter and its connection with practical thermodynamics.

LENARD'S RESEARCHES ON PHOSPHORESCENCE

By E. N. DA C. ANDRADE, B Sc., Ph.D.

In the following pages a brief account is given of the chief phenomena of phosphorescence known in E. Becquerel's time together with a description of the more recent work of Lenard and his co-workers, whose labours have contributed largely to the solution of the problems underlying the emission of light by the atom or molecule.

When ordinary bodies are heated, they begin to emit visible light at a definite temperature, which is the same whatever the substance may be (about 500° C.); it is to such radiation, due to temperature alone and usually referred to as temperature radiation, that Kirchoff's law applies, though W. Wien (*Nobel-Vortrag*, 1911, p. 5) imagines that it may be possible to extend the law to other radiations by an extension of the conception of temperature ; he admits, however, that at present it is impossible to state how, for example, a phosphorescent body can fall into equilibrium with the radiation. In certain cases light may be emitted at a temperature far below that at which temperature radiation sets in ; such cases are classed together as luminescence phenomena ; these are variously grouped under the headings triboluminescence, lyoluminescence, crystallo-luminescence, chemical luminescence and phosphorescence, fluorescence and thermoluminescence. The first three names are given respectively to the emission of light which takes place on rubbing or breaking certain substances (a well-known case being that of sugar), to the emission of light observed when certain solid substances are dissolved, and to the emission of light attending the crystallisation of salts—for instance, sodium or potassium sulphate. It is probable that the two latter cases are only examples of triboluminescence, the light being attributable to the friction and breaking of the crystals which take place on dissolution and crystallisation : apparently

the bodies which exhibit the phenomena in question are all triboluminescent.[1] Chemical luminescence is the form of luminosity accompanying certain chemical actions, such as slow oxidations : the so-called " phosphorescence " of phosphorus and of putrefying organic matter are cases in point.

The term phosphorescence is properly applied to the power which many bodies possess of emitting light after excitation by radiations. This excitation can be effected not only by visible and invisible (ultra-violet) light but also by cathode and canal rays and by Röntgen rays; irradiation of some kind is necessary, however, in all cases of true phosphorescence.

In phosphorescence, the emission of light continues after the exciting radiations have ceased ; if the emission does not persist during a measurable time the phenomenon is termed fluorescence. In the case of solids there is no true fluorescence, although the term is often used in speaking of the phosphorescence of very short duration which is exhibited by many solids : in the case of gases and liquids the duration of the period of after-glow is inappreciable and we may speak of fluorescence. But there is little point in attempting to distinguish rigidly between the two terms, though it is possible that more refined measurement would show a very short after-glow even in the case of gases. Thermoluminescence, so-called by E. Wiedemann, who first observed it, is the property of selective light-emission which certain artificial substances exhibit on being heated to a temperature far below that which conditions temperature radiation ; it is necessary to excite the substance previously by certain radiations, which do not, however, cause the emission of light at ordinary temperatures. This is only a particular case of phosphorescence, the exciting energy being stored at the lower temperature and only liberated as the transformed radiation at the higher : all the phosphoroids—as we shall in future call phosphorescent solids—prepared by Lenard can be caused to show such a storage of energy. Hence we shall include the so-called fluorescence of solids and thermoluminescence under the general term phosphorescence.

Some of the first observations of true phosphorescence seem to have been made on gems : for instance, Boyle and afterwards Wolf observed the phenomenon in the case of

[1] See Kayser, *Handbuch der Spektroscopie*, p. 678, where a detailed account of the results of various experimenters will be found.

diamonds, which are really phosphorescent[1]; and subsequently Dufay showed that a fresh exposure to light would again render the stone and other phosphoroids phosphorescent after their power of emitting light had been destroyed by heating. The first artificial phosphoroid was prepared by Peter of Bologna, the " Bologna stone " (about 1602). This is barium sulphide containing traces of foreign metals, which, as we shall see later, are essential for the phosphorescence. With the aid of this phosphoroid Zanotti, using the solar spectrum, established the important fact that the colour of the emitted phosphorescent light is independent of the colour of the exciting light; Dufay, using coloured glasses, established the same fact for diamonds and, as already stated, recognised that, in the case of phosphorescence consequent on heating, a previous excitation was necessary. Later on Wilson showed that a great number of phosphorescent shells each emitted light of a fixed colour, whatever the colour of the exciting light. The next fundamental observation, that the red and infra-red rays extinguish a glowing phosphoroid— *i.e.* cause the parts on which they fall to lose their luminosity much faster than the other parts—was first made at the beginning of the nineteenth century by Ritter, though the first easily accessible reference is to be found in the poet Goethe's scientific works (*Farbenlehre*, § 678). This phenomenon was rediscovered by E. Becquerel, who noticed also that when infra-red light was first thrown on the phosphoroid a momentary increased luminosity was noticeable, which was followed by the rapid decay of intensity just mentioned, so that the parts of a phosphorescent sheet struck by infra-red radiations first become brighter than the other parts but soon afterwards become much darker. He observed that the effect of light was similar to that produced by directly heating the phosphoroid and used these properties in investigating the infra-red solar spectrum. He also made an extensive series of observations on the spectra of phosphorescent substances by throwing a spectrum on to plates

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[1] It was generally thought by the ancients and in mediæval times—Pliny, Solinus, Isidor of Seville—that the ruby and carbuncle shone in the dark; though no phosphorescence of any duration is obvious in the case of these stones, the ruby shows the phosphorescence of very short duration usually called fluorescence; in fact, the genuineness of the stone may be tested by exposing it to *blue* light, when the true ruby—which may, however, be synthetic—emits *red* light; a paste imitation only reflects the blue.

covered with the powdered phosphors and observing the luminosity produced in various parts of the spectrum; he found, as previous observers had done, that the nature of the emitted light was independent of the wave-length of the exciting light. Becquerel also made important observations on the temperature effects and the law of decay of the phosphorescent light with time which will be dealt with later on; by systematically using the spectroscope in this work he placed the study of the whole question on a new footing. But he did not put forward any general theory of phosphorescence.

At this time, the latter half of the nineteenth century, one of the chief obstacles in the way of the study of the subject was the difficulty of preparing artificial phosphoroids which would behave in a definite way: for instance, calcium sulphide could be prepared so that it would phosphoresce either yellow or green. A first step in the direction of a solution of this problem was made by Lecoq de Boisbaudran, who showed that certain substances, which did not phosphoresce in the cathode rays when pure, were rendered phosphorescent by the addition of traces of foreign metals; that, for instance, the luminosity of many substances was due to traces of manganese. About this time, Crookes, working on the rare earths, showed that their presence, for example in salts of calcium, gave rise to definite phosphorescence spectra under the influence of cathode rays; both he and Lecoq de Boisbaudran did much work on these phosphorescent spectra. Verneuil traced the phosphorescence of calcium sulphide to the presence of traces of bismuth. It is at this point that the researches of Lenard begin, to whose work I shall now devote special attention.

Lenard, in conjunction with Klatt, first stated in detail the conditions to be observed in preparing phosphoroids from the alkaline earths and systematically prepared a large number of substances of this class, which includes nearly all those which remain luminous during a considerable period after the exciting light has ceased; a form of luminosity which it is convenient to call the after-glow. Three components are necessary: the sulphide of an alkaline metal; a small quantity—generally less than a ten-thousandth of the whole—of a foreign metal; and a fusible component or flux. The action of the flux, which may be any one of a large number of colourless fusible salts, sodium sulphate for instance, is principally to bind the loose

mass together; it has also an influence on the intensity of the emitted light which will be further referred to.

The specific character of the phosphorescent light is dependent on the presence and nature of the traces of foreign metal. Lenard and Klatt were able to attribute the phosphorescence of calcium sulphide previously investigated by Lommel definitely to traces of particular metals. To each metal corresponds a series of emission bands, the phosphorescent light being always resolved by the spectroscope into bands having a maximum of intensity at a given wave-length fading off into darkness on both sides of this maximum. The bands are referred to by the wave-length at which they have their maximum intensity; and uncertainty as to the identity of a given band, which might arise in the discussion of the displacement of a band by influences to be mentioned later, is avoided by the definition of a band as a complex of emitted wave-lengths which possess common properties in respect of temperature, excitation by light of a particular wave-length, and rate of decay after the exciting light has been cut off. These tests also serve to separate superposed bands. The spectral position of the bands is peculiar to the given active metal, but their intensity and period of decay depend to some extent on the fusible component. A pure phosphoroid is defined as consisting of one alkaline sulphide together with traces of an active foreign metal and a flux. The pure sulphides do not phosphoresce, but an addition of 0·002 per cent. of bismuth will render barium sulphide strongly phosphorescent. The colour of the phosphorescent light varies markedly with the temperature of the phosphoroid, the shade obvious to the naked eye being made up of different bands which all vary in intensity independently of one another with temperature. In the case of each phosphoroid, there is a temperature above which it cannot be excited, but there seems to be no lower limit in this respect.

The investigation of phosphorescence has been greatly facilitated by Lenard's method of plotting the distribution of the exciting and excited light in the spectrum. As long as the phosphorescent glow was treated as a whole, the complexity of the observed phenomena baffled interpretation, but the behaviour of the individual bands is not so incomprehensible. To observe the distribution of excitation, in other words, the relation between the wave-length of the exciting light and the

intensity of the incited light, a spectrum is allowed to fall upon a screen covered with the given phosphoroid, the exciting light from a Nernst lamp or mercury vapour lamp being passed through a quartz prism in order to obtain the ultra-violet portion strong and well dispersed. The parts of the spectrum which are most effective in exciting the phosphorescent light were then at once observable. On examining the incited light through a prism, using the method of crossed spectra, it is resolved into its component bands, the relative intensity of the different parts of which can be estimated. Stokes's law, that the incited light is of longer wave-length than the exciting light, is always obeyed by phosphoroids. As a first result of this
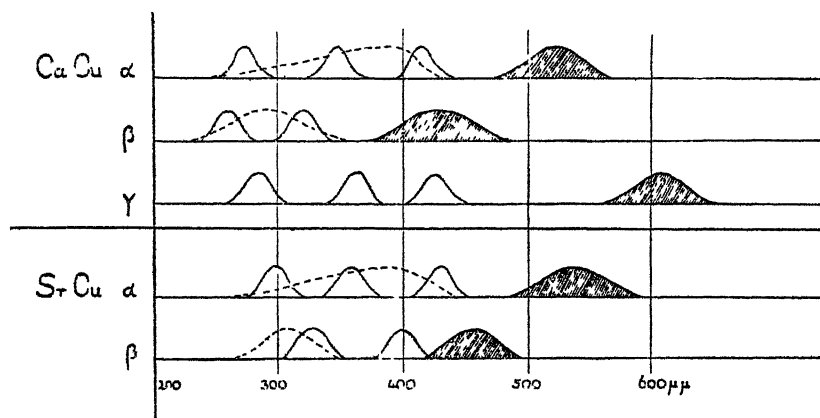
FIG. 1

method, it appeared that to each band of emitted light correspond definite ranges of wave-lengths which are capable of exciting it; these selective groups of wave-lengths will be referred to as the exciting spectrum. The composition of this spectrum depends only on the nature of the active metal and of the alkaline sulphide. Further, there are no bands common to different metals, either of excitation or emission. In fig. 1 the spectral distribution of the exciting and incited light is set out according to Lenard's method in the case of the two phosphoroids calcium sulphide containing copper as the active metal denoted by CaCu and strontium sulphide containing copper denoted by SrCu. The sharp unshaded curves indicate the distribution of the exciting light, the abscissæ representing the wave-length of the light, the ordinates the efficiency of each

wave-length in exciting the particular band of phosphorescent light in question—that is to say, the intensity of the incited light. The distribution of the intensity of the incited light according to wave-length is represented by the shaded curves. The first phosphoroid gives three bands of emitted light; these are represented separately, as there is a different exciting spectrum corresponding to each band; the three spectra are denoted by a, β, γ. The second has two bands, a and β, represented in the same manner. It will be observed that the bands are best excited by very narrow groups of wave-lengths and that in general more than one exciting band—usually three—correspond to each band of emitted light. The dotted curve gives the distribution of exciting light corresponding to the momentary process, to be referred to subsequently. The intensities of the different bands in the diagram are not drawn to scale, but they are all represented as having the same maximum intensity; this is done because, though all the bands have perfectly definite spectral positions, their relative intensities vary with the fusible component, the temperature and the manner in which the phosphoroid is prepared. Hence such a diagram can only give the general course, the position of the maximum intensity, and the range of each band.

The behaviour of a band with regard to temperature is such that it is possible to discriminate between three different states of the phosphoroid. In the coldest state, which Lenard calls the lower momentary state, each particular band rapidly reaches its maximum intensity when incited, and on the cessation of the exciting light as rapidly decays—it being a general rule that a band which is easily incited dies out quickly, and that one which is slowly incited dies out slowly. The light emitted at this stage is often very feeble, sometimes not noticeable ; as the temperature of the phosphoroid is raised, the second or "resting" state is reached, in which light energy is both emitted and at the same time stored up ; when the exciting illumination is cut off, the stored-up energy is liberated as the after-glow, the intensity of the bands gradually diminishing with time. On raising the temperature still further the third temperature state, the upper momentary state, is reached, in which, as in the lower state, there is no after-glow, but a rapid excitation followed by a rapid emission of light. It is necessary, however, to distinguish clearly between the upper and the lower momentary

states. In the lower state, besides the rapid emission, which is usually feeble, there is always an invisible storage of light-energy proceeding simultaneously, the which energy is liberated as a strong after-glow when the temperature of the phosphoroid is raised to that of the permanent state without subjecting it to further excitation. The energy thus stored in the lower state, which does not give rise to any luminosity so long as the temperature is below that of the permanent state, can be preserved during an extraordinarily long time, extending into months. The bands which appear at a given temperature are those which are permanent bands at that temperature. All luminosities which were observed by early experimenters to appear in phosphoroids on heating were due to energy having been stored in this way in the cold state of the given substance: after they had once been made luminous by warming, a fresh excitation was necessary before luminosity could be again so produced. Thus heat cannot act as an exciter of phosphorescence, but only as a liberator of light-energy already supplied and stored during the lower momentary state. During the upper momentary state, there is, however, no storage of energy. The two momentary states constitute what is sometimes referred to as fluorescence, but, as already stated, it is proposed to restrict this term to gases and liquids in which the duration of the after-glow is at least so short that it has never been measured.

Besides the two momentary and the permanent state, there is a fourth process of lesser importance, on which not much work has been done, to which only passing reference can be made. Lenard found that the shorter ultra-violet rays can excite a luminosity of medium duration falling between that of the momentary and the permanent state; it is most intense in the extreme ultra-violet, and gradually grows fainter with increasing wave-length, becoming unnoticeable in the visible violet. This form of incitation he called the ultra-violet process; it is of account only if the exciting light be of very short wave-length. It has not the definite excitation distribution of the other processes, but seems to be more nearly allied to the permanent than to the momentary states.

In Lenard's researches in conjunction with Pauli and Kammerlingh Onnes at low temperatures, and subsequent work with improved apparatus, the fact has been clearly established that there are different exciting spectra corresponding to the momen-

tary and permanent bands. These regions of exciting wave-length for the two phases largely overlap, so that in general a given wave-length may induce both processes simultaneously; but some of the shorter wave-lengths of the exciting light induce only the momentary, some of the longer only the permanent process. In fig. 1 the exciting spectrum corresponding to the momentary process is indicated by the broken line. Thus in the permanent state the energy is at the same time in part stored and in part used for the immediate emission of transformed radiation; but, while that of some wave-lengths is used for both processes, certain small spectral regions are only available for the one process, certain other regions only for the other. By going to a low enough temperature, the three states have been observed in all phosphoroids. Each band stores its own energy, as can be found by observing the exciting spectrum in the lower state.

As regards the exciting spectrum, the character of this for the momentary is somewhat different from that for the permanent state, as can be seen in the figure. The distribution, in the case of the latter, consists of well-defined bands, there being in general more than one exciting band corresponding to each emission band. The most frequent case is that of three sharp, nearly equal maxima of exciting intensity separated by regions in which the light produces no permanent glow. The distribution in the case of the momentary bands is not nearly so sharp; there is only one band of exciting light corresponding to each emission band, and this is ill-defined and lies largely in the ultra-violet: the position of the less refrangible edge of the band is characteristic of that band, however.

The theory which Lenard has developed to explain the properties of the bands just described—for the bands are the fundamental things—attributes the phenomena to a photo-electric action[1] of the light, which liberates electrons from the metallic atoms in the "centres" from which the emission of light proceeds present in all phosphorescent substances. These centres are complex molecules having as essential components an atom of the active metal, together with the alkali metal and sulphur, and they are distributed singly and separately through-out the mass of inactive material which forms the bulk of the

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[1] The liberation of negative electricity—electrons—which takes place when light of short wave-length falls upon metals and many other substances is called the photo-electric effect.

phosphoroid. They must be fibrous in structure in different directions, as the phosphorescence is destroyed by crushing the phosphoroid. To each emission band must correspond one kind of centre, the various kinds functioning independently of one another ; as a pure phosphoroid usually shows more than one band, the same active metal and alkaline sulphide must be capable of forming different kinds of centre. Again, a single band in a pure phosphoroid has often three definite corresponding bands in the exciting spectrum of the permanent phase (*i.e.* three wave-lengths particularly capable of exciting it), so that there must be secondary differences among the centres which emit one band, enabling them to resonate to different exciting wave-lengths. Furthermore, each centre must be capable of three periods of oscillation, namely those corresponding to the emission, the excitation, and the extinction by the action of infra-red light to which reference has been made in the introduction, of which details are given later. The centres which Lenard hypothecates to satisfy these conditions are of two kinds, the " momentary " and the " permanent " centres. The permanent centres are systems consisting of atoms of the active metal, the alkali metal and sulphur (say $Ca_x Cu_y S_z$, x, y, z being whole numbers) so arranged that both the metals are held by the valency bands of the sulphur atom, the difference between the various emission bands which are given by a pure phosphoroid being conditioned by the number of valencies of the active metallic atom by which the connection with the sulphur atom is effected. In support of this view we have the fact that the number of bands is never greater than the number of valencies of the active metal, and that the different bands have widely different intensities, corresponding to a greater facility of formation of certain bondages such as is to be expected. The different excitation bands may correspond to different space arrangements of the metallic atom with respect to the sulphur atom.

The permanent process is most marked in phosphoroids containing sulphur,[1] and hence the assumption is made that

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[1] Hirsch (Heidelberg Dissertation, 1912) has recently prepared phosphoroids of moderate duration which do not contain sulphur, an oxide or carbonate of the alkali metal being substituted for the sulphide ; these have not been much studied, but show that sulphur is not absolutely necessary for the production of permanent bands. Phosphoroids without sulphur had, of course, been previously prepared, notably by Crookes, Lecoq de Boisbaudran, and Goldstein.

in these phosphoroids the sulphur atom is responsible for the storage of the light energy; correspondingly the momentary centres would seem to be free from sulphur. In these, oxygen may very well take the place of sulphur, as oxides containing traces of active metal were long ago shown by Lecoq de Boisbaudran and Crookes to give a phosphorescence of short duration.

On Lenard's theory the light is emitted by the atom of active metal on the return of an electron previously photo-electrically liberated by the exciting light. In the unexcited state, the atom possesses its normal complement of electrons; in the excited state all the electrons which can be liberated from the atom by the action of light or cathode rays escape from it to other parts of the centre; whilst the intermediate condition, in which the electrons return to the atom, is the occasion of the light-emissions. In the excited state, the escaping electrons are probably stored in the sulphur atom in the case of the sulphide phosphoroids.

The broad bands of which the emitted light is made up are formed by the superposition of spectral lines of varying position, as it may be supposed that the period of the emitted light will vary within limits, both from centre to centre and from time to time in the same centre, in consequence of the variation in the immediate surroundings of the different centres in amorphous substances and the molecular agitation. In support of this view, it has been observed that on decreasing the molecular movements by lowering the temperature of the phosphoroid, the bands become much narrower. By cooling with liquid and solid hydrogen—to about 14° absolute—Lenard and his collaborators have succeeded in getting the bands very sharp: they still remained bands, however, whose intensity would not support a strong dispersion. A line spectrum could, perhaps, hardly be expected even at these low temperatures in amorphous substances, owing to the above-mentioned local variations in the arrangement of the molecules surrounding the centres; there seems more likelihood of such an emission spectrum in crystalline substances. The influence of the immediate sur-roundings of the centres on the period of the light emitted by them has been beautifully demonstrated in Lenard's experi-ments on the spectral position of a given emission band in phosphoroids made with sulphides, of the different alkali metals.

For if similar phosphoroids be prepared with the same active metal, but with different sulphides as bases, we get, passing from one to the other, a series of bands which are in every way analogous to one another, but having maxima which are displaced relatively in such a manner that the wave-lengths of the band maximum, divided by the square-root of the specific inductive capacity of the phosphoroid, gives a number which is roughly constant in all the phosphoroids. But this is what theory says would be the case for a Hertzian electro-magnetic oscillator vibrating in media of different inductive capacities, so that it is to be inferred that the electron which causes the emission of the light vibrates in and has its period controlled by the nature of the immediate surroundings of the atom to which it belongs. This leads to the assumption that the forces which bind the photo-electric electron to its atom extend out so far into the surroundings of the atom that the mean composition of these controls its period; or it may be supposed that the electron moves on the surface of the atom and, in the oscillations which it performs on its return, swings outside the atom while stimulating the emission of light from it—that is to say, from other electrons contained in it. This picture is supported by the results of other experiments on the photo-electric effect.

Very strong confirmation of this view, which attributes the phosphorescence to the photo-electric action of the light on the atoms of active metal in certain " centres " within the phosphoroid, has been obtained in direct experiment on the photo-electric effect in phosphoroid, performed by Lenard in collaboration with Saeland. As phosphoroids are good insulators, as the centres lose negative electricity under the action of light, they acquire a positive charge; finally, they are raised to such a positive potential that the negative electricity can no longer escape. On calculating from the capacity of the phosphorescent sheet and the known initial velocity of the photo-electrically liberated electrons, the charge required to raise the phosphoroid to the necessary potential, it is found that, in order that the positive charge actually acquired may be sufficient to stop the escape of electrons, only a fraction of the surface can be charged by it : this is strongly in favour of the theory that there are certain centres which alone take part both in the phosphorescent and photo-electric action of the phosphoroid. Further, it has been shown by experiment that the two effects

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are excited by light of the same wave-lengths and that the wave-lengths which are inactive in respect of the one are inactive in respect of the other phenomenon; again, separate components of the phosphoroid which show no phosphorescence also show no photo-electric effect. From the close connection of the two effects, the theory that the photo-electrically liberated electron causes the emission of phosphorescent light seems well established.

J. Becquerel has carried out some very interesting experiments, partly in collaboration with H. Becquerel and Kammerlingh Onnes, on the phosphorescence of uranyl salts. The bands of the spectrum of the emitted light became very narrow at low temperature, but a magnetic field did not appear to influence the emitted light; Lenard had likewise looked for a magnetic effect in the phosphoroids of the alkaline earths and failed to find it. A noteworthy point is that in the uranyl salts no traces of foreign metal condition the phosphorescence, which must be attributed to the uranium itself. Experiment indicates that the "centres" of light emission are present only in relatively very small numbers, as in the phosphoroids hitherto discussed, only a very few of the uranium atoms being active at a time. The experimenters suggest a possible connection between the light-emission and the radioactivity of the uranium atom, the atoms being assumed to be active only while they are breaking down. The fact that the intensity of the emitted light does not decrease when the temperature is lowered even to $14°$ absolute offers some support to this theory, which is, however, not very strongly upheld.

We now pass on to the extinction of phosphorescence by means of red and infra-red light of which mention has already been made. The effect, although most marked with these rays, is not confined to the infra-red region of the spectrum, as Fommel found a short wave region ($384-96 \mu\mu$) which could also extinguish phosphorescence. Further work by Dahms has shown that light of certain wave-lengths which can extinguish the emission of a phosphoroid already excited can also excite an unexcited phosphoroid, which shows that there is no essential difference between rays which excite and those which extinguish; if light of a given wave-length and intensity falls on a phosphoroid, an equilibrium is finally set up. Thus a piece of spar excited by the ultra-violet showed extinction to the edge of the ultra-violet, but, if previously unexcited, was excited by the

whole spectrum up to the infra-red. The experiments of Dahms referred to the whole of the emitted light, as at the time of his work little was known of the separate bands of which this light is made up.

Lenard, studying the effect of infra-red illumination in extinguishing the bands, found that it was in all respects similar to that produced by heat. As already observed by Becquerel, when the phosphoroid is exposed to the extinguishing light, it first of all lights up brilliantly during a short time and then rapidly loses in intensity, the light becoming extinct. The effect of both infra-red light and heating is thus to accelerate the emission of the stored energy and consequently the phosphoroid becomes non-luminous more rapidly. Recent measurements by Lenard have shown that the light-total—the time sum of the light energy emitted as the after-glow of a given band—is the same whatever the rate at which the light is emitted, whether normally or accelerated by heating or irradiation by the red rays. Another example in which the irradiation by the " extinguishing " rays has the same effect as heating the whole phosphoroid is supplied by the effect called by Lenard the " actinodielectric effect." It is found, namely, that if a phosphoroid be subjected to the infra-red rays, its conductivity is temporarily improved, an effect which is also produced by heating the phosphoroid.

After quenching by heat, infra-red radiation can produce no further momentary illumination, and *vice versa*. The effect of rise of temperature is to bring out each permanent band as the temperature of the permanent state for that band is reached: the bands then emit very rapidly and die out: if the initial temperature be above that of the permanent state, neither heating nor infra-red produce any effect. The thermometric temperature of the phosphoroid is not appreciably raised by infra-red radiation, but we may assume that the local molecular temperature [1] of the centres rises and that this produces the same effect on the light-emission as heating the whole phosphoroid. The conception of a raised *local* temperature is quite reasonable if we consider the excited centres as resonating to

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[1] It is doubtful if it be altogether advisable to refer to a local agitation of this kind as temperature ; as the vibrations are forced, there is a regularity about them which is essentially lacking in true temperature agitations. However, in this particular case it is hoped that confusion is avoided.

the infra-red rays so that they acquire a considerable local kinetic energy. The effect of the local agitation is probably to bring the sulphur atom which stores the electrons emitted from the active metal atom intermittently nearer to the metallic atom, so that the latter " by action at small distances " regains its electrons and so emits its light sooner than it would otherwise have done. The temperature insulation of the centres must be very good, as on cutting off the infra-red radiation its effect continues, just as if the centres remained at their high temperature for some time. If, however, the phosphoroid be first subjected to infra-red radiation and then excited, the preliminary irradiation has no effect on the light-emission, which shows that the period of the excited and unexcited centres is different, the latter not resonating to the infra-red rays. This is as might be expected, as the centres are in a different electrical state in the two cases. An interesting fact is recorded by Pauli, who investigated the ultra-violet and infra-red light emitted by phosphoroids—namely, that no phosphoroid which exhibits a marked and prolonged after-glow ever gives infra-red bands; such bands, if present, presumably accelerate the extinction of the visible bands of the phosphoroid.

The resonating system is probably the oppositely charged or polarised couplet formed by the sulphur atom and the active metallic atom; and, the extinction spectrum, which gives the efficacy of the different wave-lengths in accelerating the emission of light, will give by its maximum the free period of the polarised couplet. This accords with the theory of dispersion, which shows that the slowest free periods of the molecules correspond not to vibrating electrons, but to whole atoms or groups of atoms in the molecule, which must be electrically charged or polarised as has been imagined. The experimentally found extinguishing spectrum shows that the extinguishing power has a sharp boundary as we go further into the infra-red. Each active metal seems to have the same distribution in this respect, whatever the alkaline metal of the sulphide, although the distribution of excitation of the different bands is different. This accords well with the hypothesis.

It has been already mentioned that Lenard has shown that the total amount of light energy emitted by a given phosphoroid is the same whether the emission be accelerated by heating so as to last only a few seconds or whether it takes place normally.

In the same paper he also describes experiments demonstrating that the light total has a limit to which it tends with increasing intensity and duration of excitation; this limit is independent of the nature of the excitation.[1] When this limit is reached the phosphoroid is said to be fully excited by two different wavelengths separately, and if a band have two different light totals corresponding to these excitations, these light totals are not added together when the phosphoroid is excited by both wavelengths at once; the emission in this case is of the same intensity as that excited by one alone; this shows that there can only be one kind of centre capable of emitting the particular band which can resonate to both exciting periods. Whilst this is true of the permanent bands, the momentary bands, as Hausser has shown, have no limit of emission intensity; in this case the intensity increases steadily with the intensity of excitation, and an addition of the two emissions excited by different wavelengths is effected when these are used simultaneously.

In comparing the light total caused by excitation by cathode rays and excitation by light, a difficulty arises owing to the fact that the cathode rays cannot penetrate and so excite as thick a layer of the phosphoroid as the light rays: the emitted light increases with the thickness of the phosphorescent sheet used until this is about 1 mm. in the case of excitation by light; but with the thinnest sheets which can be prepared the cathode rays already excite their maximum of emitted energy. However, the depth of penetration of the cathode rays can be calculated from their known coefficient of absorption: from such calculations Lenard arrives at the conclusion that the total of emitted light is the same whether the exciting agent be light or cathode rays.

The laws of the decay of intensity of the emitted light were first considered by Becquerel, who, however, investigated the whole of the light emitted from impure phosphoroids and not the separate bands. Since the different bands due to one metal die out at different rates, it is not astonishing that the empirical formula which he proposed represented observation only very roughly. Subsequently Nicholls and Merrit and also Werner put forward as the law of decay of the permanent process the formula $I = \dfrac{a}{(c + at)^{2}}$, in which 1 is the intensity of the light,

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[1] Which may be light or cathode rays.

t the time, and a and c are constants ; a formula of this kind had already been used by Becquerel. This seemed to give a fair representation within the observed limits, the time of observation being about thirty minutes. Recently Lenard and Hausser have attacked the problem in great detail and have shown that, inasmuch as according to the conditions of excitation the decay can take place in different ways, so that under certain conditions curves of decay can be obtained for the same band which cut one another, no law can be given without considerable further discussion of the circumstances preceding the after-glow. This is due to a non-homogeneity of the centres, to be mentioned again shortly. They investigate the behaviour of the separate bands. Their experiments on the effect of the amount of active metal present in a pure phosphoroid show that the total of emitted light per unit volume of the phosphoroid—the reduction to unit volume follows from experiments made on phosphorescent sheets of different thickness—rises first of all proportionally with the increase of metal in the phosphoroid, but then turns and becomes constant, provided that the phosphoroid be fully excited in all cases. The law of decay, and therefore to some extent the light total, depends upon the amount of excitation, if this be insufficient to excite the phosphoroid fully : the first falling off in intensity is relatively greater for a brief excitation. These and other observations lead to the assumption of the simultaneous presence of permanent centres of different duration : those of small duration will be quickly excited and will quickly decay, whilst the more durable will have a slow excitation corresponding to their slow falling off. This assumption accounts for the observed influence of the duration of the excitation on the law of decay, as in the case of brief excitation a relatively much larger number of quickly decaying centres are excited than by a longer excitation. As regards the amount of metal present, if this be small, only more permanent centres are formed in the phosphoroid ; as it is increased, the number of such centres increases until a stage is reached when all that are possible are formed, and then the less persistent "permanent" centres are produced. After this, the addition of active metal does not increase the number of permanent centres, as experiment shows. The metal then goes to form "momentary" centres, the intensity of the momentary process being exceedingly small for small metal content. Hirsh has

shown that for a large number of bands the intensity of the momentary process continually increases with the amount of active metal, whilst, as stated, the number of permanent centres soon reaches a limit. He has also shown that a higher temperature is needed to prepare phosphoroids of pronounced after-glow, which falls in with the hypothesis, as other considerations show that the centres of long duration must be very large atomic complexes which would take some time to form, the production of which would accordingly be much facilitated by the increased diffusion consequent on a higher temperature in the preparation. Short heating at comparatively low temperature will give rise to a phosphoroid which shows a good momentary process and only a very faint permanent process.

Some account has now been given of the work carried out on the phosphorescence of pure phosphoroids of known composition which seem to offer by far the best opportunity of obtaining a true—or perhaps one should say useful [1]—insight into the mechanism of phosphorescence. The information so obtained is particularly helpful in the study of the emission of light in general, as we are dealing with single widely separated centres of emission provided by the atoms of the active metal. A great amount of interesting work has been done on the phosphorescence of substances of doubtful composition, especially for excitation with fast cathode rays, which excite a short phosphorescence in nearly all substances. It is hard to give a condensed account of this work, consisting, as it largely does, of observations under imperfectly known conditions of very complex phenomena: the lack of any broad theoretical basis for the class of experiments referred to renders generalisation as to many very interesting but apparently independent phenomena which have been observed almost impossible. I have therefore and from considerations of space confined myself to the long series of connected experiments made by Lenard and his collaborators and to the other experiments known to me which bear directly on the questions under discussion. May this brief description of systematic labours and able theorising help to demonstrate the significant and far-reaching results to which the careful study of a single, apparently insignificant, phenomenon may lead.

[1] A distinction without a difference, according to the pragmatists.

THE CORROSION OF IRON

By II. E. A.

In the first of the series of articles on this subject in this journal [1] reference was made to a number of experiments on the rusting of iron carried out by Messrs. Lambert and Thomson with very special care, and exception was taken to their conclusions in the following terms :

" There can be little doubt that although Lambert and Thomson were successful in carrying the purification of iron very far, they were not sufficiently careful to secure the removal of carbon dioxide from their apparatus. In view of the results obtained by others, it is inconceivable that they would have arrived at results such as they describe had they done so. And it is not difficult to see where they went astray. Whilst they took great care to prepare oxygen free from acid impurity by electrolysing a solution of baryta and all water introduced into the apparatus was carefully distilled from an alkaline solution, they evidently were not alive to the difficulty of removing carbon dioxide entirely from glass surfaces, although this has long been recognised ; a very large area of glass was exposed within their apparatus, especially in the vessel in which the oxygen was stored."

Mr. Lambert has continued the inquiry and has described his later work in a communication to the Chemical Society published in October last ; he has also discussed the subject in an article published in the *Chemical News* of April 13.

In repeating his experiments, he has used practically the same apparatus as before but has introduced a variety of additional refinements and precautions. The conclusion he arrives at is as follows :

" The results go to show that none of the criticisms is valid and that the claim which is founded on the experiments is substantially accurate—namely, that commercial forms of iron will undergo corrosion quite readily in contact with pure water

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[1] SCIENCE PROGRESS, No. 20, April 1911.—See also S. P., October 1911 and January 1912.—ED.

and pure oxygen under conditions such that carbonic acid (or any other acid) can neither be present nor be formed during the reaction."

The only possible comment on this is that Mr. Lambert cannot read, that is to say, interpret, his own observations. The apparatus used by him is shown in the figure. The oxygen vessel to which reference is made above is that marked D in the figure. In the earlier experiments, carbon dioxide was removed by merely exhausting the apparatus of which parts only had been subjected to the cleansing action of steam; while in the later series the exhaustion was proceeded with, the temperature of the part between H and L was raised "by heating a large metal plate fixed under the apparatus, with a hood of sheet asbestos   the parts above it. The oxygen storage
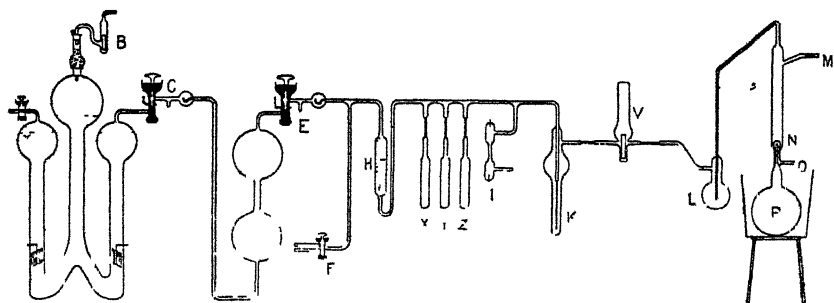
vessels and other parts which could not be heated thus were heated by means of a large blowpipe flame." At once it may be asked: Were the thick glass taps E and F thus heated? It stands to reason that they were not: so that the "other parts which could not be heated thus" were not all heated thus but only some of them; and in the case of those that were heated the heating could not have been carried to more than a moderate temperature.

Now what are the facts reported? *Experiment:* The vacuum was examined by means of the discharge produced by a large coil in the Plücker tube T. *Observation:* "During the last stages of this first exhaustion, *whilst the glass surfaces were being heated* (my italics), *the spectrum of carbon dioxide was seen* but it disappeared after some time, etc." *Inference:* Carbon dioxide was present in the first series of experiments criticised in the former article in this journal. Therefore, far from none

of the criticisms being valid, as Mr. Lambert asserts in the passage quoted above, the one of major consequence *is* justified by his own admission. Moreover, the criticism is still applicable to his later work, as he cannot possibly have heated the entire surface of his apparatus, and the degree to which he heated parts of it must have been such that it is not likely that he did more than drive off the major part, let us say, of the carbon dioxide condensed on and within the glass, thereby reaching an equilibrium, perhaps, but never removing the whole of the gas.

In order that there may be no misunderstanding, let me say that I hold it to be impossible to obtain valid results with an apparatus of so complicated a character, in which so large a surface of glass is exposed, as that used by Mr. Lambert; infinite opportunity is given in such an apparatus for the retention of carbon dioxide at the glass surfaces.

Mr. Lambert's views are summarised in the statement :

"The glass walls of all the vessels with which the water and oxygen came in contact had been subjected to the exhaustive treatment described above and so it may be said to be proved beyond any reasonable doubt that oxygen and water—both of the highest obtainable purity—have the power, of themselves, of causing commercial iron to rust.

"Further, the rusting seemed to take place as quickly as it does in ordinary air or oxygen and so it cannot any longer be maintained that carbon dioxide or any other acid is the ' dominant factor in the atmospheric corrosion of iron,' where commercial forms of the metal are meant."

Reading back we learn what is here meant, by "commercial forms of the metal" :

"Three different kinds of commercial iron were used, one in each vessel—namely, (1) a pure commercial electrolytic sheet iron ; (2) Kahlbaum's pure iron foil ; and (3) a cylinder of iron turned from a large nail taken from the roof of Merton College library while repairs were being carried out. This nail was made of very soft iron and was more than two hundred years old.

"The iron in each case was carefully polished with fine carborundum and then with Swedish filter paper. The results in all three cases were the same and did not differ in any respect from the results obtained with other good specimens of commercial iron used in earlier experiments. Corrosion was visible in a few hours and a considerable quantity of rust had formed within a few days."

Mr. Lambert took none of the precautions to cleanse the

surface of the "commercial iron," such as Moody and Friend have shown to be necessary, without which, as a rule, iron rusts even under the conditions these workers adopted—conditions which involved the exclusion of carbon dioxide, if not absolutely, to an extent far beyond that attained to by Lambert.

Mr. Lambert's second series of experiments, like the first, therefore, afford no proof of the validity of his contention that iron, both highly purified and commercial, can rust in the absence of an acid electrolyte.

In the latter part of his account he has much to say of the properties of the so-called pure iron which he prepared—not a few of his statements are self-evident propositions, though valuable and interesting as bringing out the influence impurities exercise in conditioning change.

Apparently the highly purified iron at his disposal was not so entirely exceptional as he implies; although it did not rust perceptibly on exposure to water and air, the rust test is probably a far less delicate test of purity than the acid test. It was attacked slowly by a cold, very dilute solution of chlorhydric acid and dissolved readily in chlorhydric, nitric and sulphuric acids on warming. It seems therefore to have been less highly purified than the zinc prepared by Reynolds and Ramsay, as this latter was scarcely attacked by acid.

Pieces of the iron which had been pressed by an agate pestle in an agate mortar were found to rust readily over the compressed part, whilst the uncompressed pieces remained bright. Mr. Lambert attributes the difference in behaviour to the difference in "solution pressure" but it is sufficient probably to assume that the conductivity of the metal is increased by compression and that the influence of such negative impurity as is present is thereby enhanced if no other explanation be forthcoming.

The object of Mr. Lambert's communication to the Faraday Society is to show, he says—

"that a simple and natural development of the ideas of Faraday on electrolysis will give us the beginnings of a satisfactory theory of the corrosion of iron—a theory incomplete as yet, owing to the lack of experimental facts, but one which is quite in accordance with well-established facts and which is not affected by the question whether iron is soluble to any appreciable extent in pure air-free water."

He then proceeds to sketch a "theory," but the terms used are those used in the previous articles in this journal. Evidently he has assimilated a good deal since the publication of his first communication to the Chemical Society and will soon be quite an orthodox exponent of electrolytic doctrine. I venture to think, however, that we have long had a satisfactory theory of the corrosion of iron—at all events, our "theory" of the process is certainly not incomplete owing to the lack of experimental facts but because of the general lack of appreciation of the facts, owing to the long-continued failure of chemists to take notice of a few fundamental principles. If, moreover, a simple and natural development of the ideas of Faraday will give us the beginnings of a satisfactory theory of corrosion, why, it may be asked, have Mr. Lambert and others been so slow in assimilating them?—they have simply never made the attempt until persistent hammering at the truth has forced them at last to pay some attention to it. But it is often and well said: better late than never. We may be thankful that some appreciation of the value to chemists of Faraday's teaching is at last being shown. That the tendency should become manifest even in a centre of feudalism such as that in which Mr. Lambert works bodes well for the future. Faraday's work was done only about seventy-five years ago, and therefore has not the crusted authority of Greek masterpieces.

At all events, Mr. Lambert now recognises that the presence of an electrolyte is an essential feature in rusting—he sees that otherwise there can be no change. As he says, "No part of the metal can dissolve unless an electric current actually passes through the electrolyte." Moreover, he implies, if he does not assert, that iron pure and simple, if in contact with a relatively electronegative conductor, will be attacked by water in the entire absence of acid. He speaks of water, however, as *the electrolyte water* and everything turns on this.

When he can show that water is an electrolyte, no one will hesitate to agree with him: but he cannot: both the evidence and theory go to show that it would not be, if it were obtainable. To quote, as almost every one does, Kohlrausch's experiments made many years ago in very ordinary glass as proof that water is an electrolyte is absurd; Kohlrausch never had water to deal with, and any one who seeks to refine on his experiments would only carry the purification a stage further

and observe a still lower degree of conductivity without ever arriving at water pure and simple.

It is worth while, however, to analyse Mr. Lambert's statements with regard to the case of a piece of ordinary commercial iron in contact with the electrolyte water ; they are as follows :

" Whenever we have two metals (or two modifications of the same metal) which are electrically different, that is, have different solution pressures and they are placed in metallic contact in an electrolyte, then the relatively electropositive part will dissolve with the production of an electric current flowing through the electrolyte from the electropositive to the electronegative pole and in the opposite direction through the metal.

" The rate of such a reaction depends (a) on the magnitude of the difference of electric potential—that is, the difference of solution pressure between the two metals, and (b) on the resistance offered by the electrolyte to the passage of the electric current.

" No part of the metal can dissolve unless an electric current actually passes through the electrolyte. The rate of the reaction may, however, be so infinitesimal that the amount of metal passing into solution will not be sufficient to respond to chemical tests even after long periods.

" Let us consider, in this light, the case of a piece of ordinary commercial iron in contact with the electrolyte water.

" Such a piece of iron is impure and not homogeneous— there are some parts of it which have a different solution pressure from other parts and so when it is placed in contact with the electrolyte water we have all the conditions for the production of an electric current.

" If the conditions are such that an appreciable electric current can pass between the two electrically different parts of the iron, the metal will dissolve at the relatively electropositive parts. The fact that iron is practically insoluble in pure water (in the absence of oxygen), shows that the current which actually does pass is so infinitesimal that the amount of iron dissolved cannot be detected, even after long periods, by chemical means.

" This may be due to two causes, namely (a) the small magnitude of the electromotive force, owing to the small differences of potential between the electrically different parts of the metal and (b) the great resistance offered to the passage of an electric current by the electrolyte.

" The writer's experiments seem to be generally accepted as proving beyond any doubt that commercial forms of iron will undergo corrosion quite readily in contact with pure water and pure oxygen in the complete absence of carbonic acid or any

other acid—that the only essentials for the corrosion of ordinary iron are water and oxygen. It is generally believed that iron must pass through a process of solution before rust is produced, and so, whilst the metal is practically insoluble in pure water alone, it must be soluble in the presence of oxygen. It follows, then, that oxygen must bring about some alteration in the conditions of the 'voltaic circle'—commercial iron and water—in such a way that a greatly increased electric current passes between the electrically different parts of the iron."

Firstly, it may be noted that Mr. Lambert here admits that iron is practically insoluble in pure water; he means, of course, water such as he has prepared, which cannot have been pure. It may well be argued, therefore, that as it has been shown to be *practically insoluble* in Lambertian water, iron would be *insoluble* in pure water.

Secondly, that he attributes very special influence to oxygen—of which more presently—inasmuch as he holds that pure water plus pure oxygen can attack iron in absence of acid.

According to Mr. Lambert, the current passing between electrically different parts of a piece of commercial iron in (Lambertian) water may be small because of the small differences of potential existing between the electrically different parts of the metal. This argument may at once be disposed of, as graphite and probably other impurities in commercial iron are just as effective as platinum would be.

That a great resistance would be offered by the electrolyte to the passage of the current is beyond question. And as even Lambertian water offers great resistance, water would offer infinite resistance; therefore there would be no current and no action if water alone were used.

It is necessary therefore to consider what are the alterations in the conditions which may be brought about by oxygen and whether these be such that iron would be attacked when subjected to the conjoint action of oxygen and water. According to Mr. Lambert—

"Oxygen must do one of two things—it must in some way or other increase the electrical differences between the parts of the metal and so increase the electromotive force or it must reduce the resistance of the circuit. When a piece of commercial iron is put into water in a vacuum, iron strives to pass into solution at the relatively electropositive part of the metallic surface, but hydrogen, produced by the electrolysis of

the water, is deposited at the relatively electronegative part. This film of hydrogen, forming almost instantaneously and covering up the negative pole, introduces an enormous resistance into the circuit and reduces the electric current to an almost negligible strength, so that the rate at which the iron passes into solution is infinitely small.

"Oxygen dissolved in the water probably acts by oxidising the hydrogen thus deposited at the negative pole—destroying the polarisation—and so allowing a greater current to pass between the electrically different parts of the metal.

"If this argument is true, then commercial iron ought to pass into solution, *in the absence of oxygen*, if it is placed in an electrolyte, such as copper-sulphate solution, where, instead of the non-conducting hydrogen film, there would be a conducting film of metallic copper produced at the relatively electronegative pole. Experiment shows this to be true. Commercial iron, when brought into contact with a solution of pure copper sulphate in a vacuum, causes the immediate deposition of copper on the iron just as readily as when the experiment is conducted in the presence of air; in short, all the copper is removed from the solution and iron takes its place."

Inasmuch as, *ex hypothesi* and in point of fact, oxygen and water are non-conductors both singly and when conjoined, the conditions are such, when only these are present, that an appreciable electric current cannot pass. But waiving this argument, the fact remains that we have no reason to suppose that oxygen can, in any way, reduce the resistance of a circuit— all substances which can do this are of the class commonly known as electrolytes, though in reality they only become electrolytes when used in conjunction with water. It can only act as a depolariser—but Mr. Lambert must pursue his studies of Faraday and perhaps of later writers also a little further in order that he may understand the office of the depolariser—that it not only exercises the cleansing effect to which he refers and also puts a stop to all back action but, which is far more important, throws energy into the circuit. Over and over again, this has been pointed out; but it is not yet part of "the simple and natural development of the ideas of Faraday" now in progress. However, we may hope that the doctrine may soon become the belief of pioneers like Mr. Lambert.

Apparently the part played by hydrogen polarisation is vastly exaggerated. We know perfectly well how small a part relatively it plays in an ordinary simple fluid cell and how

fluctuating is the influence it exercises; neither does it reduce the current to an almost negligible strength, though it renders it aggravatingly inconstant, nor has it such an effect that the rate at which the metal passes into solution is infinitely small.

Copper sulphate not only prevents any deposition of hydrogen on the negative surface but by exchanging copper for hydrogen contributes energy to the circuit: at the same time, owing to the deposition of copper, the resistance is greatly lowered, so that the action takes place more rapidly, both because the electromotive force is raised and at the same time the resistance is lowered.

But the changes pictured can only take place in an electrolytic circuit and such a circuit is only possible when iron is in contact not only with water and oxygen but also with an electrolyte; attack by water and oxygen alone is impossible.

In any case, the illusion under which Mr. Lambert rests, "that his experiments are generally accepted as proving beyond any doubt that the only essentials for the corrosion of ordinary iron are water and oxygen," should be dispelled by the above statements.

But there are other points of interest in his communication which deserve attention. He not only contends that he has prepared chemically pure iron but states that such iron can be exposed to the action of oxygen and water (even tap water) during an apparently indefinite time without showing any signs of corrosion. Chemical purity is not the only essential, however, as will be obvious from the following statement :

"In the preparation of pure iron by the writer's method the same sample of ferric nitrate, treated in exactly the same manner throughout its conversion into iron, will not always give like specimens of the metal.

"One batch of iron will rust quite readily, whilst another batch can be exposed for many months to the action of air and water without showing any signs of corrosion. All the pieces of the same batch behave, as a rule, in a precisely similar manner. Now, any difference between the batches cannot be due to differences in *chemical* composition. The only possible variable factors are temperature of reduction and rate of cooling, and so differences in the product must be of a *physical* and not of a chemical character.

"It is a very striking fact that the pieces of iron which will not rust can also be put in solutions of copper sulphate or

copper nitrate of any strength, without causing copper to be deposited on the iron, whilst pieces from a batch of iron which rusts always cause the deposition of copper from the same solutions of copper salts. Sometimes the copper is deposited quickly, whilst at other times several hours may elapse before the deposition of copper takes place. The metal is always attacked at one or more points and the deposition of copper spreads from these points over the whole surface in a very short time.

"It is clear from these experiments that physical differences in iron of the same high state of chemical purity can cause most profound differences in its behaviour.

"If the theory is true, we should expect such results. In the case of the iron which does not rust and is unaffected by solutions of copper sulphate or copper nitrate, the metal is probably physically homogeneous, at any rate on the surface. There would, therefore, be no differences of solution pressure—no electrically different parts—on the surface of the metal, and so no tendency for the metal to pass into solution by electrolytic action when the metal was put into an electrolyte. Since iron could not therefore pass into solution, we should not expect rusting to take place, nor should we expect the deposition of copper on the iron from solutions of copper salts."

Mr. Lambert states further that when pieces of metal which had been exposed during several months to the action of air and water without corroding were pressed upon by an agate pestle in an agate mortar and again exposed, they rusted in the course of a few hours, rust forming first at the edges which had not been pressed, while the pressed portions remained bright. In the same way, copper was precipitated immediately when the pressed pieces were placed in a solution of copper sulphate, deposition commencing at the unpressed edges. He assumes that the difference in physical state was the cause of the difference in the behaviour of the iron before and after it was subjected to pressure. But this explanation is not good enough.

In the first place, it is open to question whether two parts of a plate composed of a homogeneous material would be at different potentials after the one had been subjected to pressure if pressure had no effect in altering the chemical nature of the material and merely changed the electrical resistance of the one relatively to the other.

In the second place, it is improbable that different specimens of iron prepared by reducing oxidised iron in hydrogen—the

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method adopted by Mr. Lambert—should so differ physically that one would corrode and the other would not.

The facts point to the conclusion that the inactive samples obtained by Mr. Lambert consisted of iron which in some way had been rendered passive and that the effect of pressing with an agate pestle was to remove a protecting layer. The deposition of copper at the unpressed edges of the pieces is in accordance with this explanation; as Moody has shown, rust does not form initially in the iron itself, but separates from the solution, so that the position taken up by the rust has no special significance.

Mr. Lambert, it should be stated, has foreseen the possibility of the formation of a protective film on the surface of the metal —either of an oxide or of a hydride—but he has rejected the explanation. As it is not likely either that a hydride would be formed or that it would be effective if formed, it is only necessary to take the formation of a coating of oxide into account. Mr. Lambert contemplates the possible formation of such a film by the reversible decomposition of small traces of water in the hydrogen used for the reduction of the iron; he therefore dried the hydrogen used in reducing the iron oxide by passing it over phosphoric oxide, so as to remove all but the most minute traces of water; then the iron which was produced was brought into contact with copper sulphate solution while it was still in the atmosphere of hydrogen. As there was no deposition of copper, he came to the conclusion that the inactivity of the metal would not be accounted for by the presence of a protective film of oxide. But drying the hydrogen so thoroughly in such a case can only have been a work of supererogation during the greater part of the operation, as water is one of the products of change; it could only be effective towards the close. In view of the affinity of iron and oxygen, taking the behaviour of iron into account, it is more than probable that in some of Mr. Lambert's experiments the metal produced was superficially coated with oxide, perhaps in consequence of the introduction of a little oxygen together with the hydrogen.

It would seem therefore that the argument used against Moody, which was shown by him in advance to be untenable, is actually applicable to Mr. Lambert's work: his results, in fact, appear to be open to doubt on more grounds than one.

As pointed out in the third of these articles, Dunstan in particular has called attention to the manner in which various agents inhibit rusting, and has sought to show that the conclusions arrived at by Moody and Friend are invalidated by their having used such substances in their experiments.

It has been shown by Friend that the inhibiting effect of alkalies is due, in all probability, to the retention of a certain amount of alkali at the surface of the metal; this appears to be in some degree porous, so that the alkali can be removed only by long-continued washing—a precaution which Friend adopted in his ingeniously simple experiments referred to in the first of these articles.

The wonderful efficiency of the film formed on slightly heated steel in protecting it against corrosion is well known. Next in protective efficiency comes that which is formed when the metal is rendered passive in nitric acid. But other oxidising agents appear to act only so long as they are in contact with the metal; I have often had occasion to observe of late that they cease to be effective very soon after the iron is withdrawn from their influence.

It is to be regretted that Mr. Lambert did not take advice before continuing his experiments, particularly before publishing the account of his further work: had he taken the opinion of those who have given special consideration to such matters, he would probably have carried out the inquiry, if not in a more effective manner, at least more circumspectly, so that the time spent would not have been largely wasted in asking questions in such a way that the answers are of little avail. The possible flaws in his arguments would have been indicated.

Subjects so intricate need to be dealt with comprehensively, in the light of a mature experience; and the inquirer should ever be mindful of the pitfalls which threaten each step he takes.

At present, instead of seeking counsel of one another, we too often affect secrecy and resent all criticism.

Individualism is undoubtedly the very breath of science, but it now needs to be tempered judiciously with collectivism. Our present failure to discuss and dispute is largely the cause of the absence of understanding which now overshadows scientific workers.

Bodies such as the Chemical Society in the near future will need to be more alive to their responsibilities to their members and no longer confine themselves to the perfunctory performance of their duties as publishing organisations. The practice which prevails in several academies of submitting the more important communications they receive to the opinions of referees and of publishing the reports that are given, might with great advantage be extended; such reports would serve to guide readers, and inform them to what extent the opinions advanced were open to criticism at the moment. We are now undertaking tasks of extraordinary difficulty and it behoves us collectively to discover some means of promoting the efficiency of our individual efforts.

RECENT WORK ON VOLCANOES

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THE volcanic regions of the globe have long been known and most volcanoes have been described in detail, so that it is to be expected that a certain definiteness would have been reached as to the nature of volcanism. As to the cause, that is another matter—but just what volcanoes are and what happens when they become active, surely that ought to have been settled now beyond question. This is not the case. The investigations into the West Indian eruptions of 1902 threw a flood of light on the subject, in which, however, there are still many lacunæ. Dr. Albert Brun's daring work in Java and elsewhere has opened up an entirely new chapter, whilst Reck's work in Iceland and Russell's on the Snake River Plains of Idaho has so largely increased our knowledge that it can hardly be maintained that we have really known anything about the subject of volcanoes till quite recently. I propose in this article to review this recent work briefly, confining myself to actual observations in the field or the laboratory, and picking out only those points which are fundamentally new.

I will begin with the West Indian eruptions, more especially dealing with Mont Pelée. I need not enter into general details, as these have been so adequately described by Lacroix (1), Flett (2), Anderson (2), Russell (3), and Heilprin (4), whilst an exceedingly interesting collection of letters from eye-witnesses has been published by Flammarion (5). Mont Pelée, which but once, in 1851, has been known to show signs of activity and then only by throwing out a harmless shower of ashes, commenced its eruption on April 25, 1902. Excursionists immediately ascended the mountain and found that the bowl-shaped hollow at its summit, called L'Étang Sec, was being filled up with boiling mud from which sulphurous vapours were being given off. Eight days later, ashes were ejected, and on May 5 an avalanche of incandescent mud rushed down

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the valley of the Rivière Blanche and overwhelmed the sugar factory of M. Guerin, burying the owner and his wife and twenty-five employees. On May 8, at ten minutes to eight in the morning, a blast, blown as if from a funnel, and directed immediately on to the town of Saint Pierre, scorched and killed every living being, with the exception of two men, who was within the city, to the number of twenty-six thousand. The area of total destruction was quite narrow, but all the country to the west and south was scorched, though many people escaped who were within this outer zone. Other eruptions occurred on May 26, June 9, July 9 and 11. On August 30, after a period of quiet during which the residents around the mountain were beginning to become reassured and the fugitives to return, a second blast, as sudden and fierce as the first, was blown out, directed this time to the south and east, which destroyed a new area of country. Heilprin had actually visited the crater on the previous day and was on the margin of the cloud when the blast occurred. It is the nature of this blast which is of the utmost interest; the shower of ashes which preceded it and the torrential rain due to the violent disturbance of the atmosphere, which washed down this ash and covered everything with a slimy coating of grey ash, are phenomena which are well known from other volcanoes in their explosive stage.

Pliny, Epistola XX., describes a similar blast in the eruption of Vesuvius in 79 A.D., an eruption of a volcano likewise starting activity after a lengthy period of quiescence: "*Ab altero latere nubes atra et horrenda ignei spiritus tortis vibratisque discursibus rupta in longas flammarum figuras dehiscebat; fulguribus illæ et similes et majores erant,*" which we may translate, with the accounts of the eye-witnesses of the Mont Pelée eruption to guide us: "From the other side a black and terrible cloud—the spirit of fire—belched forth with whirling and quivering offshoots, and rent with long trails of flame like flashes of lightning, only broader." Earl Orrery in his translation renders *spiritus ignei* as "charged with combustible matter," but the sense seems to be more the "essence" or "soul of fire"; the descriptions of those who breathed this "spirit of fire" and the condition of the bodies both at Pompeii and Saint Pierre seems to point to something more than combustible matter or even heat.

Two people escaped from the area of all but total destruction.

Of these, Leon Compère-Leandre, a shoemaker, was sitting on his doorstep at the time of the blast; he rushed indoors and sheltered himself under the table. Four others came running into the room, one of whom, a child of ten years, dropped dead and the others fled. He himself came out from under the table and went into another room, where he found an old man who had fallen dead on his bed; the corpse was blue and swollen, but the clothes were intact. After finding the rest of the people in the house were dead, he threw himself on his bed and lost consciousness. At the end of an hour he woke up to find the roof burning; then, covered with burns, he fled and reached Fond-Saint-Denis, three miles distant, where he was attended to. He said that he had not felt a sensation of suffocation nor was there a want of air, only that the air was burning.

The other man who escaped was Auguste Ciparis, a negro, who was shut up in a cell in the prison without a window and only a narrow grating in the door. He was waiting for his usual breakfast on the 8th when it suddenly became dark; immediately afterwards hot air entered his cell through the grating. It came gently but fiercely. There was no smoke nor noise nor odour to suggest burning gas, but it burnt his flesh; he was clad in his hat, shirt, and trousers, but these did not take fire, yet beneath his shirt his back was terribly burned. The water in his jug was not affected and this was all the nourishment he had till he was rescued three days later.

Most of the victims seemed to have succumbed instantaneously, as if from a blast of choke-damp. Some were burned internally, having as the coal miners say, " swallowed fire "; in some instances their heads burst; others were scorched all over. A doctor's carriage stood ready before the house with the charred body of the horse in its place before the carriage; the metal parts remaining showed that it had not moved and the coachman was by its side. Clothing was never burned, but the victims in the streets had their clothes torn off them by the rush of the blast, as happens sometimes in a severe tornado in America. People in the outer zone who were rescued fell into two classes : those who were burned internally —that is to say, the upper part of the respiratory canal was destroyed; these all died. Of the others, some were singed all over, whilst some again were burned on the face and on their hands, and these mostly recovered quickly.

The evidence seems to point to the blast having been made up of an intensely hot heavy gas. Sulphurous vapours were given out before the blast but did not accompany it. M. Molinar, who observed the whole occurrence from Mont Parnasse, relates that the volcano vomited fire during a quarter of an hour and then became completely quiet; at eleven o'clock, lava and smoke began to pour out. Had the blast been water-vapour, there should have been some clouds due to the condensing vapour, but though the wind was blowing away from where M. Molinar stood and the view was perfectly clear, no clouds were seen to form. The statements at any rate establish the fact that a volcano can discharge a mass of gas downwards and that this gas is like that of a mine explosion. It desiccates, as witness the trees in the outer zone which were rendered sapless, but the leaves still hung from the brittle twigs; and it is certainly not water-vapour. What this gas is can only be guessed from Brun's researches.

Dr. Brun commenced his work in 1901 and finished his field observations in 1910(6). During this time he had visited the Italian volcanoes, those of the Canary Islands, Java and the Hawaiian Islands. His laboratory work consisted in determining the melting-points of rocks and rock-forming minerals, especially those of volcanic origin, and the analysis of gases collected from actual volcanoes either in the explosive stage or driven out of lavas in which they had become dissolved or occluded during cooling. Brun's method in the field may be gathered from his account of the ascent of Mount Semeroe in Java. Having watched the crater in eruption from a distance for some time, Brun desired to look down into the working chimney. Profiting, then, by an interval between two explosions he rapidly approached and stood on the actual rim of the crater. He was able to snap three photographs one after the other. Hardly had he finished when an explosion burst out—still he could photograph, though incandescent blocks fell all around. He observes that investigations made overlooking the volcanic orifices during the paroxysmal stage are very rare and to profit by them one must have complete control over oneself and know beforehand on what one must concentrate one's attention. When he arrived at the rim of the crater the western chimney of the three that were filled with liquid lava was belching forth gas and bluish smoke; little masses of lava were being gently

lifted and from the resulting crack gas was being vigorously expelled, rising with a violent whirling motion like that of a water-spout. The gas and fumes were insoluble in air. At the moment of the explosion not much could be seen, but from the number and velocity of the ejected blocks it was evident that the nearest chimney had entirely emptied itself. The rim on which he stood was swept with fumes, but there was no condensation of moisture on the cool surface of the rocks. On another occasion Brun thrust his geological hammer into the uprushing stream of gas and no water was condensed on the bright metallic surface.

In a neighbouring volcano, Bromo, the continued explosions prevented Brun from looking down into the crater. So he caused a little platform to be cut in the loose cinders just under the rim on the outside; on this he established his battery of thermometers, barometers and hygrometers, and also a little pump which had attached to it a long train of glass tubes connected by indiarubber joints, which was dangled into the crater. When an explosion took place the hygrometer showed no excess of moisture in the air. I can, however, find no account of an analysis of the gas thus collected directly from the throat of the volcano by the pump. Elsewhere Brun relies on the gases occluded in the lavas; these are expelled on heating the rock to a certain temperature above the melting-point. Plutonic rocks and lavas which have been in existence for long geological periods are "dead," and do not contain, or have lost, occluded gases. Recent lavas when heated to their explosion-point suddenly give off with tremendous violence large quantities of chlorides—magnesium, iron, and silicon—together with ammonium chloride, carbon dioxide, carbon monoxide, marsh gas, chlorine, hydrogen chloride, and less frequently sulphur dioxide and sulphuretted hydrogen, and lastly, hydrogen and nitrogen, but neither oxygen nor water. Gautier (7) points out, however, that the gases of fumaroles are generally hydrous. But then fumaroles belong to a late stage of the volcano, when the activity is dormant and water from the surrounding rocks can percolate and attain to the hot centre of the volcano and thence be driven up to the surface of the earth.

Fouqué's analyses of the gases from Santorin in 1866 (8), although collected from the surface of sulphurous water in a fissure, contained only traces of oxygen but nearly 30 per cent.

of hydrogen, nitrogen and carbon dioxide practically making up the rest. Specimens of the gas taken in later stages show a progressive increase in oxygen and in carbon dioxide. The fact that chlorides of magnesium and iron are deposited on cinders around the crater again proves, according to Brun, that the exhalations of volcanoes are anhydrous. The "steam" of volcanoes consists of volatile chlorides, mostly ammonium chloride. If the "steam" had been water-vapour, it would dissolve in air and soon disappear. The white cloud, on the contrary, remains suspended during long periods over the volcano and the wind may carry it many miles to leeward. The most positive evidence Brun advances is his measurement of the humidity of the cloud given off from the pit of Kilauea in eruption relatively to the humidity of the air outside. In a long series of observations, he found that there was less moisture in the cloud than outside it, and consequently he inferred that there was no water-vapour in the exhalation. On the other hand, the cloud of the fumaroles on the north of the pit, in action at the same time as the volcano, contained much water-vapour. Gautier found from 62 to 77 per cent. of water-vapour in the fumaroles of Vesuvius after the eruption of 1906; but in view of Brun's work in Hawaii, one is not justified in maintaining that the gases of the central chimney must equally be hydrous. Moissan's (9) analyses of the gases of the Mont Pelée fumaroles, interesting from the fact that considerable quantities of argon were discovered in them, show large amounts of oxygen and water-vapour. It will be remembered, also, that in the beginning water was pumped up into L'Étang Sec and caused the mud-rush which overwhelmed the Usine Guerin. That is to say, when a volcano begins to work after a period of quiescence, the volcanic gases drive before them the water contained in the crevices and pores of the rocks; then, when the eruption ceases, the same water from the surface seeks to penetrate again into the cracks which it previously occupied. As the pressure of the volcanic vapours grows less and less, the surface water advances more and more into the heated area, till, coming at last into the neighbourhood of the cooling molten rock, it is driven forth in the form of aqueous vapour mixed more or less with volcanic products.

The elements of water, it is true, are found in the volcanic exhalations, but combined with carbon, chlorine, or nitrogen.

The combined nitrogen appears to be the result of the action of hydrogen on metallic nitrides. Silvestri (10) actually found nitride of iron on the surface of lava from Etna. Metallic nitrides, when heated with hydrogen or water-vapour, yield ammonia, and this would readily form sal-ammoniac with the hydrogen chloride of the exhalations.

So far for the gases given off from volcanoes; the types of volcanoes that yield them are those that have been known since the earliest times. In Iceland and in the Snake River Plains of Idaho, there are types that are entirely new to scientific literature. The commonly known types are mostly those connected with the folding in the earth's crust. In the Mediterranean and West Indies the volcanoes lie uniformly at the back of the great folds; in the Ægean and in Mount Ararat, the volcanoes lie in a "Schaarung" or knot where two systems of folds meet. In Kasbek and Elbruz the cones lie in the centre axis of the folds, while in the Andes they are related at any rate to the folds in that they follow lines of weakness determined for them by the curvature of the strata. In Iceland and in Idaho, the whole country for thousands of square miles has been a seething mass of lava and the vents rise through it as if drilled by gases that have come through a semi-viscid magma without any sort of order. The special types represented here are the explosion rings, the slag craters, and the buckler cones; then there are the fissure eruptions which are well known and the volcanoes of block-uplift which are new to science. Although the description of these is due principally to Dr. Hans Reck from examples in Iceland, they were being investigated by Walther von Knebel at the time of his death. The latter with two companions had ascended the most wonderful of all volcanoes, the Askja, camping on the shores of the lake that lies in the south-eastern corner of the caldera on top. On the fatal day, July 10, 1907, he and his artist friend Rudloff had taken a collapsible boat and had gone for a row; when the third member of the party returned to camp there was no sign of the others. A relief party was immediately sent out, but nothing could be found of the missing ones. Dr. Reck the following year visited the place and spent eleven days searching for a clue to the mystery; the only result was the surmise that an avalanche of rocks had overwhelmed the frail boat and its freight.

Iceland is an elevated portion of a plateau of basalt and pelagonite tuff that at one time stretched in a continuous field from Antrim in the north of Ireland to Greenland, a distance of a thousand miles. The Faroë Islands are an isolated remnant of this plateau; all the rest has sunk beneath the sea. The first of the lavas rests on the topmost beds of the Cretaceous system in Scotland, so that presumably the eruptions commenced in Eocene times and are still going on at the present day in the northern part of the area.

On the other side of the Atlantic, in Oregon, Washington, California, Idaho, and Montana, an extent of country larger than France and Great Britain combined has been flooded with basalt; the previous topography has been buried under lava 2,000 ft. thick and in some places 3,700 ft. thick, the surface of which is a level plain like that of a lake-bottom. In the Snake River Plains, a part of the larger area, the lava rolls up to the base of the hills on the north and on the east and follows the sinuosities of their margin as the waters of a lake follow its promontories and bays. The basalt rests on beds of lapilli which may reach 180 ft. in thickness, and these in turn rest on lacustrine deposits. I follow I.ˑ C. Russell's description of this area (11).

Explosion Rings.—These are the more primitive forms of what Judd calls crater rings, of which many examples occur in Italy, such as the hollows in which lie the lakes of Bolseno, Bracciano, Albano, Nemi, and Frascati. The simplest of all occur in Idaho near Cleft, where there are two circular holes drilled through the basalt without any elevated rings. Their diameters are 1,100 and 800 ft.; the encircling cliffs rise 200 ft. above the floor, which is composed of fine yellow soil. In Iceland (13) we find a slight development; the type is the Hrossaborg, near Akureyri, the capital of North Iceland. Here the plains consist of doleritic lava overlying pelagonite tuff, and the volcanic eruption has lifted up a portion in the form of a circular hill with a crater, some 800 yards in diameter, on top. The only products of the volcano were gases which have drilled the circular chimney and elevated the rocks around. The inner walls of the crater are 120 ft. high, and on all sides the rocks slope outwards. It is a typical crater of elevation according to Leopold von Buch, only unfortunately we cannot apply this term to this type now, as the original name was used erroneously

for the ordinary strato-volcanoes which are built up of successive layers of ash and lava-flows from the actual chimney that they surround ; in the Hrossaborg the lava and ashes are older, and came from other volcanic vents or fissures.

From these explosion rings, or Gasmaare, as Beck calls them, we pass to the well-known crater rings surrounded with low crater walls formed of tuff and lava ejected from the volcano. Many examples occur in Iceland and Idaho, but no special mention of these is necessary here, unless to point out that in Idaho vast streams of lava issued from them. These heads of the lava columns are covered with scoriaceous and ropy lava, which makes them look like the tops of great springs of water suddenly congealed. In one case, a particular lava-flow had its origin in two such pools, and a mile from its source it was joined by a still larger river of lava. The united streams flowed some thirty miles, descending about a thousand feet, more than half of the fall being in the first ten miles, so that the distal portions flowed on a gradient of 1 in 200. Other streams have flowed for fifty miles in the same area in rivers of molten rock one to three miles across and 300 ft. in thickness.

Slag Craters.—Two volcanoes of this type are described by Russell from Idaho. Blanche Crater rises about 60 ft. above the plains, and has a perfect crater on top ; the conical pile is composed of thin cakes of highly vesicular lava, which have been blown out in a plastic or liquid condition. It is of quite recent origin, as it lies in a canyon excavated 500 ft. in the older lava. The other example is the Martin Butte, likewise a conical pile of scoriaceous lava. In Iceland, slag cones are extremely common and form the most weird objects in the landscape, as the viscid lava has built up piles of all shapes, resembling towers, organ-pipes, needles, or gigantic skittles (12). They vary from 150 ft. in height to quite small hornitos or blowing and driblet cones. They are often assembled in swarms, as if a great mass of gas had pierced a viscid covering along a number of independent channels. They frequently form, also, the caps of the next type of volcano.

Buckler Cones.—One example has been described from Idaho, the Black Butte ; it rises 300 ft., with a base two miles in diameter. It is built up of successive layers of highly scoriaceous lava, which flowed away in all directions, and there is no evidence at all of lapilli or cinders. There is no crater

on top, the last lava-flow having filled it up. In Iceland this form is very common, some nineteen " Dyngjen " being known, but owing to the low angle of the cone, the slope varying from 6° to 8°, they are easily overlooked, especially in the snow-covered area. One such buckler cone, the Skjaldbreid, is 3,000 ft. high, and seven miles in diameter at the base; it has a small crater on top, but others may have very large ones. In the Kalotta Dyngja, a post-volcanic fissure has cut through the cone, and it is therefore possible to study its internal structure. Mauna Toa, in Hawaii, belongs to this type, although the great spreading base is concealed beneath the ocean.

Fissure Eruptions.—Iceland has long been known as the typical locality of this type of volcano. The eruption of Laki, or Skapta Joküll, occurred in 1783. The first eruption took place on June 8, and was accompanied by tremendous detonations and earthquake shocks. A great black bank of ash was thrown into the air, in which several up-rushing columns could be seen; that is to say, the explosions occurred at several places along the fissure. Later on, the explosive stage became confined to the southern half, while the northern half poured out lava, as was evident from the reflection of the glowing mass in the overhanging canopy of cloud. On June 12 a lava stream, 200 yards wide, had flowed nine miles down the bed of the Skapta River. The lava in this part is covered with hornitos, little blowing cones, whose origin is ascribed to the escape of water-vapour which the lava had absorbed from the river water. Towards the end of June the eruptions ceased for a time, but in the beginning of August activity was renewed, and stream after stream of lava flowed down the river-beds, destroying all the meadow land adjoining. After a period of rest, the eruptions started again on October 25, when the entire plain in the neighbourhood became a glowing lake of lava, and the molten rock continued to flow during the whole of November. All this time the air was filled with ash and sulphurous vapours, and the vegetation over a large part of the island was killed; half the animals perished, and 5,000 people, out of a total population of 50,000, died of famine or disease. Iceland is full of such fissures, as also in all probability was the whole basaltic plateau of which it is part. The effect of fissure and other eruptions occurring more or less simultaneously over an area little short of a million square miles

must have had a far-reaching influence on the climate of the world; one can almost assert that it was this which was the cause that enabled a tropical flora to flourish in the Eocene period close to the North Pole, and that the epidemics consequent on the pollution of the air were a factor at any rate in the extermination of the Mesozoic types of animals. Not only in North Europe and America were these volcanic outbursts active, but in India the Deccan traps were extruded at about the same time, and also probably the lavas of the Mawi plateau in Central Africa. Contemporaneously with these eruptions the crumpling of the earth's crust, which gave rise to the Alps-Himalayan chains and the folds of the east of the Pacific, was also started; the vast dislocations of the earth's crust and the floods of lava which issued from it in certain parts, bring up the question whether this solid earth can contain within itself such terrific forces of disruption; or whether it is not more reasonable, seeing that we have recently had visitors from celestial space such as the planetoids Eros and M.T., which, had they fallen upon the earth, would have caused just such disturbances, to ascribe the early Cainozoic eruptions and crumplings to causes operating from without.

Volcanoes of Block-Uplift.—Reck (13) calls these Tafelberghorste, but in Iceland they always have a volcano on top. The question whether they are horsts, that is, blocks from which the neighbouring country has been faulted away, or whether they owe their origin to vertical uplift, is a matter very difficult to decide. In the Utah and Colorado plateaux, the whole country is parcelled out in long strips and the difficulty of explaining the occurrence here is as great one way as the other. If the valleys between the long plateaux had been faulted down, how could the strips between have been sustained, with the earth's crust all shattered around them? It is like the case of a pancake laid on a gridiron, but then the rods of the gridiron are here represented by narrow slips of rock fifty or more miles long, and these are not strong enough to allow of suspension from the ends. Masses of igneous rock pumped up by hydraulic pressure would supply an elevatory force for the plateaux, and this seems a more reasonable explanation; hence these Colorado and Utah plateaux are still called mountains of block-uplift. In the Ries (14) in Germany, again, there is a circular depression some fifteen miles in

diameter; it is surrounded by Jurassic strata resting on granite, whilst in the depression, whose floor is the same granite, the level of this rock is above that in the surrounding country, where it is covered with sedimentary beds. The Ries granite, then, is a gigantic plunger which has been elevated by volcanic forces, and the balance of evidence seems to indicate that the fault-blocks of Iceland have been elevated in a similar manner, although they are bounded by quadrilateral and not circular faults.

The simplest example of a volcano of block-uplift is the Herdubreid in the lava desert of the Odadahraun. The cliffs surrounding the block are some 1,800 ft. high, 300 ft. of which are concealed under tabus heaps. The rock comprising them is brown pelagonite tuff, covered on top with the basalt, which flowed from the central chimney. The volcano is of the buckler type, with a deep central crater, from which lava poured out in a symmetrical low angle cone. It is 5,450 ft. high and rises some 4,000 ft. above the plain. The walls of the pedestal on which the lava rests are kept quite fresh by the enormous weathering that goes on in such regions; there is no sign of any fissure traversing them by which the volcanic gases could have risen to form the chimney. The block has been driven upwards between two sets of crossing faults and an escape vent has been drilled in the centre through the solid rock.

To the south-west of the Herdubreid lies the much larger Dyngjufjöll block, with the square caldera of Askja at its summit. The lava desert, with its surface so scoriaceous and rent with chasms that it is all but impossible to traverse, is here covered with pumice thrown out by the Rudloff crater which lies in the Askja. A narrow gorge, the Askja Op, leads up to the top at the north-eastern corner. On entering the Askja, one finds oneself in a wide, level plain filled with slaggy lava and surrounded on all sides by steep hills, whose crests turn round at right angles and enclose the square caldera. The area of the depressed lava-field is about sixteen square miles. The surrounding hills rise from it 1,000 to 1,200 ft., but from the outside they rise from 2,000 to 2,500 ft. The outer dimensions of the block are, roughly, fifteen miles on all four sides. The remarkable fact about the Askja is that the boundary hills are made of the older pelagonite tuff of the same nature as that forming the pedestal on which the Herdubreid volcano

stands. In the Askja the volcano was formed in the same way, but towards the end of its activity the mass of lava collapsed, leaving a rim of the pelagonite tuff standing all round.

In the south-eastern corner lie the two crater lakes, the von Knebel and the Rudloff lakes. The former is much the larger; it lies against the marginal hills which rise 1,500 ft. above the level of the water in step-like or vertical cliffs. On the north and west the walls are made of the Askja basalt in which the lake is sunk 180 ft. Owing to the great steepness of the sides, there is a continual falling of stones, some of which shoot out a couple of hundred feet into the lake. Along the southern shore there are a great number of solfataras.

The Rudloff lake, so named after the artist who was with von Knebel when he met his death, is of much more recent origin. It was formed in the eruption of 1875, and the pumice thrown out of this small orifice still covers all the eastern side of the island. There is a small crater ring round it, rising some 35 ft. above the Askja lava, but the level of the lake is 180 ft. below it. The water is milky white and still steaming, while from the surrounding walls solfataras gush forth, covering the rocks with sulphur and gypsum crystals.

The Dyngjufjöll with its Askja caldera stands isolated and almost in the centre of eastern Iceland. No vegetation grows upon it and there is none within many miles; all around are the plains of bare, black lava, covered in places with the grey pumice of the Rudloff crater. The ponies carrying supplies have to be driven back to grazing-ground immediately they have been off-loaded, and should an expedition be cut off by storms or by other mishap from relief from outside, it would be quite impossible for the members of the expedition to reach safety. Caldera are now known from many examples, such as the above-mentioned case of the Ries in Swabia, and there is an excellent instance of one in Glen Coe in Scotland (15). These types simply show a central plunger with crush zones and volcanic products round the rim. In the Hegau (14), not far from the Ries, we have an example where the floor of the depression is flooded with lava. All these are circular pits; it was not till the Icelandic occurrences were described that the relationship between the caldera and faulting became clear. In the Askja, in addition, we have two sets of faults: an outer set by which the block was elevated either relatively to the surrounding

country or absolutely; these were not connected with volcanic outbursts. Then followed a collapse, and along the faults thus developed inside and parallel to the old ones lava was extruded and explosive volcanoes, like the Rudloff crater, broke out. The two blocks fit into one another like the joints of a telescope, and the last stage of the Askja volcano, judging from the Ries and other caldera, will be that the inner core will rise through the outer rim and finally settle as an elevated block.

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A CONTRIBUTION TO THE BIONOMICS OF ENGLISH OLIGOCHÆTA

PART I. BRITISH EARTHWORMS

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110, *Wilmot Road, Swadlincote, Burton-on-Trent. April* 10, 1913

Scope of the Inquiry.—The annelids fall into two great orders, which are known respectively as Polychæts and Oligochæts. The former are marine, the latter terrestrial. Polychæts are so named on account of the large number of bristles, chætæ or setæ, which are a characteristic of many of the species; while the Oligochæts are marked by the comparative fewness of the setæ. It is true that some Polychæts have few setæ, and some Oligochæts have many, but that simply shows that Nature is not bound by human laws, or that no system of classification is perfect. It is not proposed in this paper to inquire into the bionomics of the Polychæts, the other great order being more than sufficient for our present study. The Oligochæts fall into various groups, and each is worthy our most careful investigation. But in order that we may gain an accurate knowledge of our subject it is necessary to restrict ourselves to those species which are indigenous; and as these again are arranged in different families, each of which has its own peculiarities, the inquiry will in the present instance be limited to the largest forms of terrestrial annelids found in Great Britain. These are popularly known as Earthworms, and thus we are reminded of that interesting and instructive volume by Darwin entitled *Vegetable Mould and Earthworms.*

In spite of the splendid lead which that volume gave to a subject of supreme importance, it is surprising how indifferent the public has remained to the life-history and economics of this class of animals. Many thousand copies of the work were sold, and doubtless hundreds of readers opened their eyes in amazement as they read. Then the book was closed, and the eyes as well, never to be reopened except in the case of one

or two enthusiasts, who have quietly carried on the work during the intervening quarter of a century, with very amazing results. The time has now come when it is possible once more to survey the subject, and create a new point of departure.

The Number of Species.—As our inquiry is limited to the British Lumbricidæ, the question naturally arises, How many species of Earthworm are there in the British Isles? It will be instructive, in answer to that query, to look a little into the history of the subject. In 1865 Dr. G. Johnston compiled *A Catalogue of British Worms*, based on the collection then found in the British Museum. The number of Lumbricidæ there recorded is eleven, about half of which are satisfactory, while the remainder are doubtful. Under one or two headings we find more than one species confused, while in other cases the same species appears under more than one name.

Darwin does not allude to Johnston's catalogue. He remarks that "The British species of Lumbricus have never been carefully monographed; but we may judge of their probable number from those inhabiting neighbouring countries. In Scandinavia there are eight species, according to Eisen; but two of these rarely burrow in the ground, and one inhabits very wet places or even lives under the water. Hoffmeister says that the species in Germany are not well known, but gives the same number as Eisen, together with some strongly marked varieties."

When Dr. Rosa published his *Revisione dei Lumbricidi* in 1893 he enumerated six species of Lumbricus, forty-nine of Allolobophora, and six of Allurus. Thus the number of European Lumbricidæ had been raised to upwards of sixty species. Beddard two years later issued his *Monograph of the Order Oligochæta* (1895), and allowed three species of Allurus with Tetragonurus, fifty-two of Allolobophora, and seven of Lumbricus known to science. The following year (1896) de Ribaucourt's *Étude sur la Faune Lombricide de la Suisse* appeared, and no fewer than forty-four species of Allolobophora were recorded for Switzerland alone, in addition to seven species of Lumbricus and five of Allurus. Passing over the work of Vaillant, Oerley, and others, we arrive at the year 1900, which marked the appearance of Michaelsen's volume on *Oligochæta* (*Das Tierreich*, x.), in which the number of species has grown beyond all bounds.

My own researches commenced in 1890, and it was then assumed that our native Earthworms numbered half a score,

or at most a dozen species. To-day the figure stands at forty and upwards, and there are doubtless still several discoveries to be made in our gardens, islands, and mountains. It is with these forty species that we are immediately concerned.

Rarity and Frequency.—It must not be assumed that they are all generally distributed over the British Isles. In a few instances the species is represented by a solitary specimen, and in others, while the number of specimens is unlimited, they are at present known in only one locality. While many are common throughout the country, as well as in Europe, others have a range which is very instructive. Let us take a few examples. In 1892 I wrote to Dr. Rosa of Turin to the effect that a new worm (*Lumbricus papillosus* Friend) had turned up in Ireland. He alludes to it in an appendix to the genus Lumbricus (*op. cit.* 27), and notes incidentally that the name had already been appropriated by O. F. Müller. On this account Cognetti afterwards changed it to *Lumbricus friendi.* This species has been sought unceasingly in every part of England, Scotland, and Wales without a trace being found, yet I no sooner landed in Dublin in March last and began my researches than it turned up in plenty. In 1890 Michaelsen placed it in his list of species, and recorded it for Switzerland, while Southern has more recently remarked that " *L. friendi* is common in the south of Ireland. On the Continent it is markedly alpine in its range, and is only found at considerable elevations in the Pyrenees and the Alps." In the light of Taylor's recent paper on " Dominancy in Nature " this is most instructive.

We may compare with this the distribution of another of our British Lumbricidæ, which, so far as I am aware, has never been studied by any other investigator but myself. In 1910 I was spending Easter at Bridlington, and found a solitary specimen of *Octolasium gracile* Oerley. It was new to Britain, and would seem to be gradually working towards the west. Up till the present it has never been found in Ireland, Wales, or the West of England, and in Scotland and the Midlands is very rarely seen. Yet in the autumn of 1911 it was the dominant Lumbricid at Sutton Broad in East Anglia, and in Epping Forest and elsewhere in the south and east it is quite gregarious. Unfortunately Michaelsen confuses it with *O. lacteum,* from which, in England at least, it is absolutely distinct; and

thus we are unable at present to give its Continental distribution with certainty. Oerley found it near Budapest and Vlissingen. He also found it, or a variety, alike in Hungary and at Woolwich. I cannot distinguish the Epping Forest forms from that named *O. rubidum* Oerley. Mons. de Ribaucourt regarded *O. gracile* as a subspecies of *O. profugum*, and records it as such for Switzerland. Is it possible that in England it has developed along definite lines, and so become a well-marked species, while in Europe its affinities with *O. lacteum* Oerley (= *O. profugum* Rosa) are still clearly marked ?

Some curious facts relate to the genus Allurus. It was recorded as British by Johnston, and rediscovered about 1890 in Dorsetshire and Devonshire. The type (*A. tetrædrus*) is now known to be one of our commonest worms. It occurs in every part of the British Isles by streams, water-courses, ditches, ponds, and water generally. The type, moreover, is very constant in this country. I have found one or two varieties in different parts of England, but they have been marked chiefly by variations in colour (as var. *luteus*, etc.). But a study of monographs will reveal the fact that Allurus is not a simple species, and when the subject has been more carefully studied its lessons will be very instructive. On the one hand we find that a number of pigmy species are found in the Swiss Alps, while *A. hercynius* Mich. has once been found in Scotland, *A. tetragonurus* Friend at Bangor in Wales, and *A. macrurus* Friend at Malahide, near Dublin. Following out these hints, we conclude that *A. tetrædrus* is dominant, and that the allies have been forced into outlying districts, where a careful search would probably be rewarded by the discovery of other interesting forms. If the West of England, Wales, and Scotland were explored with care it might be possible to gain much light on some of the problems which such facts as these suggest.

Again we have one record only for an alpine species of Lumbricid (*Eisenia alpina* Rosa), although we certainly ought to find others in the highlands of Scotland if not in other localities. I shall have occasion under another heading to speak of certain garden worms found in various parts of the country, but it will be well to observe here that one worm (*Octolasium intermedium* Friend) has hitherto been found in Oxford Botanic Gardens only, *Dendrobœna merciensis* Friend only in leaf mould in Derbyshire, *Helodrilus elongatus* Friend (a species which has not yet been

described) in a garden in Cornwall, to say nothing of certain more or less well-known species which occur in Kew Gardens. During the spring of the present year *Allolobophora antipæ* Mich. was found by me at Blenheim Palace, *A. norvegica* Eisen and possibly other species new to Britain being discovered about the same time in Dublin. All these have a bionomic value which is unique, and suggest the need of a much more systematic examination than has ever yet been undertaken.

Having referred in the foregoing section to those species which are of rare occurrence or limited range, it may be well to add that a certain number of species are everywhere to be met with. *Lumbricus terrestris* L. and *Allolobophora longa* Ude are the dominant types. *L. rubellus* Hoff. and *L. castaneus* Savigny abound in meadows ; *L. festivus* Sav. being less common. *A. chlorotica* is always to be found in damp places, under stones, and near the haunts of cattle, where *A. caliginosa* (which includes *turgida* and *trapezoides*) is also frequently discovered. The brandling and gilt-tail, to be mentioned again later, are ubiquitous, the curious tree worms are fairly common in old tree trunks, and in road scrapings one is pretty sure to meet with *D. mammalis*. In gardens and fields one finds two species of Octolasium pretty generally distributed, and *E. rosea* is another of the widely known species. Having just completed a report on the distribution of earthworms in England I may refer the interested reader to the pages of the *Zoologist* for further details.

Habits and Habitats.—We may naturally pass on to a little fuller study of some details in the life-history of our indigenous earthworms. Is it possible to tell where certain species may be found ? Can one judge by the locality what species are likely to occur ? The answer is in the affirmative. Thus if one sees a decaying tree trunk in a moist condition he may be pretty certain that he will not look in vain for such species as *D. arborea*, *D. subrubicunda*, *L. castaneus*, *B. eiseni*, and somewhat rarely *D. octædra*. Several of these also occur in leaf mould, along with *D. merciensis*, *L. rubellus*, and *Eisenia rosea, veneta* or *fœtida*. The latter (*E. fœtida* Sav.), which is popularly known as the Brandling, is the first to attach itself to stable manure. It will thrive in such material long before any other species can find a subsistence in the strong pungent mass. When decomposition has set in, however, *L. terrestris*, *L. rubellus*, and *D. subrubicunda* will become common, along with large quantities of *Enchytræus*

albidus Henle. Later still one finds *A. chlorotica*, *A. caliginosa*, *E. rosea* and other forms. Ditches are frequented by *Allurus tetrædrus*, *A. chlorotica*, *D. subrubicunda*, *D. merciensis* and *O. gracile*. And here it may be remarked that the other species of Octolasium found in England rarely occur in such situations, but prefer gardens and ploughed fields. Another difference will be indicated hereafter.

In many parts of the country it is the custom for the roadmen to place their sweepings and scrapings in heaps either by the roadside or in a field or waste plot. For a time no signs of life will be found here; then various Fridericias and other Enchytræids begin to abound, and with these one will nearly always find such earthworms as *B. eiscni*, *B. constrictus*, *L. castaneus*, *E. rosea*, and *D. mammalis*. If a fork is inserted in the soil of pastures and worked to and fro, *L. castaneus*, *L. rubellus*, and *L. festivus* may readily be obtained. In some places the same means will be successful in bringing out *A. longa*, *A. caliginosa*, *E. rosea*, and one or two others. It thus appears that a certain number of species have well-defined habitats and definite habits, such forms as Allurus never being taken save where moisture is found, and the Octolasiums being found either in ditches (*O. gracile*) or in gardens and fields. Nearly all our native species love moisture, but they frequently perish in great numbers in times of continued flood.

Slime and Mucus.—One has not to study the Lumbricidæ long before becoming aware of great differences in relation to the matter which is given off under irritation. All our earthworms are provided with dorsal pores, and from these we frequently find an exudation of one kind or another. In the case of the different species of Lumbricus there is a watery discharge quite distinct from the slime which is one of their chief characteristics. This fluid is best seen when the worms are partially dried. They seem then to pour it out from the dorsal region with a view to moistening their surroundings and thus making progress possible. It must be observed that the native Lumbrici (of which we have four species in England, and a fifth in Ireland) never give off a coloured or fœtid liquid. In this respect Allurus, *B. eiseni*, *A. longa* and one or two other Allolobophoras are in agreement with the Lumbrici. With reference to the Allolobophoras (including therein Allolobophora, Octolasium, Aporrectodea, Dendrobæna and other genera) there is a great deal of diversity

in the matter of secretion. Some exude it from the entire length of the body, others from the head or tail, or from special segments. Nor is the appearance and smell the same in the different cases. Let us examine a few of the principal.

In the Brandling (*Eisenia fœtida* Sav.) we find a very profuse exudation of a yellow colour and pungent odour from almost the entire length of the body. To some the smell resembles garlic, to others the liquor from boiled cabbage. It leaves a good deal of powdery matter behind when dry, but I am not able to recall any memoir dealing with its chemical constituents. Next to it, so far as volume of output goes, we may place *A. chlorotica* Sav., often known as the green worm. It is very sluggish as a rule, and one would suppose the secretion serves to keep off enemies. It is similar in colour to the last, and may be poured off from any part of the body. *Eisenia rosea* Sav. and *Eophila icterica* Mich. also act in a similar way, but the fluid, particularly in the case of the former (which was once known as *Allo. mucosa*), leaves a white chalky sediment. *D. subrubicunda* has a yellowish tail, and it frequently happens that a large quantity of gold-coloured secretion exudes therefrom. Then from *O. cyaneum* and *O. profugum* a yellow exudation may be obtained from the region of the sexual organs and from the caudal segments. Thus, without giving further details, it is very clear that much variety prevails, and it seems very desirable that a careful study of the subject should be undertaken with a view to determining the exact nature and composition of the various kinds of fluid, and the exact purposes for which the fluid exists. The slime seems to be almost purely lubricative, the white and yellow fluids preservative.

Helodrilus oculatus Hoffmeister.—As illustrating some of the problems in bionomics which the study of the Oligochæts raises, it may be well to take one particular species; and I select for the purpose *H. oculatus*. The name is well chosen. Helodrilus means the worm found by low marshy ground (ἕλος) on the sides of rivers, while *oculatus* refers to the presence at certain periods of a couple of eye-spots. This is, I believe, the only species of Lumbricidæ in which eye-spots have been discovered, and is of interest because such spots are not un-known in Naididæ on the one hand and Polychæta on the other. Helodrilus was first described by Hoffmeister in 1843. No adult was known, and the description was therefore incomplete;

and for many years it was lost to sight. It was rediscovered in 1890, but as the connection was not then recognised Michaelsen named it *Allolobophora hermanni.* In 1896 de Ribaucourt gave a full description of it as found by him in company with *Lumbricus michaelseni* in extremely humid soil. He remarks that by its form and manner of life it appears to be a link between the terricolous and the limicolous species. But as yet the connection between the two had not been suspected. Rosa, in 1893, had given Michaelsen's *A. hermanni* place in his *Revisione,* but does not allude to Helodrilus, and in 1895 Beddard has the following note: " *H. oculatus* Hoffm.: This is an extremely mysterious species, neglected by Rosa in his recent revision of the Lumbricidæ, and therefore probably not believed by him to be a Lumbricid. Its most remarkable structural peculiarity is a pair of eye-spots on the buccal segment. There are four pairs of setæ in each segment, which are straight instead of curved, and said to be black ; the male pores are upon the fifteenth segment. The body is elongate and pink in colour; the length at most 135 mm. It occurs on the seashore in pools more or less dried up." Beddard adds that " Vaillant suggests that this worm is probably a Tubificid, on account of the presence of eye-spots, and because of its habitat. The black setæ are very suggestive of what I have myself observed in *Tubifex rivulorum.* But it does not seem to me that we are justified in relegating the genus to any family at present."

When, in 1900, *Das Tierreich : Oligochæta* appeared, Michaelsen put the matter right. He showed that *H. oculatus* Hoffm. and *Allolobophora hermanni* were one and the same, and gave Germany, Switzerland, and Italy as its distribution. In the course of time England was added to the list of habitats. As I was exploring the pond in the Cambridge Botanic Garden in July 1907, I found several adult specimens of the worm, and sent an account of it to the *Gardeners' Chronicle* some time later. Next it was found by Mr. Evans near Edinburgh, and at the same time I found the immature forms at Malvern, with the eye-spots distinctly visible. But though I kept it under observation for two years, I was never able to find an adult. During the past three years I have taken *H. oculatus* from mud on the banks of the Thames at Kew, near the sea at Hastings, by the dykes in Pevensey Marsh, by streams and ditches in Derbyshire

and Notts, by the Dodder at Ballsbridge, Dublin, and by the stream at Swords; and have received it from Epping Forest. The forms at Kew were large, with correspondingly large cocoons, while those at Malvern were small with small cocoons. It is in many ways a most curious worm, and seems, like *O. gracile*, to be gradually working westward.

Constancy and Variation.—This reference to the two forms of *H. oculatus* Hoffm. leads me naturally to some remarks on the tendency to change in some worms, and the evidences of stability in others. The most stable English worms are the four species of Lumbricus and the three species of Octolasium. Out of the thousands of specimens which I have examined during the past quarter of a century, it has rarely been my lot to see any varieties of either. Some years ago I recorded a short-tailed form of Lumbricus for Calverley near Leeds, and some Continental writers affirm that the girdle of *L. terrestris* extends over more than six segments, but I have never seen a single case of this kind in England.

It might here be remarked that normally the girdle in the genus Lumbricus extends over six segments, while the tubercula pubertatis occur as a band on the innermost four. Further, there is a regular gradation in the matter which is peculiarly interesting. This may be shown by the following chart, in which the figures show the segments covered by the tubercula:

1. L. rubellus Hoffm.   28, 29, 30, 31.
2. L. castaneus Sav..   29, 30, 31, 32.
3. L. melibœus Rosa   30, 31, 32, 33.
4. L. tyrtæus? .   .   31, 32, 33, 34.
5. L. studeri de R. .   .   32, 33, 34, 35.
6. L. terrestris L. .   .   33, 34, 35, 36.
7. L. papillosus Friend .   34, 35, 36, 37.
8. L. festivus Sav. (= rubescens Friend)   35, 36, 37, 38.

No. 4 is doubtful, but in view of the regularity here displayed it seems impossible to believe that there is not a true form to fit the niche. But while the tubercula are constant it is curious to observe that the girdle is variable in one or two instances, and these become instructive accordingly. Why is it, for example, that the Irish worm *L. papillosus* has only five girdle segments instead of six, and has a pair of large papillæ on each side? *L. melibœus* similarly has only five girdle segments.

The three species of Octolasium found in England are like

the Lumbrici in this respect: they each have six girdle seg-
ments; but while two of them have the tubercula extending
over the four innermost girdle segments, the third (*O. gracile*)
has the band along the whole six. Along with this peculiarity
we have also a difference of colour, habit, and habitat. *Octolasium
gracile* Oerley is somewhat flesh-coloured, emits no turbid
fluid, and is found in wet places; while *O. cyaneum* and
O. lacteum have steel-blue bodies, clay-coloured girdles, and
yellow tails, from which coloured fluid exudes, and are found
in gardens and fields, chiefly in ground which is under
cultivation.

Among the Allolobophoras the most constant seems to be
A. longa, which shares with *L. terrestris* the dominancy among
British Earthworms. The two are readily distinguishable by
the position of the girdle, the colour, and the shape of the
prostomium, but were until quite recently constantly mistaken
the one for the other. In the case of almost all the other
species of Allolobophora variation constantly occurs. Thus
A. caliginosa has two forms, which are sometimes so well
marked that they might pass for different species; hence the
name *turgida* applied to one, and that of *trapezoides* to the other.
The green worm is exceedingly variable. Sometimes it is an
intense green and very sluggish, so that it might be mistaken
for a grub. At other times (*forma cambrica* Friend) it is just as
active, and has a colour resembling that of *caliginosa*. The
mucous worm (*Eisenia rosea = mucosa*) has well-marked varieties,
one of which (*macedonica*) occurs in England and on the
Continent, and might almost pass for a subspecies at times.
So among the Dendrobenes we have *subrubicunda* and *arborea*,
which have similar peculiarities to those found in the foregoing
species; and while at times they are perfectly distinct, at other
times it is impossible for an expert to say whether a given
specimen is truly one or the other. If any one wishes to pursue
this subject further he will find that Michaelsen, Rosa, Beddard,
Eisen, Cognetti, De Ribaucourt, Vejdovsky, and others abound
in illustrations and supply abundant material for the most
critical biologist.

Allusion was made above to the genus Allurus, and a further
reference may be permitted under this heading. In July of last
year (1912), while I was collecting at Hastings, I had the good
fortune to find quite a number of Oligochæts which were either

new to science or to Britain. Among these was a fragile creature flourishing in alga at Ecclesbourne, near where the little stream falls into the sea. About a dozen specimens were collected and taken home for examination. These, however, perished almost immediately, before I was able to prepare a description. It was necessary, therefore, to get a fresh supply if possible, and preserve them forthwith. This was done, and notes were taken both of the living and the preserved forms. In no case was an adult specimen to be found, and for the present one is obliged to speak cautiously; but the evidence clearly pointed to a new species of British Oligochæta, and the creature has been named provisionally *Allurus mollis.* Just as the dominant type has driven some species to the Alps and others to the borderlands of Wales and Ireland, so it is possible that in this case a tender form has been compelled to find refuge in algæ, to take to the boats indeed, just as the Tanka people on the Chinese rivers have done in escaping from the oncoming Celestials of more robust and over-mastering character.

As a final illustration of the extent to which variation may run (without alluding to internal structure and the work of Woodward, Bateson, and others), one may take that most polymorphic of all Allolobophoras, *Eisenia veneta* Rosa. Its history is one of great interest, and may be read in the pages of Rosa and in my own contributions to annelid study. I first found it many years ago in Dr. Scharff's garden, Dublin, and named it *A. hibernica,* not knowing that it had also been found in Venice. In March of this year I found it again in Dublin, in a neighbouring locality. After the lapse of some years a second British form turned up at Oxford, which I named *Tepidaria.* This has not yet been found elsewhere, so far as I am aware; but it is a striking variety. I failed to obtain it again during a recent visit to the Oxford Botanic Garden. In 1909, while collecting in some gardens at Malvern, I came across two new forms, one of which was very robust (*E. robusta* Friend), while the other was like a dendrobene (*E. dendroida* Friend). A variety found in Cornwall has not yet been named, but Southern has taken a further form in Ireland which is similar to Michaelsen's variety *zebra,* and yet another variety is named *hortensis.* It is such facts as these which make the study of our Earthworms full of interest to the biologist. They are but samples of the kind of material which an extended investigation has enabled one to

bring together; and the examination of our Enchytræids and other Oligochæts supplies us with further material of an equally instructive character.

List of British Earthworms.—At last the Lumbricidæ of Great Britain have been fairly well investigated, and the reproach that they "have never been carefully monographed" may be wiped away. Southern and I have done our best to make the list complete, and although we shall probably be able in time to make a few further additions, when the gardens connected with our old mansions have been explored, and the highlands and islands have been investigated, yet we cannot hope to find many new species. The following list will be of service for future workers, and supplies sufficient information for working purposes.

Allurus (Eisen) = Eiseniella (Michaelsen)

1. A. tetrædrus Sav.   Dominant.   Very widely distributed.
    Var. luteus Friend.   Carlisle, Calverley, Newark, and elsewhere.
2. A. tetragonurus Friend.   Bangor in Wales.
3. A. macrurus Friend.   Malahide, near Dublin.
4. A. hercynius Michaelsen.   Scotland.
5. A. mollis Friend.   Hastings and Burton Joyce.

Eisenia (Malm. em. Michaelsen)

6. E. fœtida Savigny.   Everywhere in manure and rich soil.
7. E. veneta Rosa.   Represented by the varieties named.
    Var. hibernica Friend.   Dublin.
    Var. tepidaria Friend.   Oxford Botanic Garden.
    Var. robusta Friend.   Gardens at Malvern.
    Var. dendroida Friend.   Gardens at Malvern.
    Var. zebra Michaelsen.   Ireland.
    Var. unnamed.   Gardens in Cornwall.
8. E. alpina Rosa.   Perthshire, Scotland.
9. E. rosea Sav.   Widely distributed.
    Var. macedonica Rosa.   In gardens: Kew, Chelsea.
    Var. unnamed.   Cambridge Botanic Garden.

Allolobophora (Eisen em. Rosa)

10. A. georgii Michaelsen.   Valencia, Ireland.
11. A. caliginosa Sav.   Widely spread.   Two forms:
    Var. turgida Eisen.   Common.
    Var. trapezoides Dugès.   Common.
12. A. longa Ude.   Everywhere dominant.
13. A. relictus Southern.   Clare Island, Ireland.

Aporrectodea (Oerley)

14. A. chlorotica Sav. Very widely distributed.
    Var. cambrica Friend. Wales, Cambridge.
15. A. similis Friend. Kew Gardens.

Dendrobæna (Eisen em. Rosa)

16. D. rubidus Sav. Under two forms:
    Var. subrubicunda Eisen. Very widely spread.
    Var. arborea Eisen. In decaying tree-trunks.
17. D. mammalis Sav. Frequent in road scrapings, etc.
18. D. merciensis Friend. Derbyshire, England.
19. D. octædra Sav. Local and somewhat rare.
20. D. submontana Vejd. Kew Gardens.

Helodrilus (Hoffm. em. Mich.)

21. H. oculatus Hoffm. Sussex, Surrey, Essex, Notts, Derby-
    shire; also Dublin and Swords, in Ireland; Scotland.
22. H. ictericus Sav. Kew, Chelsea, Cambridge, etc.
23. H. elongatus Friend. Pencarrow, Cornwall.

Bimastus (Moore)

24. B. beddardi Mich. Ireland.
25. B. eiseni Levinsen. England, Ireland, Wales, Isle of Man,
    and Scotland.
26. B. constrictus Rosa. Not very common, but somewhat
    widely distributed.

Octolasium (Oerley em. Rosa)

27. O. cyaneum Sav. In cultivated ground.
28. O. lacteum Oerley (= profugum Rosa). Pretty generally
    distributed, in cultivated ground.
29. O. gracile Oerley. In ditches and wet places, chiefly in the
    East of England.
30. O. intermedium Friend. Oxford Botanic Garden.
31. O. rubidum Oerley. Reported by the discoverer as found at
    Woolwich, but not confirmed hitherto.

Genus not yet Determined

32. Allolobophora antipæ Mich. Blenheim Palace, 1913.
33. Allolobophora norvegica Eisen. Dublin, March 1913.
34. Allolobophora (doubtful). Dublin, March 1913.
35. Allolobophora (doubtful). Dublin, March 1913.

LUMBRICUS (Linnæus em. Eisen)

36. L. rubellus Hoffm.   Universally distributed in Britain.
37. L. castaneus Sav.   Similar distribution to last.
38. L. festivus Sav.   Less common than the foregoing.
39. L. papillosus Friend (= L. friendi Cognetti). South of Ireland.
40. L. terrestris Linn.   Widely distributed.

This list shows a total of forty species, with about a dozen forms and varieties, some of which have been given specific rank by one or other of our leading authorities. I have pleasure in gratefully acknowledging a grant from the Government, through the courtesy of the Royal Society, to enable me to carry out this research into Annelid Bionomics and Economics.

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ENZYMES AS SYNTHETIC AGENTS

I. IN CARBOHYDRATE METABOLISM

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INTRODUCTION

In the present state of our knowledge, the constructive syntheses in the plant that precede the formation of the protoplasmic complex, present a peculiarly difficult problem.

The activity of organic chemistry has brought to light so many possible compounds and reactions that may form links in the numerous syntheses required, that it is difficult for the biologist to decide what lines best admit of experimental attack. In this quandary it is very desirable that some thread of guidance should be obtained through the labyrinth of possibilities, and such a thread is perhaps provided in the idea that the plant may employ enzymes as catalysts to such synthetic chemical reactions. As the number of available enzymes present in an organism is presumably limited and as their powers as a rule seem strictly limited, this narrows the field of inquiry in relation to metabolic synthesis, and it is perhaps worth while considering what light is thrown upon the problem when it is considered from this standpoint.

Since Croft Hill first announced the synthesis of maltose by the use of the maltase (glucase) extracted from yeast, a number of investigators have experimentally attempted to use enzymes as catalysts to synthetic reactions. The idea underlying these experiments is simple.

Most of the reactions catalysed by enzymes are of a reversible nature, as is indicated by the way in which the reactions gradually slow up and ultimately come to an equilibrium point if the products of the reaction are allowed to accumulate. Thus if a reaction of the general type be expressed by the formula $A + B \rightleftarrows C + D$, then the arrows indicate that at any time this reaction is going in either direction and the resultant effect of

these dual reactions depends upon the extent to which either
A + B or C + D are present in excess of equilibrium concentra-
tion. If A + B are present in excess of equilibrum concentration,
then the reaction will be proceeding more rapidly in the direction
from left to right, and this will continue to be the case until so
much C + D has been formed that the reverse conversion
C + D → A + B is going on as rapidly as the conversion A + B
into C + D. This is the equilibrium point of the reaction and,
for a definite reaction, at a definite temperature, is a quite
definite point that can be expressed in terms of the concentration
of the reacting bodies.

Now if an enzyme behaves as an ordinary catalyst its
addition should make no difference to the position of this
equilibrum, but only shorten the time in which this equilibrium
point is attained. In such a reaction as

$$C_{12}H_{22}O_{11} + H_2O \rightleftarrows 2C_6H_{12}O_6$$
$$\text{(maltose)} \qquad\qquad \text{(glucose)}$$
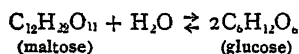
if the reaction proceeds from right to left it will be of a synthetic
nature. Realising this, Croft Hill attempted to obtain con-
centration conditions such that the reaction should tend to go
from right to left to attain equilibrium, and in this way managed
with the use of an enzyme catalyst to synthesise maltose. So
far, then, experiment seems to be in agreement with theory, but
a closer acquaintance with the literature suggests a number of
fresh problems of great importance to the biologist.

These it is proposed to consider briefly and by no means
exhaustively in so far as they touch the two main types of
synthesis with which the biologist is particularly concerned,
viz. carbohydrate synthesis and protein synthesis.

Synthesis of Carbohydrates

It is possible that in the many problems that this subject
presents, the study of reversible chemical action as catalysed by
enzymes offers us the best experimental method of attack under
" in vitro " conditions because it may thus be possible to realise
the essential conditions in regard to stereo-isomerism. Emil
Fischer,[1] in his Faraday lecture to the Chemical Society, referring

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[1] " Synthetical Chemistry in its Relation to Biology," *Transactions of Chemical Society*, 1907, vol. 91.

to the attempts that had been made to synthesise sugars from carbon dioxide and water, pointed out that in addition to the small yields obtained by these chemical methods they also failed to realise the condition of producing only optically active sugars. Since then in more recent experiments (Stoklasa, Sebor and Zdobnicky [1]) the yields have been improved by the use of the ultra-violet rays of the quartz mercury vapour lamp, but the difficulty of producing the right optically active sugar still remains. All the naturally occurring sugars in the plants are optically active, having different powers of rotating the plane of polarised light, and all are what are termed *d* forms, that is of the same general type of constitution as the sugar that Fischer has termed *d*-glucose. The difference in the power of rotating polarised light is traced to the different arrangement of the asymmetric carbon atoms within the isomeric sugars. The problem then is to produce in vitro not only a sugar but the sugar with the natural arrangement of the asymmetric carbon atoms, not merely an isomer of this sugar but the correct *stereo-*isomer, as it is called.

Enzymes, themselves probably asymmetric organic bodies, are in most cases extremely restricted in reference to the reactions they can accelerate and can usually only react with a certain class of stereo-isomer. This fact, which is of great biological significance, is probably to be traced to the method in which they produce their accelerating effect; they are usually regarded as combining with the reacting substances, and if these are asymmetric, then in all probability this temporary combination is facilitated by their own asymmetric constitution. The same fact should hold good in relation to their activities in synthesis, and they should therefore produce optically active bodies instead of inactive mixtures containing equal quantities of both stereo-isomers. They therefore provide a possible agent by which this necessary asymmetry should be introduced in the course of the process of synthesis known as photosynthesis. The starting-point for this synthesis is, of course, carbon dioxide, but when the substance has diffused into the chloroplast the next substance in the transition to carbohydrate is still a matter for speculation.

Considerable, but not conclusive, evidence has accumulated that formaldehyde is produced within the plant, and the passage

[1] *Biochem. Zeitschr.* 1912, vol. 41, p. 333.

from formaldehyde to a glucose is then a step which can be produced in the test tube by the use, for instance, of various inorganic reagents such as calcium hydrate.[1]   But in some very important papers[2] in which the evidence to be obtained from the distribution of sugars within the leaf is considered, the conclusion is reached that the first sugar in the series of up-grade sugars is the di-saccharose cane sugar, a conclusion which is more difficult to reconcile with the statement that formaldehyde is the first detectable compound in the transition from carbon dioxide.

Considering the question from our present specialised viewpoint, light may be thrown on the contradiction if we consider that the series of sugar transitions are probably reversible reactions and attempt to obtain light upon the up-grade series by considering the well-established steps in the hydrolysis of the starch molecules with the aid of enzymes as it occurs under *in vitro* conditions.

The stages in the process are represented in the following scheme :

$$\text{Starch} \xrightarrow{\text{(by diastase [amylase])}} \text{dextrin}$$

$$\text{dextrin} \xrightarrow{\text{(by diastase [dextrinase])}} \text{maltose}$$

$$\text{maltose} \xrightarrow{\text{(by maltase)}} \text{glucose}$$
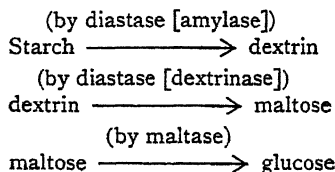
It will be seen that cane sugar does not figure in this series at all; cane sugar, a di-saccharose, is itself broken down by the action of sucrase (or invertase) into the mono-saccharoses glucose and fructose.   Beyond *d*-glucose the catalytic reactions by which the sugar is split up into simpler molecules are still unknown owing to the difficulty in carrying out the process away from the plant tissues.   Glucose can be split up into carbon dioxide and water, it is true, by the action of three purely inorganic catalysts acting in series,[3] but this affords no proof that the reactions in the plant proceed in the same manner.

Zymase will give alcohol and carbon dioxide when in contact

[1] Fischer, *loc. cit.*, p. 3.

[2] Brown and Morris, "A Contribution to the Chemistry and Physiology of Foliage Leaves," *J. Chem. Soc.*, 1893, **63**, p. 604; Parkin, *Biochemical Journal*, vol. vi. p. 1.

[3] See Euler, *General Chemistry of Enzymes*, Eng. trans. by Pope (pub. Wiley & Sons), p. 52.

with glucose, but the intermediate stages in what is undoubtedly a complex process are still in dispute,[1] and in any case zymase is not at present regarded as an important factor in the decomposition of sugar in the aerobic tissues of the plant, though it apparently occurs in the higher plants and especially in massive ill-aerated tissues. It is to oxidases that the catalysis of the sugar in the aerobic tissues is generally ascribed, and as the details of this process have never, I think, been followed in vitro, stages in this return from sugar to carbon dioxide and water are still quite obscure.

This being so, we can only suggest from our present standpoint that if formaldehyde be the first formed product, a *d*-glucose would be the first sugar likely to be formed, and we may now proceed to consider whether any light is thrown upon the next step, if it is considered as a condensation of two molecules of dextrose to give maltose, the process being accelerated by the enzyme maltase.

I fear that in the present state of the literature of the subject our conclusion will be that though the idea may be suggestive, the subject is too full of contradictions to enable one to reach any hypothesis with a satisfactory decisiveness.

It was previously pointed out that Croft Hill described the synthesis of maltose from glucose by the aid of the enzymes of an extract of yeast which contained considerable quantities of maltase. But a difficulty arose when it was subsequently pointed out, and the statement confirmed later by E. F. Armstrong, that the di-saccharose formed was an isomer of maltose and termed iso-maltose. This point has since become of considerable importance as the actions of enzymes have been more fully investigated and their properties become more strictly defined.

It is realised that the molecule glucose, containing several asymmetric carbon atoms, can exist in a large number of isomeric forms, and that moreover the dextro-isomer, *d*-glucose can itself exist in two stereo-isomeric forms which can pass over into one another through an intermediate modification.[2]

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[1] For a review of recent literature, see Harden, "Alcoholic Fermentation," *Monographs on Biochemistry.*

[2] More probably a stable equilibrium point exists between the two forms when in solution (Lowry). For a clear account of these problems of sugar constitution, see E. F. Armstrong, "The Simpler Carbohydrates and the Glucosides," *Monographs on Biochemistry.*

These two forms, the a and the β, will give recognisably different glucose compounds, the a and β glucosides, and maltose is such a glucose compound, maltose itself being the a-glucose-glucoside, iso-maltose the β-glucose-glucoside.

Translated into terms of this nomenclature, the maltose synthesised in Croft Hill's experiments was the β-maltose, and it was presumably synthesised through the agency of the maltase present in the yeast extract.

But if the matter be tested, maltase will be found to be without action upon the β-maltose, and will only hydrolyse the a-maltose, the substance formed during the hydrolysis of starch.

This is accepted as a statement of the facts by some writers,[1] and it is regarded as marking a distinction between the ordinary catalyst and the behaviour of the enzyme catalyst.

But such a distinction is so vital, and renders the whole interpretation of enzyme action so uncertain if accepted, that any alternative explanations need serious consideration. Bayliss,[2] while pointing out the obvious difficulty that if the enzyme is synthesising a sugar it is incapable of hydrolysing, the equilibrium point of the reaction must be affected, indeed abolished, suggests that another possible explanation is that the synthesis of β-maltose may have been due to the presence of another enzyme. Yeast extract would certainly contain many enzymes, and in some yeasts Henry and Auld have detected appreciable quantities of emulsin. Emulsin, the enzyme usually associated with the breaking down of the glucoside amygdalin, is capable of attacking β-glucosides, indeed amygdalin itself is really a β-glucose-glucoside, from which the emulsin (or the amygdalase portion of it, it is really again a group of enzymes that is included under this name [3]) splits off one molecule of glucose, leaving the mandelo-nitrite glucoside to be still further broken down. If then the yeast extract contained emulsin, this might be expected, in the presence of excess of glucose, to synthesise the β-maltose.

The difficulty in the way of accepting this explanation lies in the fact that it is difficult to explain the preponderance of the β synthetic compound, bearing in mind the relative preponderance

[1] See for instance, Abderhalden, *Physiological Chemistry*, Trans. Hall, p. 481 (1908).

[2] Bayliss, "The Nature of Enzyme Action," *Monographs on Biochemistry*.

[3] For review of recent literature, see Euler, *loc. cit.*, p. 23.

of maltase in the yeast extract which experience of yeast extracts would lead investigators to expect.

As E. F. Armstrong[1] has also shown that emulsin synthesises the a-maltose, again the opposite form to the one it attacks, the difficulty is here complete, and needs apparently to be worked out upon the line of these suggestions. But unless the difficulties can be traced to impurities in the enzyme preparations it seems that whatever suggestion is made to get over the difficulty must involve a new interpretation of the nature of an enzyme as an organic catalyst.[2] We need not yet give up the hope of seeing the knot unravelled upon the lines of the simpler interpretation of enzyme nature, as Bourquelot and Bridel[3] have recently announced the synthesis of β-methyl-glucoside from an alcoholic solution of glucose by the aid of emulsin—a fact that suggests a normal behaviour for this enzyme at any rate under certain circumstances. In later papers they attribute this synthetic activity to a lactase present in the extract of emulsin.[4]

With this discussion of the present state of our knowledge of the transition from glucose to maltose in vitro we may briefly consider the process in the tissues of the leaf. Here we are at once met with the surprising difficulty that maltase has not been described as usually present in the tissues of the leaf. This is astonishing in view of the nightly conversion of starch into maltose, and presumably the further change of some of the maltose into mono-saccharose sugars, although carbohydrates may apparently leave the leaf as maltose.[5] The absence of reports as to its occurrence may be due to difficulties in the way of extraction. Students working with me have on one or two occasions obtained indications of hydrolysis of maltose when studying the enzymic activity of extracts of dried and powdered leaves, but certainly such activity is often not recognisable. The point seems well worthy of further investigation, especially as the curious facts as to the distribution of storage carbohydrates in leaves may possibly find some explanation in

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[1] E. F. Armstrong, *loc. cit.*, p. 75 (1st ed.).

[2] For instance, the suggestion of the existence and synthetic activity of anti-enzymes. See Euler, *loc. cit.*, p. 266.

[3] *Compte Rendus*, 1912, t. 155, p. 319.

[4] See, for instance, *Comptes Rendus*, 1912, 155, p. 1552. Synthesis of a-glucosides by another enzyme have now also been recorded. See *Comptes Rendus*, 1913, 156, pp. 168, 491 and 1493.

[5] See Mangham, SCIENCE PROGRESS, New Series, Nos. 18 and 19.

this direction. In leaves such as the snowdrop, where cane sugar seems to be stored to the complete exclusion of starch,[1] the enzyme disastase is yet present, and leaf extracts exert a rapid hydrolytic action on starch. No maltase however can be extracted, and possibly in the absence of this enzyme no maltose can be formed,[2] and therefore no starch. In cases where maltose is presumably freely formed, that is, on this view, in all cases where starch is subsequently formed, it is difficult to know at present whether the often reported presence of emulsin in such leaves may or may not have significance.

From starch to maltose the down-grade stages are by no means clear. As was suggested in the scheme given earlier, the process probably takes place in two main stages, associated with different enzymes or more probably groups of enzymes. At present it is perhaps only worth pointing out that the statements in the older literature[3] as to a portion of the starch molecule incapable of complete hydrolysis, arose from a mistaken interpretation of an equilibrium point which is very definitely obtained in the hydrolysis of dextrin to maltose.[4]

It is not unnatural that it should have proved impossible as yet to form starch granules by merely reversing the enzyme mechanism in vitro, seeing that the process in the plant is apparently so complicated that it never occurs but in association with a controlling plastid. Everything points to a complicated process involving the use of a series of enzymes under close protoplasmic control, and presumably held to definite places in the internal surfaces of the solid phase of the granule—indeed, so carefully controlled apparently that they are not liberated in death, so that I do not think it has ever been found possible to detect appreciable disappearance of starch from the plastid after death produced by chloroform or other anæsthetic, although the diastatic activity of an aqueous extract of such a leaf seems to be fully adequate to the hydrolysis of the amount of starch present.[5]

In view of these facts one has to interpret very tentatively

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[1] Parkin, *loc. cit.*

[2] The statements as to the distribution of enzymes in the snowdrop leaf are based on work done in this laboratory, but not yet published.

[3] See for instance, Reynolds Green, *Fermentation.*

[4] Bayliss, *loc. cit.*, Chap. VI., p. 55 (1st ed.).

[5] See Brown and Morris, *loc. cit.*, p. 651, discussion of Wortman's results.

such statements as those of Fernbach and Wolff[1] as to the existence of a coagulating diastase, and to suspend judgment upon statements as to the production of starch from sugars within the cell upon concentration of the sap by plasmolysis.[2]

Possibly light may be thrown upon the question by the similar but perhaps simpler problem of the synthesis of glycogen,[3] upon which Cremer and others have conducted investigations.

While progress may be slow, recent work on the chemical constitution of starch seems to hold out much hope, in suggesting that the molecules of the substance are perhaps more simply constituted than one has dared to hope;[4] in this case their ultimate synthesis will be an experimental problem admitting more readily of the construction of the hypotheses which lead to the laboratory.

(*Note.*—If it proves possible to utilise physical methods on a sufficiently large scale, new methods may possibly be provided to the physiologist enabling him gently to break up his unwieldy molecules into more recognisable constituents. Ultra-violet radiation seems likely to be largely employed as a tool in such investigations; see for instance the recent investigations of Berthelot and Gaudechon[5] and many others. Professor Bragg, in drawing my attention to recent work on these lines, in which X-rays were used,[6] suggested to me that in these cases we may have in a large molecule more than one collision resulting from the passage of the β-particle through its constituent atoms; there will then result two or more charges of the same sign upon the molecule, and inevitably disturbance of the distribution of its surface energy will follow, probably accompanied by the disruption of the molecule.[7])

[1] *Comptes Rendus*, 1903, **137**, p. 718.
[2] Overton, *Vierteljahrsschr. d. natur. Ges in Zurich*, 1899, **44**, p. 88.
[3] *Chem. Ber.*, 1899, **32**, p. 2062.
[4] See note in SCIENCE PROGRESS, October 1912, referring to recent work of Pringsheim.
[5] *Comptes Rendus.* See also Bierry, Henri, and Rane, *Comptes Rendus*, **151**, p. 316, etc.
[6] Colwell and Russ, *Nature*, vol. 90, p. 531.
[7] See also Bragg, *Nature*, vol. 90, p. 531.

SCIENTIFIC NATIONAL DEFENCE

By COLONEL CHARLES ROSS, D.S.O.

Author of " Representative Government and War "

THE National Defence problem has, of late years, obtruded itself with no little force on the attention of the surprised and indignant British Citizen. Since the downfall of the great Napoleon he has come to regard himself as perfectly secure in his island home. Guarded by his unassailable fleet and the jealousies of continental powers, he has been able to devote himself to problems of internal politics, to colonisation, commerce, and sport. From time to time the sudden advent of hostilities in some far-distant colony, a royal review at Aldershot, or the outbreak of war between foreign powers, has recalled to his mind that he possesses an army, in which, however, he has never taken any very great or intelligent interest. When he comes to think of it, he remembers, with a sense of considerable gratification, that this army enjoys an unrivalled record of past victories in every quarter of the globe. But the British Citizen has always been somewhat hazy as to the reasons for the existence of this army. He supposes that it is really in the nature of an Imperial police force, and cannot quite grasp why it should have interfered in other people's quarrels on the Continent in the times of Napoleon and Marlborough. But that was in the "good old days," when the British people were, probably, rather harebrained; and no one would, of course, venture to suggest that anything of the sort should be done in these days of business and hard common sense.

On the other hand, he was profoundly convinced of the vital importance of the navy. It had always been evident to him that, so long as he held command of the sea, he would be safe from serious attack in his own home; and he had held this sea-supremacy for so many years that he had come to believe that some special dispensation of Providence had placed him in his sea-girt isle in order that he might march securely in

the van of progress and bear the banner of civilisation to the uttermost ends of the earth.

Such had always been his simple creed of national defence.

A partial awakening—so to speak, a yawning and a stretching —occurred in 1899, when he was quite suddenly and unexpectedly attacked by the Boers. To his profound astonishment, not only did the Boers care nothing at all for his navy, but that navy itself proved to be practically helpless. For the moment the citizen was seriously disturbed; he feared that all was not well with a navy which could fail him in his crisis. But he cheered up when he heard that some naval guns had been very cleverly transported to Ladysmith by sailors, on carriages designed by sailors ; and that, at the very first shot—or was it the second shot ?—the matter is unimportant—had struck a Boer gun full on the nose. His navy had retrieved its reputation. Later on, he found that his navy had done him great service; for its overwhelming power had rendered intervention by certain neutral powers impracticable. His army proved to be altogether too small to execute its task ; and he passed through his " black week." But, to his delight, the Empire and the Volunteers rose to the occasion ; money was poured out like water ; recruits were enlisted wherever they could be found ; and, once more, the Briton triumphed.

A further awakening occurred in 1904, when the struggle between the Russians and the Japanese commenced; and the Press teemed with descriptions of bloody and desperate conflicts of a type which the British citizen had thought to be long since obsolete. The savagery of it shocked him. It was an interesting war, because a nation of islanders was fighting for its existence against a powerful continental State. The citizen watched it with keen interest, and with keen sympathy for the islanders. He foretold that they would defeat the continental power on the sea, because they were islanders whose blood was partly composed of ozone, and that the breath of the sea kills all but the hardiest. It may yet be proved that there is a certain substratum of truth in his reasoning, or instinct. He was inclined to regard himself as something of a prophet when his forecast came true. He was somewhat astonished, however, when the islanders, not content with having defeated their enemy on the sea, proceeded to disembark large armies on the mainland and attack the Russian armies. They beat the conti-

nentals—that goes without saying—because they were islanders; but were they altogether wise in carrying the war, in this fashion, on to the mainland ? Where was their common sense ? But, after all, they were mere tyros at this sort of thing ; we must all live and learn.

Nevertheless, in spite of his complacency, there lingered a certain doubt in his own infallibility. The Germans had set to work in a very calm and deliberate fashion to construct a fleet. They had expressed the intention of becoming lords of the Atlantic. They had shaken a mailed fist in the air. At the outset he was inclined to regard this exhibition with some amusement. He knew, of course, that the Germans, situated as they were in the midst of possible enemies, were obliged to maintain a vast and very efficient army; and he did not consider it possible that a nation would make the necessary sacrifices to be strong on the sea as well as on the land. But, as time went on, and the German navy steadily increased, his amusement gave place to wonderment, then to gravity, finally to no little consternation. It dawned upon him slowly, very slowly, that a great continental State was about to fly in the face of Providence and actually challenge his sea-supremacy. His consternation was accentuated by the attitude of his Government. The latter, far from accepting the challenge boldly and building ships and recruiting additional men, and all the other things that are necessary to ensure naval supremacy, sought to induce the Germans to change their mind ; with the result, as was only to be foreseen by every man of common sense, that they, believing the British to be afraid of them, built ships more rapidly, and in greater numbers, than before.

The citizen commenced to regard his Government with great contempt. One good had, however, resulted from its action, or lack of action. The Empire, as a whole, had been convinced that the Germans were the aggressors; and the Dominions were displaying a very pronounced inclination to support the Mother Country. The citizen had visions of Canadians and Australians and New Zealanders and even of Boers and Indians marching shoulder to shoulder against the common foe; but whether the march was to take place on the Continent or in his own country he did not stop to consider.

It was about this time that his business called for a rapid visit to Australia. During the long and wearisome voyage he

learned many things. First and foremost he grasped the fact that, while Australia is a very long distance away from England, Germany is very close to it; and that there would be ample time for the German Army, or a small portion of it, to over-run England, before ever a single Australian could reach the country to help in its defence. His visions of an Imperial army marching to victory vanished.

There were several soldiers on board the ship, and the citizen heard many interesting discussions. These men, he found, regarded the subject from a totally different standpoint to his own. Their talk was all of force—the stronger force and the weaker force, and how the latter might hope to beat the former. He had always held the view that the conscripts of the Continent were, in reality, slaves, and that one free-born Briton would be more than a match for any three of them. When, with some diffidence, he suggested this view, a curious silence reigned. Finally, one said that continental armies were not slaves, that they were composed of very fine and well-trained troops, and that they had always fought with the utmost gallantry and devotion. He, the speaker, while fully confident in the capacity of his own men to beat equal numbers of any troops in the world, would be sorry to " take on " three times, or even double, his own numbers. For his part, he was in favour of universal service ; and this remark evidently expressed the view of most, if not all, of those present. The citizen was greatly astonished, for he had always understood that the volunteer was equal to three pressed men.

It was gradually impressed on him that it was a great thing to possess superior numbers, for that these would make up for a multitude of sins. If possible, one should bring double numbers to bear against the enemy ; because even the great Napoleon had never been able to withstand double his own numbers. The citizen rather took exception to this statement, for had not Clive and other British heroes constantly beaten double and even treble their own numbers ? He pointed to the battles of Crecy, Poictiers, Agincourt. It was explained to him that such battles had been fought against undisciplined—that is, inefficient—troops, and that no superiority of numbers could make up for inefficiency. He asked what it was which constituted this " efficiency," and was told that it consisted of many things ; that, before troops could be termed efficient, they must

be thoroughly well trained and able to act, both by day and night, in any and every sort of country; that they must be thoroughly disciplined, the rank and file having perfect confidence in their officers and in their own prowess, and the officers having perfect confidence in their men, in their leaders, and in themselves; that, in addition, they must be well organised, the arrangements for supplying the troops with food, ammunition, clothing, and everything they required, for tending sick and wounded, being almost perfect. Weakness in any one of these, and in numerous other items which it was impossible to remember offhand, would result in a loss of efficiency.

But the matter did not stop here. The most perfectly organised, trained, and disciplined troops would probably be beaten if badly led. This made him ask questions relative to this leading. He was told that the principle of the thing was "to concentrate superior force at the decisive point at the decisive moment." He thought this sounded very pretty, and he rather believed that he had heard the expression before, but he was not quite certain of the exact meaning of it. After some little hesitation he was told that the battle was the decisive point, and that the moment at which the battle was fought was the decisive moment. He pointed out, however, that there were many battles in each war, and that they could not all be decisive points. He was told that they were; or that, if they were not, then the first battle was the decisive point; and that, if that one was not, then the next one would be; or it might be that the last battle would prove to be the decisive point. He said it seemed to him very difficult, and was told that it was difficult; that the average man found it sufficiently hard to say which had been the decisive point in a war after it had been fought, and that it was one secret of success to be able to forecast the decisive point and another to prepare the superior force in peace time; for, unless that were done, it was unlikely that superior force would be available at the first battle. Then followed a discussion as to the consequences of losing the first battle, and the general consensus of opinion was that, in modern war, it would almost certainly prove disastrous. The reason seemed to be that defeat led to demoralisation. The citizen found it difficult to believe that men could be downcast by a single beating; but he was assured that, judging from history, it was undoubtedly the case, only, of course, the better the

troops the better would they stand up under defeat. It was impressed upon him that, with superior numbers and superior efficiency, a nation could make almost certain of winning a war; and he was also told that some German general had written that, as it was impossible to make certain of superior efficiency, it became necessary to aim at superior numbers by training every man in a nation to arms.

On another occasion the conversation turned on the Russo-Japanese War, and how the Japanese had very cleverly attacked the Russians, without, in the first instance, declaring war, and inflicted what proved to be a wound from which the Russians could never recover. It appeared that the great thing to aim at was to surprise the enemy, and that the most disastrous form of surprise was that in which a nation was caught napping—that is, unprepared for war—and suddenly attacked. Such an idea seemed to the citizen to be perfectly monstrous; and, in spite of the illustrations of the Boer and the Russo-Japanese Wars, he refused to believe that nations could act in so dastardly a manner. He recognised, however, that if that form of making war did come into fashion, it would be a poor look-out for a nation which was not perfectly prepared; and he also recognised that, if a nation refused to act in that fashion, it must endeavour to compensate for its exemplary behaviour by making itself stronger than any possible enemy. He found that a certain pessimism reigned as regards a possible struggle between Great Britain and Germany : simply for this very reason, that it was thought that the Germans, having made their preparations, would attack at their own convenience, suddenly and unexpectedly, when Great Britain was least ready to meet the attack; and that there were no signs that the British people were even aware of such a possibility, or were making any efforts to prepare for it. The citizen was half convinced, the exponents of these views evidently being so very much in earnest; nevertheless, he drew consolation from the fact that, in the Russo-Japanese War, it was the fleet of the island power which had surprised its adversary in so effective a fashion, and if the Japanese fleet could accomplish it, assuredly the British fleet could do likewise. It was pointed out to him, however, that he was optimistic, for that it was not the sailors or the admirals who decided when it was time to attack an enemy, but the statesmen ; and he was asked whether he had sufficient faith

in British statesmen to believe that they would order the fleet to go and surprise the enemy. As his political views were pronouncedly opposed to those of the Government, he felt that there was but little hope until after the next General Election. Nevertheless, on thinking matters over, he refused entirely to believe that a modern civilised nation would suddenly attack an unsuspecting neighbour.

He had but just arrived at this conclusion when the Austrians suddenly, without warning, seized two Turkish provinces. Shortly afterwards the Italians, again without warning, attacked the Turks and seized Tripoli; and, while the Turks were still at war with the Italians, they were, again without warning, attacked by the allied Balkan States. So unsuspected had been the existence of this alliance and so rapid the collapse of the Turkish power, that the citizen was obliged, against his will, to discard his previous conviction and admit to himself certain fundamental truths:

That wars are won by superior force, wisely employed.

That superior force consists of superior numbers combined with superior efficiency.

That the first battle is all-important.

That the best way to win it is to attack the enemy before he is ready.

That modern wars are, accordingly, won by peace preparation.

While, however, he admitted to himself that this was the scientific method of conducting war, yet he refused to believe that the great British nation would ever be guilty of such methods. Such being the case, it became evident to him that the nation would do wisely to organise and train every available source of fighting strength, in the hope of successfully repelling a sudden and unexpected attack.

He had always belived that the Government would make the necessary arrangements to assure the security of the nation; and, being of a tractable disposition, with plenty of work of his own, he was entirely content that it should be so—always provided, of course, that he was not overtaxed. He recalled to mind, however, that after the Boer War the Government had disclaimed all responsibility for neglect to prepare for it; and had asserted that the defence of the country was the business of the people themselves, that is, of the British citizen. Evidently,

it behoved him to devote the most earnest attention to this problem of the national security. He determined to study the whole matter on strictly scientific, or business, lines. But he found it difficult to commence; the whole business was an unknown quantity to him; there were no known quantities at all, except these two horrible ideas, of superior force and attacking the enemy when he was unprepared. Where was he to turn to gain knowledge?

Though quantities of literature had been produced on the subject, yet such of it as he had read arrived at conclusions which were hopelessly conflicting. Some were in favour of one thing; some in favour of another; some in favour of nothing; but most people were apparently stoutly opposed to the views of everybody else. He began to think that, perhaps, Lord Roberts was not altogether wrong in his strenuous advocacy of national service; but, on the other hand, " militarism " was said to be (by those who knew what it meant) a fell disease. Besides, it had been said by a member of the Government, a man in whom everybody had the utmost faith, that there were two descriptions of strategy, one which controlled armies in the field and one which constructed them in peace time; and that Lord Roberts, though a master of the former, was ignorant of the latter. Then there were assertions that the field gun and rifle of the army were not all that could be desired, that the cavalry were short of horses and that the army would be seven thousand short of officers on mobilisation. This seemed a large number. On the other hand, the reassuring official statement had been made that the army was better than it ever had been. That was very consoling. At the same time, one must evidently compare an army, not with what it has been in the past, but with those armies against which it might have to fight in the future. The state of the navy was also disturbing. There were men who could hardly be termed either pessimists or alarmists, who questioned both the efficiency and sufficiency of the navy. It was said that there were not enough cruisers and not enough men to man the navy when mobilised. On the other hand, the citizen had been officially told to sleep peacefully in his bed. But he had already slept for nearly a century on this matter of defence; surely, it was time to be up and doing. He began to doubt this official optimism. It had been clearly proved, so he understood, that the naval superiority of 160 per cent. over the

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next strongest navy which he had enjoyed a few years ago had now been reduced to a mere 60 per cent.; the two-to-one standard had not been maintained. The Mediterranean, moreover, had certainly been practically evacuated by his fleet; and he had read somewhere, at one time or another, that the Mediterranean was the strategical pivot of manœuvre, or strategical centre of gravity—he could not quite remember which; but, at all events, it was something of first importance.

These official statements did not ring true. The citizen religiously read all the debates in both Houses of Parliament; and he had been struck by the very unconvincing answers to certain questions. Some of the official statements, moreover, were rather conflicting; while some of the statesmen appeared to have changed their minds whenever it suited their convenience. He had gained a temporary increase of confidence when he read that the official views were supported by the General Staff and by the Committee of Imperial Defence. But, within a few days, the statesman concerned had modified his assertion; and everybody had gathered that the General Staff had raised some objection. He had asked a soldier friend of how many officers the General Staff consisted; and had been told that he was not quite certain, but that he supposed there might be some two or three hundred scattered about in various parts of the world. The citizen ruminated, asking himself, were all these officers unanimous on this tremendous problem of national defence? He also made inquiries as to composition of the Committee of Imperial Defence; and this body seemed to consist chiefly, if not entirely, of members of the Government. The citizen had lately been reading Dickens aloud to his family after dinner; and all had been hugely amused at the cleverness of Sairey Gamp in putting the closure on an argument by quoting the opinion of the non-existent but expert Mrs. Harris. It seemed to the citizen as though the Committee of Imperial Defence and the General Staff were being used by statesmen as political Mrs. Harrises.

The citizen did not at all like it. His suspicion was accentuated by the fact that, while he had been asleep, or, rather, while he had been in the act of yawning and stretching, neighbouring nations had left him far behind in the matter of aerostatics. Here was a patent danger. Of what value was the command of the sea if the command of the air were lost? He

had visions of bombs, literally bolts from the blue, bursting on his devoted head in the middle of the night. Clearly he should awake and work to make up lost ground; but he trembled to think what it would mean to him if war broke out while he was still unprepared. It was this that taught him, more than anything else, that, during all these years of sleep, the business of war, like everything else, had progressed and become more scientific; and that the conduct of war, which he had fondly believed to be an art to be left to the genius of the artist who should appear when the occasion arose, had become a science in which forethought and preparation would play a dominant, possibly a decisive part.

But what was he to do? He knew nothing of the subject, not even the rudiments of it. Who was he to believe? Was Lord Roberts right; or were the politicians right? What did the General Staff, or those responsible for it, really think? What did the Naval General Staff think? After all, these were probably the men who knew most about it; and it struck him, for the first time, as an absurdity that the men who knew most about so vital a matter as national defence should be the only men who were not allowed to express any opinions.

He must find time to study the matter for himself; but how should he begin? To maintain forces, aerial, sea, and land, superior to those of any possible combination of enemies would necessitate taxation which he, for one, was by no means prepared to pay. It was also a counsel of perfection unless the nation possessed resources, both in men and money, far superior to anything which other nations enjoyed, and also unless the men of the nation were prepared to pay a tax of one, two, or three years' personal service as well as a mere money tax. That the navy and the aerial force should be stronger than those of any possible enemy, or even probable combination of enemies, he was quite prepared to admit. But why should the army be stronger than that of a possible opponent? He considered and discussed this question; and finally concluded that it was necessary to maintain an army of such size and efficiency as would enable it to safeguard the over-sea possessions and home territory in all eventualities and assure allies in the event of European complications.

He had hesitated to admit this last; but he had now learned that Great Britain had, in the past, been constantly obliged to

intervene in Europe in order to maintain, or restore, the balance of power, because no nation had ever established its supremacy on the Continent but it immediately sought to compass the downfall of the British power.

What size army was, then, required? And what plane of efficiency? It was evident to him that the highest efficiency was necessary; and that it was excessively foolish and extravagant to maintain anything in the nature of an inefficient armed force. But the size of the army proved to be a great stumbling-block. Expert opinion seemed to differ in the most remarkable fashion from an army numbering millions, obtained by European compulsory methods, to a small voluntary army.

The citizen has not yet made up his mind as to the strength of the army he requires, or whether voluntarism is sufficient or compulsion necessary. He is, however, inclined to think that the voluntary system is incapable of producing an army of the required numbers or efficiency, and that the men of the nation must be prepared to pay a tax, not only of money, but of personal service. One view he has heard, however, which has given him food for thought. Can a nation, he was asked, which is content to train but a very small portion of its men to arms, hope to compete with success in preparation for, and in the conduct of, war, whether on land, sea, or air, against one which trains every able-bodied man? In the one case you have a general ignorance of military matters; in the other a general knowledge. That, it appeared to him, was the scientific problem of the future; and it also appeared to him that the British nation was determined to try the experiment of her voluntary systems against the modern system of the nation in arms.

Another point impressed him greatly. He was assured that it requires twelve years in which to convert a voluntary system into an efficient modern system.

WOMAN'S PLACE IN NATURE

I.—By M. S. PEMBREY, M.A., M.D.

The present time is one of unrest; and one of the signs, the violent agitation in pursuit of the so-called "rights of women," is worthy of consideration as a problem of biology. As such the movement has both a physiological and pathological aspect, and there are many indications that a frank discussion on these lines is needed. The problem is not a simple one. The agitation is not supported but resisted by a majority of the women of this country; in the ordinary sense of the word it is not political, for the militants of the so-called "woman's movement" will support alike Tories, Liberals, Radicals, and Socialists, provided that they will cry "Votes for Women." It is a movement supported by a limited number of women and men, whose views may be in advance of civilisation or may on the other hand be an expression of the pathological effects of over-civilisation.

It is often forgotten that men and women are subject to biological laws. The effects of civilisation upon the characteristics which they have shared with animals for unknown ages are very small and are not necessarily progressive. Public opinion in this country has been greatly influenced by the advances and theories of biological science. The belief in the Bible as a guide to conduct has been undermined, but the practical application of the theory of evolution has not taken its place. Even among scientific men the pressure exerted by public opinion is so strong that conventional views on morality are often more effective than the teaching of science. Public opinion upon what is right and what is wrong varies from time to time, and at any time is a question of geography. The biological basis of a true morality must be eternal, the same at all times and in all places and for all mankind.

If the subject of woman's place in nature is examined from the biological standpoint, it will be found that there is no support for the doctrine of equality. Biology shows that differentiation in structure and division of labour go together,

Man and woman can never be equal. The only way to bring about an approximate equality is to unsex both. Such a levelling process the primitive instincts of healthy women and men will prevent. Nevertheless it must be admitted that too much attention has been given to the views of those in whom these healthy instincts are not properly developed. Signs are not wanting that some men and women, who think that they have a public mission, look upon their animal characteristics as an obstacle to the attainment of what they call the higher intellectual and spiritual life. They have lost or never fully possessed the natural instincts which serve as a guide to life. They do not know what or how much to eat or drink, when to work or when to rest or when to marry, and vainly seek for rules of life ; they have overlooked the fact that excesses of intellectuality and spirituality as often lead to wayward conduct, illness, and degeneration as the more common vices. Sexual antagonism is the special mission of other extremists. The words of our national marriage service, which has long been cherished by many generations of women, are declared to be offensive and indecent. The widespread decline in the birth-rate has shown that marriage has been debased from the position which it should occupy according to the teachings of religion and biology.

The old-fashioned view of woman's place in nature is the one supported by biological knowledge. Woman's sphere was the home and family, for there she found ample opportunities for the exercise of her special gifts of patience, kindness, and love of offspring. Her influence in the State was indirectly as great as that of man, for apart from the control she exercised upon man, she held in her hands the training of her sons and daughters in those early years during which character is most easily moulded. The responsibility of a family prevented her from becoming too much interested in herself or in intellectual problems. As a young woman she looked upon marriage as the aim of life, and as an experienced matron, with every wish for the happiness of her daughters, she kept the same ideal before them. The term " old maid " was one of reproach ; a childless marriage was a calamity, a reflection upon one or other or both partners; the marriage of a young man and an old woman was an unnatural condition to be explained only by sordid motives. All of these prejudices had a true biological

basis, and, although it may sound harsh in these days, served a good purpose in maintaining a true ideal. Even the feminine fashions and adornments were a recognition, often unconscious it is true, of the importance of secondary sexual characteristics. The mind and the body react upon each other; mental conditions influence the internal secretions, and as is well known, the internal secretions have a profound effect upon the mind. The woman who was afraid of a mouse gladly braved the risks of childbirth and bore her pains without the use of anæsthetics. The restrictions imposed upon her activity by bearing and suckling her children were not deplored as unfair limitations of her career, but were accepted either with joy as a holy duty or as a matter of course. It would have been an insult to suggest that she lacked in the least degree the maternal instincts so well developed in many of the lower animals. The true mother toiling for her husband and children did not deplore her lot or consider herself a slave or martyr any more than the sailor or miner regards himself as a hero in running risks of shipwreck or explosion. She was not worried by ideas of equality with man; she knew full well that in many respects she was superior, and as such claimed and obtained exceptional treatment and respect. Her womanly charm was more effectual than reason in influencing man in her favour; her natural tact and intuition were more useful than a logical argument. The fact that she was educated and trained along special lines was no reflection upon her mental or physical capacity; it was a recognition of the ideal division of life's labour and purpose. The limitation of the means of earning a living was not a grievance, for domestic service, teaching, and nursing were responsible duties which formed the best training for a woman whose future was in married life.

On all these points a biological defence, if defence be needed, can be offered, and there is little doubt that, even if the new women increase in influence by obtaining votes, the majority of women will maintain their position by those qualities which have served them so well in the past. The old-fashioned ideal is not debased because it is sexual and has an origin in animal instincts. The slur cast upon our Victorian mothers has not been properly resented. It is true that they did not glory in competing in mental and physical contests with men, but they could and did bear and rear large and healthy families. The

possession of a baby is of more value to the State than a first-class in classics or a silver trophy for sport. The peasant woman gazing with longing eyes upon her child at her breast has an experience of the purpose of life which the highest intellectual gifts alone cannot supply.

It may now be asked why with such an ideal before them is there a revolt among certain classes of women? What are the causes and how are they to be removed? It seems clear that the chief cause of the unrest is modern education, which has been artificially forced and encouraged along wrong lines. Too much stress has been laid upon intellectual attainments and pleasures, and it has been loudly proclaimed that the education of the two sexes should be the same and that a woman should not be debarred from entering any profession or occupation she may choose. It is maintained that a woman is a better mother if she be well educated. Even if this statement be admitted, it depends upon the definition of a good education. The natural instincts of healthy women have for ages guided her in the performance of the duties of a daughter, wife, and mother, and there is little doubt that an unsuitable or bad education by suppressing or blunting those instincts will make her less efficient in these services which are of fundamental importance to the race. The effects of education and of a specialised profession or occupation are obvious even in a man; his body and mind are moulded to type. The effects upon woman would be greater especially if the occupation were continued for life; her sexual life begins early and ends early, and under natural conditions makes a great demand upon the resources of the body. Even if she can perform more efficiently than man any of the work generally done by men, the race will lose thereby, if at the same time she becomes unfitted for those very duties which man can never assume.

It is difficult to obtain data, but there is general agreement that the more highly educated people are the less fertile. There is both a comic and a pathetic side in the meetings of learned men and women to discuss the subject of eugenics; it would not be an unduly rash calculation to say that the average number of offspring of the married members at most meetings is not more than two.

The extension of the old doctrine of internal secretions by the modern work upon the functions of the ductless glands has

shown that bodily and mental health are a complex interaction of all the organs performing their functions in proper sequence. The distinctive organs of the two sexes are no exception to this rule, and no one with common sense and a belief in either design or evolution will maintain the contrary.

The intrusion of women into the occupations formerly occupied by men has made them independent but at the same time has deprived men of employment. Every healthy man is a potential husband. Now the woman's demand is " equal wages for equal work." It is impossible for any woman, however able she may be, to carry out the duties of a profession and at the same time bear and rear numerous and healthy children. By the very nature of things, and by no means due to man-made laws, the woman is not in a position of equality. Even if she removed these obstacles by practising celibacy, she would not be entitled to equal wage for equal work ; a man's duty to himself, to woman, and to the race is to marry, and the State should recognise, as it is beginning to do in greater measure, that the fulfilment of this duty entitles the man to better pay or less taxation. The celibate woman, who performs for the State no duty which a man cannot equally well do, is not entitled to greater pay than her sister who is forced by the claims of motherhood to retire for a time from the same kind of work.

The higher education of women and their employment in posts which might be filled by men has brought about a postponement of marriage to such a late stage that often half the period of the woman's sexual life is already past. Late marriages are bad for the health and morals of both sexes and bad for the State, for the offspring will be less numerous and, as the evidence goes, less vigorous. The idea that a smaller number of children born to parents no longer young will grow up into better citizens owing to a better environment has no biological support. The only child lacks the beneficial effect of the struggle for existence in the family, the mutual education, the discipline and the hardening of both body and mind produced by the clash of its interests with those of numerous brothers and sisters. A woman should experience the joys and trials of a family when she is young and able to adapt herself to circumstances and play with her children ; she should look forward to spending her old age not with her children around her, but with her grandchildren or great-grandchildren.

The so-called higher education of women is not a good ideal for either woman, man, or the State. Education at a University for three or four years makes a considerable demand upon the bodily, mental, and pecuniary resources of the woman, and there is little doubt that these would be more useful to all concerned if they were devoted to, or reserved for, marriage. There is no evidence that the middle-aged intellectual woman makes a better wife or mother. The indications are all the other way. The mental training causes the woman to be self-centred and more sensitive to any discomfort or pain associated with child-bearing and distracts her attention from those domestic duties which mean so much for the health and training of her children. So little is known of the conditions determining the transmission of intellectual capacity that an anticipation of the propagation of intelligence or genius by the marriage of the highly intellectual is even less justified than the prediction of mediocrity or insanity. The woman who is married for her services as a cheap secretary or assistant in her husband's intellectual pursuits is as much degraded as the wife who is valued only as a cheap housekeeper and cook. The physiological test of woman's efficiency is motherhood.

To all these arguments it may be objected that marriage as a career is not open to all women, because there are about a million and a half more women than men in this country. Why, if it is maintained that women are equal to men, should not women take their share in building up the Empire by emigration to the Colonies, where there is a dearth of women? In Australia and New Zealand they might obtain both husbands and votes, and might reintroduce the old-fashioned morality of family life. In these Colonies where the women have the vote, the artificial and immoral limitation of offspring has resulted in a decline of about 30 per cent. in the birth-rate. Some details of the opportunities for marriage in Canada were given at the recent meeting of the Central Emigration Board; a lady, who had spent the greater part of the last four years in the Dominion, is reported [1] to have said that "if a woman went out to the West she married almost inevitably. She had had seven proposals in seven weeks. She did not know even the names of some of the men, one of whom was a cook in a Canadian Pacific Railway train. A party of forty-five girls went from

[1] *The Daily Telegraph*, May 2, 1913, p. 15.

Vancouver to Montreal. Forty of them got married on the way, and only five arrived at their destination."

A further remedy is to be sought in a return to a simpler standard of living. Limited pecuniary resources are no obstacle to a happy and healthy family, and it is notorious that many of the greatest men have been the sons of poor parents in humble positions. A true biological ideal is necessary : early marriage, numerous offspring, and a healthy struggle for existence. Women, even without votes, have more than their share of influence in moulding public opinion. Let them recognise that conventional morality, which allows and even preaches the prevention of conception and the induction of early abortion, is wicked, degrading, and injurious, especially for the woman. Let them admit that the servant girl who gives birth to an illegitimate child is more moral, even if she is less educated, then the woman who, from the day of her marriage, openly sanctified by a religious ceremony, takes measures to prevent motherhood. From a biological standpoint an illegitimate child is a testimony that a woman is more moral than her sisters who have taken preventive measures. A decline in the number of illegitimate children is no evidence that a country is more moral. This truth appears to have received little recognition from women, but judges and juries, knowing the bitterness of the persecution of women by women, always show a sympathetic attitude to women, even when they are guilty of infanticide.

The prevention of conception, voluntary abortion, and prostitution have no analogy among the lower animals ; they are not physiological, but pathological. These evils are not due to man-made laws, but to the absence of a true sexual instinct in many women. They are not due to low wages, and it is the grossest insult to women to say that poverty is a bar to true virtue. Twenty or thirty years ago domestic servants had low wages, but there is no evidence that they were less virtuous than the servants of the present day, who, without the aid of any trade union or votes, have raised their wages by about 50 per cent. The demand for domestic servants exceeds the supply, and there is no economical reason why a woman should degrade herself for money. There is no evidence that woman suffrage has abolished these evils ; indeed, it would appear that the increased occupation of women in commercial pursuits has led to a wider spread of the disease in a less virulent form.

It is common to speak of an immoral person as a brute, but it is not true. If all women had the healthy sexual and maternal instincts of animals, these evils would not exist.

The demand for equality in the matter of divorce is not well based, for it pays no attention to the physiological differences in the two sexes, and, if it should be granted, would probably decrease the stability of family life, which is the fundamental basis of every nation.

These subjects have been mentioned here because they figure so largely in the discussions on the supposed inequality of women. Women can rightly claim, and generally receive, pre-ferential treatment, but they cannot obtain equal treatment, except to their own detriment, for it has no firm basis in biological conditions. The natural protector of womankind is man, not woman. Motherhood is the true ideal for women; a voluntary celibacy is not virtue, but at best the expression of a neurosis.

<center>II.—By O. A. CRAGGS, D.Sc.</center>

WHEN—more than a year ago—a number of women knelt in prayer for votes before the Rhadamanthuses of Westminster and hoped that they were at the very point of melting those stony hearts and brains, in ran a wild person flourishing a torch. This flambeau, he cried, was the Torch of Science; which had lighted him to see into the very depths of feminine nature; in which he had descried nothing but physical and mental weakness, vanity, silliness, hysteria, emotion, partiality, dogmatism, excitability, unreasonableness, and utter ignorance. Woman's place in nature was (he said) merely that of a semi-human matrix of humanity (which is really man); and she was fit only to scrub doorsteps, to cook, and to bear children. At this, not only did the assembled idols harden their hearts and refuse the women's petition, but, as Carlyle says, innumerable Rushlights and Sulphur-matches were kindled at the torch and waved up and down the world by other wild persons; and the women went away and redoubled their violences, and even rooted up Golf-greens.

I protest that the torch was not that of Science at all, but a miserable counterfeit lighted by politicians to dazzle the eyes of their own likes. For votes for women I care not a jot, either

for or against; because the whole quackery of politics—votes, representation, parties, caucuses, divisions—has now been discovered by the intelligent part of mankind. But the name of Science should not be dragged into this welter of fraud; and I have enough good northern blood in me to resent rudeness to women under any plea. It may or may not be wise to give them votes; there may be other reasons against it; but those urged by these farthing-dip bearers in the name of Science are not hers. Her light is shed equally on all sides of a question— not only on one. Come then, let us see how the same argument will apply to the other half of the race, the males.

Woman's only duty is motherhood, they say. But surely we might as credibly affirm that man's only duty is fatherhood. If the franchise be excluded on these grounds, none but bachelors and spinsters should have it. But sociologists maintain that these are the least worthy of it because they have not performed the duty of parentage to the State. If, then, only parents should have it, why not mothers as well as fathers? And the mother's share of the burden is far more onerous than the father's, involving often the health of a lifetime, and, indeed, life itself. Moreover, nearly the whole care and teaching of young children is in the mother's hands—in addition to many other duties. True the father provides the livelihood; but, hour for hour, is his work harder, or more difficult, or more painful than the mother's? Scarcely; and on this count, if either must be excluded from the franchise, it should be the father. As regards spinster and bachelor, it is the latter who neglects the duty, because it is only he who is always, or generally, able to marry if he chooses. So here again the woman's case wins.

But if the performance of natural duties to the State gives the first claim to a vote, what shall be said of the men who neglect to train themselves for war? If it is the duty of woman to be a mother, it is that of the man to defend her and his country. The woman performs her part of the obligation—with travail and at the risk of her life; but how many of the young cubs of the day who deride her claims to the franchise perform theirs? What of the idle, unhealthy, and dirty crowds who boo the women at their meetings, but who, likely as not, would run like rabbits at the first shot of war if ever they had strength to reach the front? This is the just answer to their contemptible

contempt. Nor can their claim be allowed that they pay taxes to hire substitutes in a voluntary army. For sacred duties there can be no substitutes; and, besides, our best soldiers tell us with unanswerable reason that the time has come when the country needs all the men it has. In the light of this logic, then, every woman who has borne a child should have the franchise; but not a single man who has not done his turn of military service. And moreover such men should by rights be forced to pay the taxes for the whole army and navy. But in our brainless nation, the mother has no vote; the father of a large family pays nearly as much as the gay and careless bachelor; and the soldier and dutiful volunteer as much as one who serves the State not at all!

But, say the pretended scientists, the women do not possess the knowledge and judgment of the men. Good gracious, how many men possess either? As for knowledge, most of them know a few tricks, learnt from others, which they call a trade or a profession, and which as a rule they perform indifferently. Not one in a thousand ever reads a worthy book, ancient or modern, or, after his schooldays, ever troubles himself again to study anything. Their knowledge, like that of most women, comes from newspapers, poor novels and plays, picture shows and current talk—good enough perhaps for the mass of humanity. Women have their own knowledge, of the same level. Is the man who knows only how to rivet boilers or how to sell cheese a better judge of national policies than a woman who knows how to cook or how to keep a happy home?

In the end, what proof have we that the knowledge and intelligence of women are inferior to those of men? To measure either with close enough accuracy for comparison is almost impossible. The assertion that such measurements have been made by " Science," with this or that result, is a pretence and a falsity. The only possible justification might be that women have not taken the first place in most of the highest lines of intellectual work, science, art, and invention. But such work is the rare, the very rare, efflorescence of mind; and we must not judge the average degree of knowledge and intelligence by such exceptional phenomena; while other causes than that of mere inability may be at work.

A man of any experience of the world, looking broadly at the human race of the present, will not easily accept the im-

mense superiority of the male. It is a common thing to hear of
the tallness, healthiness, and strength of the young women of
the day; and also of the weediness, laziness, and unhealthiness
of the young men. We can compare them in any train or
omnibus—not at all to the advantage of the latter. Every day,
at an early hour in the morning we see hundreds of young
women hurrying happily and healthily to their shops and offices
for a hard day's work; and also, somewhat later, hundreds of
men smoking cigarettes with bored expressions and evidently
vacant brains. As for the older men, how dull and stale they
often are—with not a grain of enthusiasm for anything in the
world, yet sniffing in a superior manner about the efforts of
those who attempt any reform whatever. No; I for one think
that the woman is on the whole the better of the two, except
only in the matter of muscular strength.

And what, I should like to know, have the greatly superior
political aptitudes of the men done for humanity all these cen-
turies. The great progress of the world in health, prosperity,
and general happiness has been due almost entirely to a very
few men of genius—mostly men of science, writers, and in-
ventors; and not at all to the politicians. Measure up candidly
what these people have actually given to the human race—
perhaps a few good factory laws; to which, by the by, they
have almost always been driven by public opinion, that is, by
the writers. After endless heat, immense discussions, portentous
debates, the formation of endless parties, the interaction of
innumerable intrigues, this political mountain has brought forth
only this one little mouse. On the other hand, they with their
false notions of party, their trained and organised party pre-
varication, and the false ideals which they ever hold before the
public, are mainly responsible for the international and the inter-
social strifes of the day which impede further progress. What
do they do for science, art, invention, or morality?—nothing
whatever. Their very laws are so badly framed that the
lawyers who profit most by that bad framing condemn them.
Amateurs at their own art, they do little but confuse the issues
which poor humanity is called upon to face.

But I have nothing to do with the political question of the
franchise for women. The answer for that depends, does it
not? on what is the use of the franchise at all—a very difficult
problem. But every scientific man, however humble, is con-

cerned with the honour of Science. It is false to say that Science has discovered the inaptitude of women for votes. Science has not even discussed the subject; and cannot discuss it until she possesses much more data than she has at present.

It would be easy to spin a dozen similar biological explanations of the present revolt of the women. For instance (it may be argued) their increasing physical and mental excellence is some subtle compensation of nature for the increasing deterioration of the men in this country, due to centuries of peace, to the neglect of true warlike exercises and physical emulation, to indulgence in mean pleasures and indifference to all high effort; that the women are conscious of this relative change, and are no longer content to be ruled by masters whom they no longer trust as much as they did. That is as good a theory as the other. Neither can dare claim the sanction of Science.

THE SEATS OF THE SOUL IN HISTORY

By DAVID FRASER HARRIS, M.D., B.Sc. (LOND.)

IT is well known to the historian of biology that even the plants have been supposed to possess souls.

The famous naturalist, Andrea Cæsalpinus (1519–1603), of Arezzo, who is even now regarded in Italy as the dicoverer of the circulation of the blood, enters into a long discussion on the nature and seat of the plant-soul in his book, *De Plantis Libri* xvi. (Florence, 1583). He writes : " Whether any one part in plants can be assigned as the seat of the soul, such as the heart in animals, is a matter for consideration—for since the soul is the active principle (' actus ') of the organic body, it can neither be ' tota in toto ' nor ' tota in singulis partibus,' but entirely in some one and chief part from which life is distributed to the other dependent parts. If the function of the root is to draw food from the earth, and of the stem to bear the seeds, and the two cannot exchange functions . . . there must either be two souls, different in kind and separate in place, the one residing in the root, the other in the shoot, or there must be only one, which supplies both with their peculiar capabilities. But that there are not two souls of different kinds and in a different part in each plant may be argued thus : we often see a root cut off from a plant send forth a shoot, and in like manner a branch cut off send a root into the ground, as though there were a soul indivisible in its kind present in both parts. But this would seem to show that the whole soul is present in both parts, and that it is wholly in the whole plant, if there were not this objection that, as we find in many cases, the capabilities are distributed between the two parts in such a way that the shoot, though buried in the ground, never sends out roots— for example, in Pinus and Abris, in which plants also the roots that are cut off perish."

We need not follow the subtle Cæsalpinus through all the details of his arguments as to where the soul of the plant must reside, but he finally places it at the junction between the root

and the stem. This region, later known as the "collet" or neck, was, even after the time of Linnæus, regarded with a superstitious respect, as though here had been established some special focus of vitality.

Cæsalpinus is, however, later on in this dissertation, quite inconsistent with the notion of the localisation of the plant-soul, for, although he has assigned it to the union of the root and the stem, he is afterwards forced to admit that the vegetable soul must be diffused through all the parts, even to the extremities of the leaves, which, of course, are very much alive.

Cæsalpinus had only followed Aristotle in believing in a plant-soul: his conception of plant-life is quite Aristotelian, thus: "As the nature of plants possesses only that kind of soul by which they are nourished, grow and produce their like, and they are therefore without sensation and motion, in which the nature of animals consists, plants have accordingly need of a much smaller apparatus of organs than animals."

The well-known man of science, the Burgundian Mariotte (died 1684), in his *Sur le Sujet des Plantes*, declares that, as we know nothing about the vegetable soul, the assumption of it is not helpful in plant physiology.

If we go far enough back in the history of thought about the relations of the soul to a material substratum, we find that the seat of the mental processes was not originally supposed to be within the nervous system at all. The ancient Egyptians regarded the soul as seated in the heart, as also did Aristotle (B.C. 384–322), an idea by no means fantastic when we reflect on the ease and certainty with which emotional states influence the force and rate of the action of that organ. As late as the time of the Neapolitan philosopher Vico (1678–1774) this idea was revived, Vico insisting, contrary to Descartes, that the mind was in the heart and not in the head.

Aristotle, in particular, referred to the brain as "cold and bloodness," and imagined its function to be that of cooling vapours from the heart.

Another old Greek idea was that the mind or soul resided in the diaphragm, a reference to which still lingers in our own word phrensy (frenzy), which is derived from phren, the Greek word for the diaphragm. "Phreno-pathia" is a now little-used term for mental disease, and "phrenetic" means mentally excitable, while "phrenitis" has actually become a synonym for

inflammation of the brain.   Hence the word " phrenology," a term for that pseudo-science which purports to be a discourse on the localisation of things mental, is actually derived from a word which refers to the diaphragm, and neither to the brain nor the head at all.   It is not difficult to see how the notion arose that the soul was resident in the diaphragm, since strong emotions—affections of the soul—strongly affect that great muscle so important in breathing.   Emotions made the chest to heave visibly, therefore emotions arose or existed locally in the chest and in its chief muscle, the diaphragm, so the ancients argued.

That viscera are related to mental and emotional states is a very old observation, as for instance in the Bible when we read in the Psalms, " My reins instruct me in the night seasons."

From time immemorial has not the spleen been thought to be the seat of anger and envy?   We even yet talk of a " splenetic " man and of a " fit of spleen " as meaning an angry man and a fit of anger.   While Shakespeare undoubtedly accepted these notions on the visceral distribution of the emotions, placing love, for instance, in the liver, he had at the same time undoubtedly heard of the soul as seated in the brain, for he wrote in *King John* (Act V. Sc. 7):

> It is too late : the life of all his blood
> Is touched corruptibly, and his pure brain
> (Which some suppose the soul's frail dwelling-place)
> Doth, by the idle comments that it makes,
> Foretell the ending of mortality.

The early Belgian chemist van Helmont (1577–1644) was probably one of the last men of science to regard the soul as existing outside of the head : he placed it in the pylorus of the stomach.   His reasons for this are very quaint reading : " Though it carries out sensations and movements by means of the brain and nerves, its actual throne is in the pylorus ; it resides in the orifice of the stomach."   In proof of this van Helmont says that a great emotion is always felt at the "pit of the stomach," and that " a man may have his head blown off by a cannon-ball and his heart continue to beat for some time, whereas a severe blow over the pit of the stomach will stop his heart and take away his consciousness simultaneously." But he qualifies this in the following subtle manner : " Though

it is placed in a locality it is nevertheless not there in a local manner; it is present in the stomach in some such way as light is present in a burning wick."

Concurrently with these ideas regarding the extra-cranial seats of the soul, there had been schools of thought from the earliest times which regarded the central nervous system as that to which the mind was related. As long ago as about 300 B.C. Herophilus of Alexandria had imagined the soul to be inside the fluid of the cerebral ventricles—these innermost recesses of the entire body, the mental Holy of Holies. Herophilus regarded the fourth ventricle as particularly mental: this is very interesting to us, seeing that below that cavity some of the most important vital centres in the nervous system are undoubtedly situated. Claudius Galen (died 200 A.D.), to do him justice, taught that the brain was the place where the soul and intellect had their home.

We may pass over all the centuries intervening between Galen's death and the date of the publication of Vesalius' great work, the *De Corporis Humani Fabrica*, 1543, because they contributed nothing towards clear thinking about the localisation of mental attributes. The father of Anatomy (1514–1564), to whom physiological problems were by no means uninteresting, has the following prescient remarks on the mind as related to the brain: " But how the brain performs its functions in imagination, in reasoning, in thinking, or in memory (or in whatever way, following the dogmas of this or that man, you prefer to classify or name the several locations of the chief soul) I can form no opinion whatever. Nor do I think that anything more will be found out by anatomy or by the methods of those theologians who deny to brute animals all power of reasoning and indeed all the faculties belonging to what we call the chief soul. For as regards the structure of the brain the monkey, dog, horse, cat, and all quadrupeds which I have hitherto examined, and indeed all birds and many kinds of fish, resemble man in almost 'every particular. Nor do we by dissection come upon any difference which would indicate that the functions of those animals should be treated otherwise than those of man. In proportion to the size of the body, first the ape and then the dog exhibit a large brain, suggesting that animals excel in the size of their brains in proportion as they seem to be endowed with the faculties of

the chief soul. I wonder at what I read in the scholastic theologians and the lay philosophers concerning the three ventricles with which they say the brain is supplied."

The particular views Vesalius could not accept were that the most anterior cavity in the brain was for sensations, the middle one for imagination and the posterior for memory; notions that had originated with the Arabian doctors and had been adopted by such scholars as Duns Scotus and Thomas Aquinas.

The next attempt to localise the soul and one that attained to a notoriety commensurate with its ingenuity was that by the Frenchman Réne Descartes. The great philosopher of Touraine placed the soul in the pineal gland. There was a show of reason for his choice of this local habitation; the soul, according to all current conception, had to be one and indivisible and not extended in space. No region of the body seemed so suitable for the seat of such an essence as the single, simple, not bilaterally developed pineal gland—the nearest approach to a single point which could be discovered in the central nervous system. Here, after the manner of a general governor or overseer, sat the soul, said Descartes, thither came information from all the senses to it, thence it issued its commands to all parts.

There was a dark side to Descartes' speculations, for his followers, denying the existence of a rational soul in the lower animals, taught that the members of the brute creation were unconscious automata. The practical outcome of this philosophical absurdity was that certain Cartesians treated the lower animals with positive cruelty. Very unfortunately for Descartes, when the pineal body came to be examined under the microscope, it was found to consist only of some atrophied cells and a few crystals of carbonate of lime and other earthly matter—a most unlikely dwelling-place for the soul, for "dust thou art, to dust returnest," was *not* spoken of the soul. Philosophy had to try again. We must next notice the views on this subject of a great Englishman—Thomas Willis, M.D., in his early life a pupil of Harvey. Though Willis wrote extensively on the nervous system, his views are not nearly so well known to the general reader as those of Descartes. Whereas according to Descartes the soul was as nearly as possible an indivisible point which could exist only in an

organ that was not even bilateral, for Willis there were two souls, each widely diffused, the one in the blood, the other in the nervous system. Willis asserted that the soul in the blood was of the nature of a flame, that in the nervous system of the nature of light. Willis's explanation of the way the soul (through its derived spirits) was related to the brain was somewhat as follows: " The lighter and more spirituous parts of the blood ascend by the arteries to the brain, where a distillation takes place, and animal spirits are the result. These spirits flow over the surface of the cerebrum and cerebellum, whence they descend all over the nervous system. Only the spirits in the cerebrum are destined for voluntary movement and sensation, those in the cerebellum are for involuntary movement." This last idea is interesting in the light of modern work, for although we cannot admit that, as stated, it represents the truth, still it is a fact that the activities of the cerebellum are carried on entirely outside the sphere of consciousness. Undoubtedly Willis had glimmerings that sensations and their memories—mental images—were on their physical aspect modifications of the substance of the brain. He talks of " the pictures or images of all sensible things admitted into these secret places." One of Willis's books is actually named *De Anima Brutorum* (concerning the soul of animals). The soul, then, was by Willis allowed to reside in the cerebral hemispheres, where it has ever since been permitted to rest in peace, at any rate on the part of those who believe that it needs a circumscribed dwelling within the bodily frame.

When we come to the brilliant young man of science, the Dane Nicholas Stensen (1638–1686), we come to the first attempt to express the modern notion of localisation of function within the brain, a truth parodied by the phrenologists, believed in by the physiologists. This was how Stensen put it when writing of the fibres in the white core of nervous matter : " If, indeed, the white substance be wholly fibrous in nature, we must necessarily admit that the arrangement of its fibres is made according to some definite pattern, on which doubtless depends the diversity of sensations and movements. It is my opinion that the true method of dissection would be to trace the nervous filaments to the substance of the brain to see which way they pass and where they end ; but this method is accompanied with so many difficulties that I know not whether we may hope ever

to see it executed without a special method of preparing" (1662). We had to wait about 200 years for that special method.

The notions of a central soul and peripherally acting spirits in the nerves of the senses and in the motor nerves lingered for a long time in the minds of the learned. The closing lines of the *Principia* (1687) show that they were the working hypothesis of such an intellectual giant as Sir Isaac Newton.

A return to the idea of the soul as permeating the entire body was made by the famous German thinker, Georg Ernst Stahl (1660–1734), the originator of the unfortunate conception of phlogiston. Stahl spoke of an " anima sensitiva " which penetrated into and possessed every organ and tissue of the body. No tissue really living was outside the sphere of its immanence. The views of Stahl are alluded to as those of " Animism."

The modern statement of the problem has come to be—Is consciousness restricted to an association with cerebral activity, or does it also accompany activity of lower centres, including those of the spinal cord ? Few biologists can now be found who uphold the doctrine that consciousness is awakened by activity of the spinal cord alone : all inferences from experimental work on the nervous system forbid such a conclusion. We cannot imagine that the decapitated snake with only its cord intact which coils itself round the red-hot poker is a conscious organism. On the contrary, it allows itself reflexly to be burnt up just because the seat of its consciousness, its brain, has been removed from the intelligent direction of its body.

As regards emotional and intellectual localisation, the phrenologists have neither advanced nor retarded the scientific study of the material relationships of consciousness. John Joseph Gall (1758–1828), usually thought to be the founder of phrenology, originated neither the term itself nor the body of beliefs known by that name. The term was given by one Forster in 1815. Gall was imbued with the notion, correct, but in advance of his time, that certain mental attributes were localised in the cerebrum. He rightly supposed centres to exist for intelligent speech and for word-memories. Gall lectured on the functions of the cerebrum before various universities in Germany. His colleague, Spurtzheim, much less of a man of science and more of a popular lecturer, developed phrenology as we know it to-day. Its dogmas and absurdities are too well

known and have been too long refuted to detain us now. But possibly some of us have little idea of the furore that phrenology caused in the early years of last century. The Phrenological Society of Edinburgh had 630 members, that of London 300, and a Chair of Phrenology was actually established at the Andersonian College in Glasgow.

The modern problem is not where the soul is seated, but what precise modification of cerebral tissue constitutes the physical concomitant of a mental process—that the two processes are intimately correlated no one doubts. Until lately, physiologists had been content to refer states of consciousness to states of activity of the bodies of the nerve-cells found inside the grey matter of the cortex of the cerebral hemispheres. But the physiological psychologist, Dr. MacDougal, of Oxford, has brought forward some evidence which points to certain delicate junctions between the processes of the one nerve-cell and those of another as being the actual seats of consciousness. The problem is one of interest entirely to the specialist, and one only to be solved by the specialist; but the broad fact remains that natural science knows of no mind as apart from matter, and only a very specialised kind of matter, as directly related to the existence and development of what we understand by mind.

THE OUTLOOK FOR HUMAN HEALTH

By BERNARD HOUGHTON, B.A.

Indian Civil Service

MANKIND, or at least the educated portion thereof, have within the past half-century entered into a new and very beautiful world. In almost every branch of science, whether astronomy, biology, geology, chemistry, physics or anthropology, the atmosphere teems with the busy toil of workers and is electric with the actual or expectant discovery of new and important facts. Brilliant and fascinating as is this fairyland of science, all may not fall within its glamour or perceive the true significance of the gifts it ceaselessly tenders for the benefit of humanity. But there is one branch of knowledge which, whether we will or not, intrudes itself on our attention and insists, under penalty of death or torture, on a punctilious regard to its teachings. Such is the science of medicine or rather hygiene, viewed in its broadest and most comprehensive aspect. The goal of this science is, or should be, to maintain human beings throughout their lives in perfect physical health. And when we reflect how profound an influence health or its absence exerts not only on our happiness, prosperity and material welfare but also on our intellectual achievements and outlook on life, it will be admitted that the progress of medical science possesses for all of us a quite exceptional interest. Has it shared fully and completely in the grand forward march of knowledge, or is there reason for supposing that in some respects at least it lingers behind, a loiterer with the rearguard ?

To understand the position it is necessary to remember that, at least from the standpoint of the general public, medical science is separated naturally into two capital divisions, the prophylaxis or prevention and the therapeutics or the cure of disease. There exist various other important sections, such as anatomy, diagnosis, histology, pathology and so forth but, so far as the general public is concerned, prophylaxis and therapeutics constitute the really vital and essential ones. And since

diseases generally must be classified as parasitic or those of microbic origin, such as tuberculosis, cholera and plague, and non-parasitic or those arising from disorders of metabolism—as, for instance, gout, heart-disease, tumour, etc., it will be convenient similarly to proceed in our discussion of them, that is, we will first consider the progress of medical science in relation to parasitic diseases, and subsequently its position in relation to the remainder.

Just as modern biology is based on the *Origin of Species*, so the foundation of our knowledge of the parasitic or microbic diseases, so far as it is scientific and not mere empiricism, was laid deep and true, a veritable Yggdrasil for strength, by the investigations of M. Pasteur. Prior to his revolutionary discoveries, the vague theories current ascribed their etiology to morbid poisons—note the tautology—in the air, to decaying vegetable matter, to ferments floating about promiscuously, and so forth. The supporters of the germ theory of disease, before the increasing body of facts proved too strong for their opponents, encountered a strenuous opposition from the more "conservative" element of the medical profession; they had in fact to fight a kind of Quatre Bras against the doctors before aligning themselves for their Waterloo against the microbes. All such controversies, however bitter and envenomed at the time, are fortunately now a thing of the past and possess merely that historic interest which still enchains our attention when reading of the discoveries of a Galileo, the enunciation of Newton's laws, or the gradual acceptance of the atomic theory.

In the brief period—scarce a third of a century—since M. Pasteur's discoveries marvellous progress has been made. Though we stand as yet only as it were in the early morning of discoveries touching the etiology of the parasitic diseases, their prophylaxis and cure, the sun of science shines brightly above the horizon and all the air is radiant with hope. In spite of the opposition of such fanatics as anti-vaccinationists—soon, let us hope, to be as extinct as the Fifth Monarchy men—and in spite of official discouragement and of a lamentable exiguity of funds, very noteworthy results have already been achieved. In malaria, perhaps, estimated both in its annual death-roll—some 1,300,000 in India alone—and in the chronic ill-health it inflicts on the involuntary hosts of *Plasmodium malariæ*, the most

disastrous scourge the human race has known, the discoveries of Laveran and Ross have clearly demonstrated the etiology of the disease and have pointed the way to its extirpation. True it is that, owing to the existence in many places of extensive swamps or of rice cultivation, the cost of the necessary measures for the elimination of the Anopheles mosquito seems at present prohibitive; but the improvements and inventions in the campaign against this malign insect which will surely come in time will render practicable the latter's disappearance in at least the most populous areas. Final success may come slowly; it is unreasonable to expect its advent swift as the lightning flash from a summer cloud. By way of contrast to the complexity of this problem stands the case of Malta fever. Here, once it had been ascertained that goat's milk formed the medium of entry of the bacillus into its human host, the prophylaxis was ridiculously easy; it sufficed simply to abstain from goat's milk in order to eradicate the disease. Sleeping sickness, that most gruesome and fantastic of human ills, after decimating the population of Central Africa, is in a fair way to be abolished. The trypanosome which causes it takes, so it has been ascertained, as its secondary host a tsetse fly which fortunately never wanders far from lakes or rivers. Hence by moving the population to a specified distance from such collections of water there is every hope that ere long both human beings (and tsetse flies) will emancipate their bodies from this parasite. Turning to temperate climes, all recognise the enormous gain to human health and happiness wrought in such cities as London or Glasgow, for instance, by measures of sanitation—that is to say, by measures having for their object the prophylaxis of parasitic diseases. In the fall of the death-rate, in the absence nowadays of serious epidemics and in the sinking into oblivion of diseases whose very names once struck terror in the heart of the householder, we may discern the gleam of the triumphant standards of science as they advance against the hosts of disease. Even with diseases such as phthisis, which are as yet far from being under control, science points out certain simple precautions which, for those capable of following them, render this dreaded disease as remote a peril as small-pox to the properly vaccinated. No deeper chasm indeed divides modern freedom of thought and independence of opinion from the superstition of the middle ages than the immunity from

parasitic disease enjoyed by the modern citizen from the pest-ridden existence of his predecessors.

So promising is the outlook in this domain that the final triumph of mankind over parasitic diseases would seem to be trammelled and delayed by two things only. Firstly, there is the cloud of ignorance which still conceals the real etiology of parasitic disease from the great mass of the public, especially in the tropics. The once universal belief in the supernatural origin of epidemic diseases, their ascription to demons, gods and evil spirits, lingers on tenaciously among uneducated people, who, holding this belief, naturally regard with hostile or contemptuous eyes the best designed efforts of sanitary officials. It is this ignorance which lies at the root of the appalling death-roll from parasitic diseases in India and until it is removed by appropriate instruction in the schools and elsewhere no real and permanent progress in their prophylaxis in that country would appear feasible. The second obstacle to ultimate victory lies in the dearth of funds for original research. In spite of some recent donations in England, America and Germany, no one who takes the trouble to realise clearly in his own mind the awful carnage inflicted on humanity by parasitic diseases and the brilliant results already achieved by modern scientific research but must be lost in amazement that, whilst avalanches of money are readily forthcoming for objects that gratify the vanity or subserve the complacence of the wealthy, so little finds its way to furnish the very moderate assistance required by scientific workers. The agonies inflicted by many of these diseases recall the hells of theological imagination ; the hecatombs of lives sacrificed to them in the past, aye even to-day, utterly dwarf the puny efforts at wholesale slaughter of an Attila, a Timour or a Napoleon. Yet the rivers of monetary aid that well so bounteously nowadays from the founts of benevolence and kindness for the most part lose themselves in sterile and unprofitable deserts, only the merest trickle reaching the fertile soil of scientific research. All the more honour then to the hardy pioneers of science who, with scanty encouragement and in the face of great difficulties, have already achieved for humanity such great and permanent alleviation of its torments.

But in the domain of therapeutics the advance made of recent years, whilst not inconsiderable, differs, whether in respect to method or the results achieved, from that in the prophylaxis of

parasitic disease as "Puffing Billy" from a modern express locomotive. The technique in vogue still depends largely on the empirical use of drugs and relies for improvement on the primitive method of progress by trial and failure. Thus the therapeutics of plague consists mainly in the treatment of the symptoms as they occur, in contrast with the more scientific methods of prophylaxis by the elimination of the rat flea or through the injection of Haffkine's serum. But there already exist some commencements at least of treatment on scientific lines that promise important results; witness the discovery of the opsonic index, the new vaccine therapy, or the treatment of phthisis by formalin inhalations. And even on the purely empirical administration of drugs some light has been thrown by recent developments of bacteriology. For instance, whilst it was previously known by experience that the proper time to exhibit quinine in an attack of ague was during the sweating stage, we now know that at this time new crops of malarial bacilli are born and that the occasion is therefore appropriate for a massacre of these innocents.

If the therapeutics of parasitic disease still leaves so much to be desired, what shall we say of the next division of our subject, the prophylaxis of metabolic disease? Progress, if any there be, resembles closely that strategic movement to the rear so dear to unsuccessful military commanders. Anæmia, rheumatism, gout, dyspepsia, diseases of the heart and kidneys, neurasthenia and the whole *Ilias malorum* due to faults of metabolism still flourish amongst us with the vigour of the proverbial bay-tree. According to recent statistics, the incidence of some of them at least, such as the circulatory diseases, so far from exhibiting any sign of check, seems on the whole to show a distinct upward tendency. Others, like appendicitis, threaten to be numbered amongst the accomplishments essential in polite society. People are patched up more effectually and, let us add, more often than seemed the case formerly—else why the large increase in the number of their medical advisers—but as for winning free or partly free from this large group of diseases, that, it would seem, is a consummation so hardly obtainable as to be a mere crying for the moon. With the exception of the prophylaxis by Bulgarian bacilli, the discovery, be it noted, not of a doctor but of a Professor of Bacteriology, no real attempt appears to have been made by the orthodox to avert those ills

by the treatment of which they make their livelihood. A gross fatalism, chill and hopeless as the inscription at the portals of Dante's Inferno, would in this respect seem to brood over and benumb the minds of both the medical profession and the general public. In a recent work on that somewhat depressing locality, the East End of London, the writer thus describes the mental attitude of its denizens towards their unwholesome physical environment : "The factory chimney belches forth obstruction. But no murmur escapes the East-Ender. Smoke in his view is inevitable, part of the ordinary course of nature; and he would as soon think of opposing it as he would of opposing the thunderstorm." That, with all deference, appears to be the present standpoint from which the majority of doctors envisage the majority of this large class of diseases; they prescribe for the symptoms from an overgrown yet continually increasing armamentum of drugs; they will recommend a change of climate, a holiday and so forth; on occasion they even suggest some half-hearted alteration in the diet customary in the patient's particular class; but that the affliction pressing upon him was preventible, that through any acts or abstentions the public generally may attain freedom from such disease or class of diseases, these are ideas wholly foreign as yet to the psychosis of the medical profession. Like simple Orientals at the shrine of Mariamma, the goddess of small-pox, the orthodox medical practitioners and the laity in their train abase themselves with quite pathetic humility before the spectre of metabolic disease.

Perhaps the key to this attitude of sterile pessimism may lie in the very word "laity," so commonly used in the course of medical discussions. Is there not more than a tinge of sacerdotalism in the mental attitude affected by the great majority of the profession; "the air of the priest with the feeling of personal importance, the thin unction and private leanings to the cord and the stake"? Do not too many doctors still regard any discussion of medical matters with members of the public as unprofessional, and do they not too often assail novel ideas as to the etiology of disease with all the acrimony of a mediæval priest? The welcome accorded to Jenner's and Harding's discoveries, to John Brown and to Ignatius Sammelweiss, has many an analogy in modern times. In no other profession are the public styled the laity; no other men of

science guard so jealously from the profane the secrets of their art, for all the world as though they were veritable mysteries of Isis and they the priests of her temple. As an instance of this attitude let us take the famous—is that quite the word ?—manifesto on the use of alcohol issued less than four years ago in the *Lancet*. Previously various investigators, taking different lines of research, with much accuracy, diligence and endeavour to eliminate adventitious factors, had arrived at the conclusion that alcohol, except as a drug, affected injuriously the human organism. Did the signatories to this manifesto refer to or refute the reasoning of these investigators ? Not at all. After a reference to the use of alcohol in medicine— which need not here concern us—they announced with due decorum and solemnity that in their opinion "the universal belief of civilised mankind as to the beneficial results of a moderate use of alcoholic beverages is amply justified." Now that kind of thing may be good theology, but it is uncommonly bad science. Never, indeed, did Council of Trent thunder forth dogmas with greater unction or a more invincible authority than that assumed by these hierarchs of the medical world. (The clerics had, however, this advantage, that whereas their doctrines were enunciated under the solemn arches of cathedrals, this latter-day creed of the medical profession has filtered down to the laity chiefly through the agency of delighted publicans.) In the discussion that followed it seems quite natural and fitting that one physician, naïvely abandoning all reference to modern science, should endeavour to bolster up the case for alcohol with the aid of a text from the Book of Judges. By what abysmal depths is not this fulmination divided from the patient collection of facts, the admission of possible causes of error, the frank and full examination of arguments that distinguish a Darwin or a Pasteur ? Can we any longer feel surprise at the halting progress of medical science when such convincing expressions of opinion, such illuminating arguments are tendered in all seriousness in a scientific journal on a matter of science pure and simple ? Surely it is not through methods such as these that knowledge advances and the spirit of human thought makes wide her boundaries.

In the therapeutics of non-parasitic disease, as distinguished from their prophylaxis, some progress has indubitably been effected. Thanks to a notable advance in diagnosis, errors of

treatment occur much less frequently than of yore ; increased knowledge, mostly, however, of empirical nature, obtains of the uses and dangers of various drugs ; whilst owing to a marvellous and brilliant advance in the surgical art numerous diseases formerly regarded as desperate or hopeless are now cured with ease and certainty. Indeed, the glittering successes of surgery serve in no small measure as a veil to conceal from the public the failures of the medical profession viewed as the custodian of the public health. Were it not for the wonderful advance in the use of the knife rendered possible by the discovery of chloroform and of aseptic methods, diseases of metabolism would claim a tale of mortality and suffering so shocking as long since to have called forth an imperative demand for an effective prophylaxis. As it is, a certain portion of the public, both in this country and in America, are beginning to look askance at a profession which in an age of exceptional scientific progress has failed so conspicuously in the prophylaxis of a large class of diseases, and to seek for themselves some causeway out of the dismal morass of ill-health in which the orthodox view would condemn mankind for ever to wander. They regard with more than suspicion the constantly reiterated explanation of the increase of diseases of the heart, of appendicitis, cancer, lunacy and so forth, as merely due to more accurate diagnosis. The treatment of symptoms by drugs no longer satisfies their aspirations ; they wish to know whether by some radical alteration in the conduct of our lives it may not be possible to avoid absolutely or nearly so all risk of diseases of metabolism.

Not for the first time, indeed, have these by no means unreasonable aspirations cheered and encouraged the minds of men. The fact is that after an interval of many centuries the civilised world is once again beginning to realise the cardinal importance of good health, not only in their happiness, but in their morals and their intellectual outlook, to realise that a healthy body forms a more satisfactory basis for a healthy outlook on life than many tomes of ethics and of erudite dogma. Amongst the ancient Greeks and Romans, especially the former, the care of the body assumed the importance of a religious cult, so much so that regular worship was accorded to the goddesses of health, Hygeia and Salus. Medical science had reached no standard of excellence ; bathing, massage, dieting, in addition to

the more primitive use of drugs, did much to counteract the
evils inevitable in a voluptuous and self-indulgent age. But
with the advent of Christianity a change passed over the scene;
the storm-cloud of the new theology swept over the country
and left it bare not only of the old superstitions but also, alas!
of hygienic knowledge. Salus and Hygeia passed away—
enjoyed fairyland, as the Burmese quaintly say—the practice of
bathing was neglected, and the baths fell into disrepair; since
the body formed *ex hypothesi* the source of evil, all care of it was
naturally contemned as sinful; the rising sciences of medicine
and of hygiene crumbled into ruins and almost disappeared
beneath a weedy outcrop of superstitious charms and magic
observances. When people trusted in all seriousness for the
cure of disease to pilgrimages or a visit to a shrine, they would
scarcely, it will be admitted, regard seriously their treatment or
prophylaxis on scientific principles. But against these untoward
results we must in justice set the gifts brought by the new
religion, namely the institution of hospitals, a tenderer regard
for the poor and the increased sanctity of human life. Nowa-
days, influenced no doubt by the altered mental atmosphere due
to modern science—call it materialistic or not as you will—
men, as already remarked, once more begin to regard bodily
hygiene of at least equal importance with say the " subtleties of
the eastward position," and to take thought how to avoid the
physical evils that so insistently menace them and their
families. And with this increased attention there necessarily
follows a bitter dissatisfaction at the failure hitherto of medical
science to attack resolutely the Hydra of metabolic disease and
a resolve, joined in many with a high hope of success, to win
clear from its poisons and miseries.

It is claimed by the pioneers of this new movement that,
with a properly conditioned physical environment, disease
should be practically unknown ("death from disease is an abomi-
nation," say some of them) and dissolution due to old age after
a span of life much beyond that now accepted as natural
the normal bourne of human beings. And in a consideration
of the circumstances affecting the human body they not un-
naturally attach a special importance to the question of dietary
—that is to say, the kinds and quantity of food necessary to
keep the human body well nourished and in perfect health.
(A person occasionally subject to twinges of gout or rheumatism,

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or whose blood pressure is excessive, or who harbours an
undue amount of anaerobic microbes in his intestines can
hardly lay claim to the latter designation.) The importance
of this branch of science few who have studied the biological
significance of food amongst animals in a state of nature or
the variations in health amongst domestic animals resulting
from altered dietaries would be concerned to deny. Yet the
attitude of orthodox medical men on this crucial matter re-
mains far from satisfactory. In the first place the physio-
logical allowance of food for men in health—recently, be it
noted, seriously impugned by actual experiments in America
—rests on that customary amongst inhabitants of the British
Isles at the present day. Now the consumption of meat per
head in these islands has within the last fifty years more than
doubled itself. Apart then from the questionable propriety of
taking as standards the dietaries in use amongst a people like
ourselves riddled with diseases of metabolism, either the present
allowances of meat are excessive or those customary in the
good old days—before physical deterioration commissions—
were very deficient. Again, according to accepted views on
human physiology and nutrition, what is more clearly demon-
strated than the impossibility of maintaining health and strength
on a diet of rice alone ? Nevertheless there is reliable evidence
that labourers in China, living on such a diet, carry to great
distances loads that an Englishman could not even lift.

     The fact is that in a consideration of the standards of health
and of the causation of disease—when indeed their scrutiny
extends so far—the medical profession are much too prone
to limit their inquiries to the peculiar and special circumstances
of humanity as found in their present day of grace in England,
America, France, Germany and a few other countries. The
diseases—where not microbic—diet, drink, clothing and mode
of life generally of modern civilised man they regard in the
light of established norms as the matrix in which humanity,
or at least civilised humanity, must inevitably crystallise.
Thus, when an English authority defines health as "that con-
dition of structure and function which, on an examination of
a sufficient number of examples, we find to be the commonest,"
we may be quite certain that the examples in question will be
drawn from England and consequently that an unduly high
number of anaerobic microbes in the colon or an excessive

blood pressure will be regarded as compatible with perfect health. The great mass of humanity which lives, thrives and maintains a high standard of physical health under totally different conditions exists, it is true, but to their myopic vision the outlines of the physiology of these peoples appear blurred and indistinct, to them as little worthy of study as would be the course of Halley's comet to a fish. Like the ancient Romans and Greeks or the Chinese until recently, they contemn where they do not ignore the habit of life of the outer barbarian tribes. The absence of any particular nexus between high civilisation and a condition of rude health seems to have wholly escaped their attention; on the contrary not a few seriously connect the present custom of heavy meat-eating with modern intellectual development, one hardy authority even ascribing the lack of enterprise amongst South Italians to the absence of this substance in their dietary. Shades of Plato, of Pythagoras, of the Caliph Omar and hosts of other vegetarian worthies down, we had almost written, to Bernard Shaw !

Setting aside such bizarre suggestions as unworthy of a profession which at least claims to think scientifically, surely to those who decline to accept the commoner non-microbic diseases, like the winter sleet and the summer rain, as unavoidable incidents in human life, the existence of large populations living under the most varied conditions of climate, geographical surroundings, dietary, clothing and dwellings affords an admirable field for the investigation of the real etiology of these diseases. If various populations in which a specified disease is rife have only one outstanding circumstance in common, whilst others in which it either does not occur or occurs only very infrequently have nothing else in common except the absence of that circumstance, why, then, one may reasonably link the causation of the disease with the circumstance in question. What in fact is required is an application to pathology of the method which Dr. Archdall Reid has used with such brilliant effect in respect to the mentality of races. Thus, as he has pointed out, the followers of the orthodox religions are usually inferior to the heretics in intelligence, energy, and initiative, tend in fact, under equal circumstances, to become "hewers of wood and drawers of water" to the latter. This difference is not due to any question of race, for portions of the same race differ in mentality ac-

cording to their religious belief, and, as a matter of history, sudden changes in religious belief have resulted in almost equally sudden variations in intellectual outlook. Similar arguments preclude or minimise the connection between mental capacity or incapacity and climate, geography, soil, situation, etc. Into the reasons connecting religion and a national psychosis we need not here enter. The point is that this method, which is a perfectly logical one, lends itself readily to the investigation of the etiology of disease, since, by taking account only of large masses of men, it avoids pitfalls due to local peculiarities, and at the same time its inductions, based like those of anthropology on data supplied by the whole world, are not liable to refutation by facts drawn from distant countries, as, for example, the English physiological standards by experience amongst the Chinese. A few authors have, it is true, done some excellent pioneer work in the field of geographical pathology; but their investigations, which relate chiefly to zymotic diseases, lack much in exactness and in necessary elaboration of detail, nor do the data collected permit of discrimination between the dietary, clothing, houses and manner of life of the races concerned.

As a concrete instance of the suggested method let us take the case of appendicitis. Certain medical men point out that this disease, relatively common in countries such as England and the United States, with a high consumption of meat *per capita*, is rare, if not quite unknown, amongst wheat- and rice-eating populations, such as the Hindoos and the Chinese, or those, such as the inhabitants of the Balkan States and Brittany, where a minimum of meat is eaten. From this and other facts they argue that a carnivorous diet or at least one rich in purins is an indispensable concomitant of appendicitis. We are not here concerned with the truth or falsehood of this theory, which at any rate, so far as it based on an induction from racial dietaries, is still quite incomplete. But the inquiry proceeds on right lines and, if pushed, should permit of a definite and trustworthy conclusion.

After all the goal of medical science is the maintenance of a high standard of health, not merely in youth but in later years; the prolongation of human life, active and vigorous, into years now abandoned to senility and ineptitude. It is idle to apply the epithet of healthy to people who, however

vigorous their youth, suffer later on from such complaints as gout, rheumatism, Bright's disease, or arterio-sclerosis. Nor will the man in the street greatly laud a learned discussion on the enzymes of the stomach when such discussion wholly fails to point the way whereby he may assuage the pangs of dyspepsia; he does not yearn so much after a knowledge of the histological changes of the kidney in Bright's disease as a method by which this disease may be safely avoided. To many sufferers such discussions must appear as futile as the historic controversy of *Homoous* and *Homoious*, and as empty of benefit to tortured humanity. What kind of opinion should we entertain of gardeners who wiled away their time in acrimonious discussions on the diseases of their plants, and whose utmost endeavour extended only to the temporary cure of their distempers or to the alleviation of their sufferings? Surely we would say: "Study the environment—using the word in the broadest sense—of your plants and so regulate it that these diseases become at least as rare as theft and dishonesty in a well-ordered community. In neighbouring gardens we discern whole masses of plants free from those disorders which plague the specimens under your care; go and examine wherein consist the conditions through which these plants enjoy robust health whilst yours are diseased. These conditions undoubtedly exist; it is for you by patient inquiry and logical induction to particularise them."

And should such a transfiguration of the medical profession dawn on an expectant public, perhaps not the least of its concomitant advantages may be the disappearance of that dark horde of quack medicines which in season and out of season intrude themselves on our unwilling attention. To the cynic few subjects tend more to the gaiety of nations than the execrations and anathemas which the orthodox doctor never wearies of hurling at his heretic brother, the vendor of secret remedies. After the profession has practised the treatment of disease for many centuries by the empirical use of drugs, and thoroughly inoculated the public with the belief that therein lay at once their certain, facile and sole hope of physical salvation, what wonder that others, doubtless ignorant and mercenary—*cela va sans dire*—should trade on the habit of mind thus engendered, and "jump the claim" of the orthodox practitioners? With just as much logic did the mediæval

ecclesiastics, after inculcating as a pious duty the murder, torture and maltreatment of heretics and witches, hold up their hands in horror at the brutalities practised by nobles and kings on those who differed from *their* convictions on details of fiscal and social economy. *Quis tulerit Gracchos de seditione querentes?*

Perhaps also in the not distant future we may see the medical profession finally discard that subtly hierarchic attitude —as though "angels listen when they speak"—which, whilst it impresses so profoundly the female portion of their *clientèle*, accords ill with their position as men of science. The advisability of some such change of attitude is the more urgent since Herbert Spencer had the temerity to allege a common cradle in primitive times for physicians and priests. Indeed, did not the clergy in comparatively recent times monopolise with octopus grip the art of medicine? and did not the Archbishop of Canterbury confer the degree of M.D. so late as 1858? Unless care be taken, evil-disposed anthropologists may trace back sacerdotal leanings amongst modern doctors to the thaumaturgics of the primitive medicine-men. After the doffing of the priestly biretta, and the adoption of a mental attitude more in accordance with the motto *Nullius in verbâ*, we may perhaps no longer find medical men, when writing to support a new theory of eye-strain, not daring to publish their names; nor one well-known man of science describing "the attitude of doctors to everything new as pitiful, not to say disgraceful"; and another affirming them to be in matters of science "just as non-receptive to fresh evidence as the average solicitor or merchant."

Indeed with this altered outlook the very title of doctor may give way to some such designation as officer of health. We have already officers of health in municipalities; why not private officers of health for individuals? Just as the former (concerned primarily with microbic disease) feel as a stigma a high rate of mortality amongst the citizens under their charge, so will it be considered disgraceful in the latter to possess a *clientèle* distinguished by a low state of vitality or prone to metabolic disease. Nor is this all. The public no longer cringing before the least utterance of the priest-physician, but accustomed in matters hygienic to think and act for themselves under the guidance of mere men, but men

of science, will, we may hope, constitute a body of opinion intelligent, watchful and keenly critical of results. They will come to regard the science of hygiene not as something vague and remote like the ethics of a Spinosa or the philosophy of a Hegel, but as a body of exact knowledge the elements of which closely concern every intelligent being—form, indeed, the very woof and weft of the fabric of our happiness. Under the influence of the higher standard of thought and intelligence thus inculcated, there may quite probably arise a public opinion or "herd suggestion" which will regard every grave infraction of the rules of health, every serious disease in the light in which until recently people contemplated theological sin. In this hygienic Utopia the sufferer from chronic ill-health will incur much the same opprobrium as for instance the "open and notorious loose livers" of our forefathers, whilst to be compelled to undergo—save for an accident—a surgical operation, that will rank as a criminal offence stamping the patient with all the stigma of a convicted felon. And since the mind reacts in an amazingly close degree to the health or sickness of the body, we may justly look forward in this Utopia—if indeed such a one be possible—to a higher and brighter spirit in civilised man, with less selfishness and cruelty and a largely increased measure of altruism, public spirit and all that makes for a healthy and prosperous community

REVIEWS

The Theory of Light. By the late THOMAS PRESTON. Fourth edition. Edited
    by W. E. Thrift, M.A. [Pp. xxiii + 618.] (London: Macmillan, 1912.
    Price 15s. net.)

IN this fourth edition of Preston's *Theory of Light* the unique character of the
original work has been jealously preserved. The additions made to the text
include a fuller treatment of dispersion, an account of radiation phenomena in a
magnetic field and a more complete presentation of the electromagnetic theory.
The additions made to the text in these respects and by the description of modern
experimental work amount to some thirty pages but the additions have been
enclosed in brackets in order that they may be distinguished readily from the
original text. The brevity of the description given of recent experiments would
be regrettable but for the fact that they are described in detail in Prof. Wood's
Physical Optics, published in the same series of volumes. Under these conditions
there is every justification for retaining the historical and mathematical form of
Prof. Preston's work, the value and vigour of which are undiminished after twenty-
two years of active service.

                                                               T. M. L.

The Age of the Earth. By ARTHUR HOLMES, B.Sc., A.R.C.S. [Pp. 189,
    illustrated.] (Harper's Library of Living Thought. Price 2s. 6d.)

TWO years ago, Mr. Holmes published a research on the association of lead with
uranium minerals and its application to geologic time.[1] On the assumption (not
yet directly proved) that lead is the final product of the uranium series, and on
several other assumptions, the quantity of lead contained in a mineral affords
some clue to the date when it was laid down. Mr. Holmes's results were
unusually concordant, and, emboldened by his success, he has essayed to treat the
whole subject of geologic time.

    Needless to say, the chapters (in all comprising nearly half the book) dealing
with radioactivity and cognate subjects are the most valuable. A somewhat fuller
account of experiments such as those he has himself carried out would have been
welcome, but this part of his work is clear and carefully written. Nor is he unduly
dogmatic concerning the validity of his own method compared with those of other
workers. There is a danger of our repeating the error of the last generation and
laying too much stress on the validity of physical methods of investigation. In
place of the dogmatism of Lord Kelvin and Prof. Tait, we are liable to substitute
that of modern exponents of radioactivity. But such an attitude, if it occurs,
will not be favoured either by Prof. Strutt or by his pupil Mr. Holmes.

    Nevertheless, Mr. Holmes, having reached the conclusion that many minerals
were laid down 1,500 million years ago, is bound to try to correlate other lines of
evidence, and to attempt to show that, if rightly understood, they support his
view. He has against him the fact that the greatest modern authorities, arguing
from many diverse lines of thought, have repeatedly stated that 100 millions of

              [1] *Proceedings of the Royal Society*, Series A, April 11, 1911.

years is ample to account for geologic phenomena. Prof. Sollas was satisfied with 26 millions of years, and, though his recent work shows some sign of a modification of that opinion, the discrepancy between the results is great and glaring.

On this side, Mr. Holmes's work must be described as weak. He neither proves his case nor, in attempting to do so, does he make the best use of the materials at his disposal. A considerable portion of the book may be dismissed as padding. Pictures and descriptions of spiral nebulæ, and of the polar caps of Mars, look very pretty in a semi-popular work, but they have the remotest bearing on the matter in hand. Mr. Holmes is an advocate of Prof. Chamberlin's planetesmoid hypothesis. He thinks that, *after the first sediments were formed* (p. 31), the Earth was still growing by reason of the capture of planetesmals. The speculation seems exceedingly improbable, and, indeed, we are entitled to ask why we find no traces of the occurrence in the earliest sedimentaries, but this and others matters we may pass by as side issues and irrelevant.

To come to the sections that really matter, the problem of the duration of solar heat presents the greatest difficulty. Mr. Holmes could not be expected to make much of this. At the time his book was written, no adequate theory of the subject was published, though there have been vague anticipations in articles by the Messrs. Jessup[1] and others. Mr. Holmes accepts Prof. Arrhenius's idea of the existence in the Sun of compounds which contain vast stores of energy due to exceptional conditions of great heat and pressure (p. 119). There is no space to criticise this view. It will be sufficient to point out that it is entirely inconsistent with the planetesmal hypothesis, because the planetesmals, *ex hypothesi*, are not subject to great heat and pressure. Chamberlin's planetesmals and Arrhenius's internal heat certainly form a curious eclectic mixture.

The other points that call for attention are Prof. Joly's researches on the saltness of the sea, and Prof. Sollas's on the thickness of the sedimentary rocks. With regard to neither of these does Mr. Holmes appear to be aware of recent literature. As a chemist, Mr. Holmes ought to know something of the special liability to error of the average sodium analysis of river water, especially when (as is usually the case) no particular trouble is taken to assess it with the necessary accuracy. There is a continual tendency towards unduly high results. The fact has been pointed out repeatedly by Mr. Acroyd, Prof. Dubois, and myself.[2] Nor does Mr. Holmes appear to realise the cumulative effect of the errors. Mr. Holmes's conclusion that the quantitative deductions are purely provisional is correct, but his reasons are very inadequate.

Nor, in his discussion of Prof. Sollas's theories of sedimentation, is he much happier. Prof. Sollas is a geologist of the highest rank, and certainly deserves the compliment of detailed refutation. On this matter, Mr. Holmes's view, which he supports by a private communication from Prof. Chamberlin, is that land radients to-day are much higher than the average, and that, consequently, the

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[1] *Philosophical Magazine*, January 1908.
[2] Particularly in the following papers : 1. *Proceedings Geological Society Yorkshire*, 1902 (on Cyclic Salt); 2. *Chemical News*, 1901 (Discussion between Mr. Acroyd and Prof. Joly) ; 3. *Proceedings Amsterdam Academy*, 1902 (On the Ratio between the Sodium and the Chlorine in the Salts carried by the Rivers into the Sea) ; 4. *Chemical News*, May 30, 1909 (On the Sodium and the Chlorine in River and Rain Waters) ; 5. *Journal of Geology*, 1910 (The Age of the Earth and the Saltness of the Sea) ; 6. *Contemporary Review*, February 1911 (Modern Theories of Geologic Time). The latter paper also contains a criticism of Prof. Sollas.

sediments now brought to the sea are from nine to fourteen times as great as those of other geologic epochs. Past experience in matters geological teaches us to regard with great suspicion theories that require a departure from the hypothesis of practically uniform conditions. What Prof. Chamberlin's opinion may be is known only to himself, but, in a recently published paper on the subject, he assesses the lower Cambrian as, roughly, 75,000,000 years ago.[1] In any case, it will be sufficient to point out that this argument is not available against Prof. Sollas. Prof. Sollas's results refer to the *maximum* thickness of sedimentary rock, and it is absurd to suppose that the fastest accumulation of sediment, presumably representing the steepest land gradients, has, on that account, proceeded nine to fourteen times more slowly than under current conditions. The average relief of the land has no bearing on the subject. Prof. Sollas's arguments are valid as against any that Mr. Holmes has brought forward. As a matter of fact, an attempt at a detailed refutation has been published, but Mr. Holmes does not appear to be aware of it.

With all the faults, however, there is some value in the publication of a book on the subject by one specially competent to speak from the standpoint of radio-activity, and we can echo his wish that the work will stimulate an interest in the time problem, and provide material for further discussion.   H. S. SHELTON.

Problems of Life and Reproduction. By MARCUS HARTOG, M.A, D.Sc., F.L.S., F.R.H.S., Professor of Zoology in University College, Cork. [Pp. xviii + 362.] (London : John Murray, 1913. Price 7s. 6d. net.)

DR. HARTOG'S book is, actually, a collection of essays published, from time to time, in the leading scientific and popular journals. It is intelligible to those having no special knowledge of the subject matter, admirably discursive, and yet possesses a unity of its own. In such a work it is not easy to emphasise the salient points of interest to the general reader. It may be regarded as the epitome of the biological writings of a lifetime. The three features that stand out most prominently are, perhaps, the pronounced neo-Lamarckian tendency, the Spencerian attitude towards biological problems, and the appreciation of the biological writings of the late Samuel Butler. All of these are of interest and value. Each one, separately, would tend to give the writer a special position among English biologists, and all three combined make his position distinctive and unique. Fashions in biological theories change continually, and in every instance Dr. Hartog has the distinction of maintaining the point of view that is not, at the present time, fashionable, and he does so with a wealth of knowledge and a clearness of exposition that ensure him a hearing both from biological specialists and from the general intelligent public.

The first two features are, perhaps, but aspects of the same. No clearer or more consistent statement of the so-called neo-Lamarckian view than Spencer's is to be found in modern literature ; indeed, in its modern development, it might more correctly be described as neo-Spencerian. Dr. Hartog is a worthy successor. The uncritical and unphilosophical dogmatism of present-day neo-Darwinian biologists, though masked, for the time being, by the rise of Mendelism, requires a corrective, and Dr. Hartog admirably supplies the need. It is difficult, in a brief review, to summarise or to criticise Dr. Hartog's arguments or to make any original contribution to the discussion. The following extracts will illustrate his point of view :

"We must consider what is the *à priori* ground that has led naturalists,

[1] *Nature*, vol. liii. p. 80.

themselves not wholly devoid of that merit and reasoning power which they deny to their opponents, to assert the impossibility of such transfer. The reproductive bodies are not formed of a secretion in which the whole organism takes a part : in complex animals they are cells set apart at a very early stage in the development of the individual, and take no direct share in the life of the parent, which may almost be said to play the nurse to them in the way of feeding them ; to push the view to an extreme, the reproductive or germ-cells are *in* the body but not *of* it. . . . Now these reproductive cells may be fed and grow and multiply at the expense of the nourishment brought to them by the organism in which they lie; but, so far as we know, there is no nervous apparatus connecting them with the body, to influence them ; and without nerves we know of no transmission of impulse in animals. Therefore, for the majority of adaptations, there is no *ascertained mechanism* of transfer from the soma to the stirp, and as a consequence there *can be no transmission.* This assumes the canon : 'No mechanism can exist that escapes the modicum of knowledge that we have gained during the century and a half or so that we have had to learn physiology '" (pp. 180–1).

This is one of the reasons which have led so many to deny the possibility of the inheritance of acquired characters. Dr. Hartog certainly does not overstate his case. Indeed, it is easy to go a step further and to ask whether, in normal instances, the reproductive cells *do* separate from the body soon enough to justify the fundamental Weismannian distinction between stirp and soma. In most of the cases when such a phenomenon has been noted (*e.g.* the aphides) there are special biological reasons why it should be so. Nor is dogmatism based on our ignorance of physiology the only factor to which the bias is due. The neo-Darwinian theory is specially useful to those who advocate a very narrow and mechanistic view of evolution. Also, as Dr. Hartog has briefly noted (p. 178), the view that Natural Selection is the sole and only cause of evolution has become the stock-in-trade of a certain class of political theorists, of whom Mr. Benjamin Kidd is the chief spokesman. Because Natural Selection amongst individual human beings has, by modern civilisation, been reduced to a minimum, therefore it must be transferred to groupings, therefore the group is all-important, therefore the individual must be subordinated in every possible way, therefore follows socialism or cheap imperialism according to the bias of the individual. It is absurd to suppose that considerations of this kind have been wholly without influence in biological circles, especially among the more popular writers who have no claim to rank high in the biological world.

To bring the question back again to the basis of fact and pure science is exceedingly difficult. What is an acquired character? Whatever observations may be made, whatever experiments may be performed, there is always a loophole for the surmise that a character which has all the appearance of being a true case of the transmission of the effects of use and disuse is either not inherited or not acquired. Moreover, on any hypothesis, there are cogent reasons for such transmission being slow and gradual. The difficulty of proof thereby becomes greatly enhanced. But the neo-Darwinian school, which, it is as well to emphasise once more, did not include Darwin, is not entitled to claim the involved character of the facts and the extreme difficulty of correct interpretation as a proof of their view. The searching criticisms of a competent biologist such as Dr. Hartog are very valuable to enable us to realise that much of this current so-called science is, at the best, rash theorising, at the worst palpable pseudo-science.

The Spencerian leanings of the book are not confined to the neo-Lamarckian controversy. In many other ways Dr. Hartog shows an appreciation of the wider philosophical view of biology of which Spencer has been the greatest representa-

tive. The theories of physiological units, of the limitation of the size of land animals, and others of less general interest receive careful attention and criticism.

The exposition of the biological writings of the late Samuel Butler has a peculiar interest of its own. It is a strange fact, with all our professorships and other direct or indirect forms of endowment of research, that so much of the advancement of knowledge, in the things that really matter, is due to outsiders whom the scientific world is careful to ignore. Afterwards they are dragged into the light in a way which they would probably not appreciate. The case of Mendel is, perhaps, not surprising. A modest unassuming monk, who loved his experiments, and neither sought for nor desired recognition, had nothing to gain by self-advertisement.[1] But Samuel Butler was by no means disposed to hide his light under a bushel. And now we find a first-rate biologist telling us that *Erewhon* was not his only achievement, but that his biological writings were really scientifically valuable. Dr. Hartog traces his influence in Romanes and others, and is unable to explain why *Life and Habit* missed its mark. Bergson is not mentioned. The Bergsonian boom had not started when most of these essays were written. But it is interesting to note that the only part of Bergson's evolutionary theories which have any particular scientific interest or value—*Matter and Memory*—is strangely reminiscent of Samuel Butler's work on unconscious memory.

There is much else of interest in this collection of essays. The article on nature study should be valuable to teachers. Here, as in other instances, Dr. Hartog is a pronounced opponent of fads. Avoid pseudo-science, is the burden of his remarks. Do not call carbon dioxide chalk stuff gas, and do not teach more than you can help which will have to be unlearned afterwards. The articles reprinted from the *Quarterly Journal of Microscopic Science* should interest the technical biologist. But the admirable discursiveness, though interesting to the reader, is embarrassing to the reviewer. The book is a distinct addition to the series, and the essays are well worth reprinting in permanent form.

<div align="right">H. S. SHELTON.</div>

Reduction of Domestic Flies. By EDWARD HALFORD ROSS, M.R.C.S., L.R.C.P. [Pp. 98, 18 illustrations.] (London: John Murray. Price 5*s.* net.)

THIS work emanates from the researches so generously organised by Mr. John Howard McFadden and is written by Mr. E. H. Ross, who was formerly Health Officer of Port Said and is now connected with the researches referred to. The book deals with the whole subject of Domestic Flies chiefly from the sanitary point of view. The author (my brother) is one of the few Englishmen who have conducted large-scale work against insect pests. While at Port Said he commenced and carried through a campaign of extermination against the mosquitoes which used to abound there in very large numbers—chiefly *Stegomyia* and *Culex*. The work was of great difficulty because the town contained a large mixed population of many nationalities and possessed neither sanitary laws nor traditions; and the result was a very complete and brilliant success—in fact, I think the greatest success which has been obtained in British possessions. Mr. Ross s therefore peculiarly well qualified to speak on the practical reduction of flies, and his book deals with the subject, not only from an entomological point of view, but, what is very different, from the Health Officer's standpoint.

The method of breeding house flies and proposals for their reduction have

[1] Reference to the *Catholic Encyclopædia* elucidates the fact that even Mendel was somewhat bitter at the manner in which the scientific world ignored his discoveries.

really been before the public for about fifty years, and many books have been
written on the subject. These are usually, however, more academical than
practical; and the present book will therefore be particularly useful in the
latter direction. Mr. Ross is very gentle with the authorities in that he
attributes the absence of practical measures mostly to ignorance. Stupidity is
generally the appropriate word. People who are pestered by flies in any part of
the world ought to retort by pestering the local Sanitary Magnates in return.
As the author explains, this is the only way of having attention paid to abuses.

R. ROSS.

BOOKS RECEIVED

(Publishers are requested to notify prices)

Man's Place in the Universe. A Study of the Results of Scientific Research in
Relation to the Unity or Plurality of Worlds. By Alfred R. Wallace, O.M.,
LL.D., D.C.L., F.R.S., etc. New and Cheaper Edition. London:
Chapman & Hall, Ltd., 1912. (Pp. 283.)

A Text-Book of Experimental Metallurgy and Assaying. By Alfred Roland
Gower, F.C.S., Lecturer in Chemistry and Metallurgy to the Educational
Authority, Barrow-in-Furness. London: Chapman & Hall, Ltd., 1913.
(Pp. xiv, 163.) 3s. 6d. net.

Continuous Beams in Reinforced Concrete. By Burnard Geen, A.M.I.C.E.,
M.S.E., M.C.I., Consulting Engineer. London: Chapman & Hall, Ltd.,
11, Henrietta Street, W.C., 1913. (Pp. 210.) 4to, many tables and
diagrams. 9s. net.

Experimental Domestic Science. By R. Henry Jones, M.Sc., F.C.S., Head of the
Chemical Department, Harris Institute, Preston; Lecturer in Science, School
of Domestic Science, Preston; Dalton Chemical Scholar, Manchester
University; Assistant Examiner in Elementary Science and Chemistry to the
Central Welsh Board. London: William Heinemann, 1912. (Pp. ix, 235.)
2s. 6d.

A very interesting and useful little book.

Penal Philosophy. By Gabriel Tarde, Late Magistrate, and Professor in the
College of France. Translated by Rapelje Howell, of the New York Bar.
With an Editorial Preface by Edward Lindsey, of the Warren, Pa., Bar, and
an Introduction by Robert H Gault, Assistant Professor of Psychology in
North-Western University and Managing Editor of the *Journal of Criminal
Law and Criminology.* London: William Heinemann, 1912. (Pp. xxii, 581.)
20s. net.

Wireless Telegraphy. By C. L. Fortescue, M.A., Professor of Physics, Royal
Naval College, Greenwich. Cambridge: at the University Press, 1913.
(Pp. vi, 143.) 1s. net.

For "the reader who, possessing a general scientific knowledge, is anxious
to know something, not only of the accomplishments of wireless, but also of
the means by which they are attained."

The Wanderings of Animals. By Hans Gadow, F.R.S., Lecturer in Advanced
Morphology in the University of Cambridge. Cambridge: at the University
Press, 1913. (Pp. vi, 150.) 1s. net.

The Religion of the Open Mind. By Adam Gowans Whyte, B.Sc., Author of
"A Comedy of Ambition," "The Templeton Tradition," "Yellowsands,"
With Foreword by Eden Phillpotts. London : Watts & Co., 17, Johnson's
Court, Fleet Street, E.C., 1913. (Pp. xi, 191.) 2s. 6d. net.
    An excellent essay upon the scientific attitude.

The Science of the Sciences. Constituting a New System of the Universe which
Solves Great Ultimate Problems. By H. Jamyn Brooks, Author of "The
Elements of Mind." London : David Nutt, 17, Grape Street, New Oxford
Street, W.C. (Pp. ix, 312.) 3s. 6d. net.

The Britannica Year-Book, 1913. A survey of the World's Progress since
the Completion in 1910 of the Encyclopædia Britannica, Eleventh Edition.
Comprising A Register and Review of Current Events and Additions to Know-
ledge in Politics, Economics, Engineering, Industry, Sport, Law, Science, Art,
Literature, National and International, up to the end of 1912. Edited by
Hugh Chisholm, M.A., Oxon., Editor of the "Encyclopædia Britannica." The
Encyclopædia Britannica Company, London ; The Encyclopædia Britannica
Company, New York, 1913. (Pp. xliii, 1226.) Price 10s. upwards according
to binding.
    Begins with diaries of important events during 1911 and 1912, and contains
a series of articles on important developments during 1912 in politics, science,
art, archæology, philosophy, engineering, and information on and statistics
of the principal countries.

Researches on Irritability of Plants. By Jagadis Chunder Bose, M.A., D.Sc.,
C.S.I., Professor, Presidency College, Calcutta. With Illustration. Longmans,
Green & Co., 39, Paternoster Row, London, New York, Bombay, and
Calcutta, 1913. (Pp. xxiv, 375.) 7s. 6d. net.

A Beginner's Star-book. An Easy Guide to the Stars and to the Astronomical
Uses of the Opera-Glass, the Field-Glass and the Telescope. By Kelvin
McKready. With Charts of the Moon, Tables of the Planets, and Star Maps
on a new plan. Including 70 Illustrations. G. P. Putnam's Sons, New
York and London. The Knickerbocker Press, 1912. (Pp. 148.)

Annual Magazine Subject-Index, 1912. A Subject-Index to a Selected List of
American and English Periodicals and Society Publications not Elsewhere
Indexed. Edited by Frederick Winthrop Faxon, A.B. (Harv.). Compiled
with the co-operation of Librarians. Boston : The Boston Book Company,
1913. (Pp. 299.)

Fortschritte der Naturwissenschaftlichen Forschung. Edited by Prof. Dr. Emil
Abderhalden, Direktor des Physiologischen Institutes der Universität Halle
a.S. Achter Band. Mit 217 Textabbildungen und 1 Tafel. Urban &
Schwarzenberg, Berlin N., Friedrichstrasse 105b; Wien, I., Maximilian-
strasse 4. 1913. Contents. The Present Position of Research in Metallurgy,
by Doz. Dr. W. Guertler, Berlin-Grunewald. Our Knowledge about the
Oldest Tetrapods, by Prof. Dr. F. Broili, Munich. The Scientific and Economic
Importance of Pond Management, by Doz Dr. Walter Cronheim, Berlin.
About the Galls in Plants (New Results and Discussions of General
Cecidology), by Prof. Dr. Ernst Kuster, Bonn a. Rh. Propagation, Mating,
and Spawning of Fresh-Water Insects, by Dr. C. Wesenberg-Lund, Hillerod
(Denmark). Architecture and Earthquakes, by Prof. Dr. F. Frech, Breslau.
(Pp. 308.)

NOTES

Professor Nathaniel Henry Alcock, M.D., D.Sc.

Almost at the moment of going to press, news reaches us of the death, in Montreal, at the early age of forty-two, of Dr. Nathaniel Henry Alcock, Professor of Physiology at McGill University, who was, with Mr. W. G. Freeman, one of the Editors of SCIENCE PROGRESS (New Series) at its start. His work for science was considerable and valuable; but of that it is impossible to speak adequately in this passing note. Of his personal qualities and his eagerness for the success of this periodical we can testify with cordial appreciation and gratitude. He proved himself in those difficult pioneer years keen and painstaking, genial and charming; his death will be regretted by all who have known him.

The University of Bristol

The affairs of this young University continue to receive some attention in Parliament and in the press. *Prima facie*, there would appear to be some division of opinion between the business and academical elements of the University as to which shall have the predominant voice in its administration. At an early stage, the services of one of the professors who was most active in the foundation of the University were, it is alleged, dispensed with by some indirect procedure; and, later, the Council bestowed a number of honorary degrees, of which a considerable proportion fell to the share of members of their own body. Lastly, the services of another member of the staff who objected to these and other proceedings have also, it is said, been dispensed with. A memorial concerning the case of the professor referred to, signed by a large number of men of eminence, was forwarded to the Chancellor of the University, but was, we understand, referred by him to the Visitor, who, we also understand, has referred it again to the existing constitutional machinery for dealing with such complaints; but it is doubted by some whether this machinery is competent to conduct an independent and impartial inquiry. The case, especially as regards the very generous distribution of honorary degrees, appears to be a serious one; and the progress of it should receive close attention from all scientific workers. Academic life is by no means too prosperous in this country; and it will become even less so if it is not carefully protected against such proceedings as those which are alleged to have occurred in this University.

NOTICE

THE EMOLUMENTS OF SCIENTIFIC WORKERS

IT is proposed to undertake an inquiry regarding the pay, position, tenure of appointments, and pensions of scientific workers and teachers in this country and the Colonies. The Editor will therefore be much obliged if all workers and teachers who hold such appointments, temporary or permanent, paid or unpaid, will give him the necessary information suggested below. The figures will be published only in a collective form and without reference to the names of correspondents, unless they expressly wish their names to be published. The Editor reserves the right to publish or not to publish any facts communicated to him. Workers who are conducting unpaid private investigations must not be included. The required information should be sent as soon as possible and should be placed under the following headings :

(1) Full name
(2) Date of birth. Whether married. Number of family living
(3) Qualifications, diplomas, and degrees
(4) Titles and honorary degrees
(5) Appointments held in the past
(6) Appointments now held, with actual salary, allowances, fees, and expected rises, if any. Whether work is whole-time or not
(7) Body under which each appointment is held
(8) Conditions and length of tenure
(9) Pension, if any, with conditions
(10) Insurance against injury, if any, paid by employers
(11) Family pensions, if any
(12) Remarks

THE BUSINESS AFFAIRS OF SCIENCE

THAT the time has come for a serious stock-taking in the business affairs of science is recognised by all scientific men—that it is a task long overdue is apparent to many. During the last century the whole position of scientific work in relation to other forms of human effort has changed. Science is no longer merely a gentle preoccupation for the leisured and intelligent few—for the philosophers of the Cephissus, the rural school-master, the university recluse, the physician, or the well-to-do amateur. It was, indeed, these who made the beginnings of science, and their work was great; but on the foundations laid by them an edifice has grown up which it is beyond their unaided powers to carry further towards completion with the rapidity required to-day. Science has now become an industry. It has indeed become the premier industry of all. It has grown to affect every other industry and occupation of men. Mathematics leavens not only navigation and engineering, but all the other sciences, and is coming in these days to take possession of physics and chemistry, and even of epidemiology. In their turn, chemistry and physics enter into the very being of almost all manufactures, and of physiology and medicine. Physiology, zoology, and chemistry form the basis of the daily work of the physician and surgeon. Chemistry and botany revolutionise agriculture, and geology and mineralogy illuminate mining. Nothing new can be done without a call upon some branch of science—often upon some quite unexpected branch of it. The wonders of modern invention—steam-engines, artificial lighting, photography, the phonograph, the telephone and telegraph, X-rays, wireless telegraphy, motor-cars, aeroplanes, new fire-arms, aseptic surgery, scientific medicine, hygiene, and agriculture—have produced a greater revolution in the world than has ever occurred before as the result of the widest tribal movements, the most decisive battles, and the most elaborate politics—the change made during recent centuries is greater than that made during all previous known periods of the past

put together. After all, the common life of two centuries ago differed little from that of previous civilised periods, such as the great ages of Greece and Rome. Since then we have suddenly become endowed with a hundred new powers which were unthought of before—and with new outlooks upon the past, the present, and the future.

The complaint has been made that science furnishes us only with petty utilities, and adds nothing to happiness, character, or greatness of mind. But this is the opinion of those who have never climbed the heights of science to see the view disclosed from that summit. The mere utilities themselves affect both happiness and character. The humble bicycle possessed by the modern workman enables him to see something of the world which was never seen by his pedestrian ancestor. Mechanical transit is probably a better educator than some schoolmasters, and the happiness and self-confidence of every civilised man are vastly increased by the consciousness of scientific knowledge. If we have no access of magnanimity, it is not the fault of science, but rather of defects which science may hope to remove. Some one once said that a knowledge of the stars is of no consequence to any of us, and that the Greeks were happy without possessing it; but what would not the ancient Greeks have given to have seen what we can see in the heavens to-day? Science not only makes us "lords of little things," but lifts us into higher spheres of truth. It is constantly recalling philosophy to fact; and gives, or ought to give, more concreteness to art. It has revolutionised the military arts; and it should revolutionise politics. It brings the ends of the earth together, and mingles humanity in a manner which was undreamed of a century ago.

The gifts of science, unlike those of war and politics, are not given to a single tribe and to a single generation, but to the whole civilised world and to all time, until "the future dares forget the past." But they also affect each nation separately. It is scarcely too much to say that the overwhelming superiority in power and influence of a few nations of to-day is due, not perhaps to their physical or moral superiority, nor even to the intellectual superiority of their individual citizens, but to the greater scientific knowledge which these nations possess. It is to be doubted, for instance, whether we could excel in arms and conquer savage tribes merely by our personal bravery or

physical strength.   It has seldom been the general or the soldiers who have won the victory so much as the men who invented their rifles and cannons.   Thus science possesses a distinct political potentiality—it gives hegemony to the nations which possess it and leaves nations, like individuals, which do not possess it in a backwater of failure and poverty. Efficiency in science is not merely an academical asset, but a practical and national one.   In the great international competitions of to-day, whether in armaments, policies, industries, or even sport, the possession of scientific knowledge and especially of scientific modes of thought furnishes the deciding factor.   And this international struggle is a part of the evolutionary scheme of nature.   Nations no more than individuals can be allowed to remain ignorant, sluggish, and unscientific. Like individuals, they must train all their faculties, or else they will suffer in the future as indolent nations have invariably suffered in the past.   Their rivals of to-day are apt to become their enemies of to-morrow, and possibly their conquerors of the day after.   There are those who shudder at all ideas of contention, and who would have the world be a pleasant garden for non-competitive angels; but the world must be taken as it is; and, so far as we can ascertain, rivalry is the only instrument which nature possesses to maintain racial efficiency.

At two points science goes outside direct utilitarian effort. The study of disease and of its prevention and cure has become a sacred obligation for all the nations; and, secondly, science trains the mind to better ways of thinking.   Science is not merely common sense.   Her judgments are not merely like those of the law courts, which consider only the evidence placed before them, and which are prone to "rule out" this or that fact as being irrelevant to the issue.   She must collect her own evidence; with her scarcely any fact can be altogether irrelevant to the issue; and often with her the trial is always proceeding and the final judgment never given.   She has learnt, and she teaches, humility in decision.   The happy jingoism of dogma should not be hers.   She has learnt, and she teaches, the necessity for the infinite preparation of evidence and the infinite distrust of personal opinion.   Her methods, unlike those of the dogmatist, have been justified by her wonderful successes; and it will be good if these methods were more employed in every line of human thought.

The early founders of science, the great amateurs, were sublime figures ; but, though we may still hope for such powerful assistance as they gave, the fact is that science now needs professional service in every branch. If science has become the first industry, then for rapid progress it should be treated as such. Our policy should direct itself towards perfecting the organisation which makes most for science—the scientific education of the individual and the national encouragement of scientific work. We must ask, what is the world doing to render more smooth the machinery of scientific thought and investigation, and what part does our nation play in this great world-work? Men of science are apt to think that their duties extend to no more than investigation. But, if they are wise, they will attend also to the means by which great investigation is to-day rendered possible. They will unite to insist that proper attention be paid to science, that disabilities be removed, and that enough means be provided. The first duty of individuals and of nations is to see to their own efficiency, and the first duty of science is to see to hers.

THE SANITARY AWAKENING OF INDIA

By SURGEON-GENERAL SIR CHARLES PARDEY LUKIS, K.H.S.,
K.C.S.I., M.D., F.R.C.S.

Director-General, Indian Medical Service

In the admirable address with which the Hon. Mr. S. H. Butler opened the proceedings of the First All-India Sanitary Conference, held at Bombay on November 13 and 14, 1911, he said : " The basis of all sanitary achievement in India must be a knowledge of the people and the conditions under which they live, their prejudices, their ways of life, their social customs, their habits, surroundings and financial means."

This was emphasised by me in a memorandum which I laid upon the table at the meeting of the Imperial Legislative Council, held at Simla on September 15, 1911. In this memorandum, which dealt with the measures taken for the suppression of plague and malaria in India, I pointed out that although the important discoveries and the vigorous prophylactic efforts that had been made in India had resulted in a very accurate knowledge of the measures necessary for the control of the above-mentioned diseases, even a modicum of success in effective prevention could not be hoped for unless the people themselves were willing to co-operate wholeheartedly in the campaign. I stated moreover that, in my opinion, this active co-operation will not be secured until the people have learned to understand and to have faith in the principles on which these preventive measures are based, and that their education on these matters is a primary and essential condition of success.

No one unacquainted with the conditions of life in tropical or subtropical countries can have any idea of the difficulties that beset the path of the sanitary reformer in a continent of such vast size as India. The illiteracy of the vast majority of the population, their prejudices, their conservatism and suspicion of innovation or change, their fatalism, and their ignorance and disregard of the most elementary rules of domestic and personal

hygiene, all combine to form an insurmountable obstacle to rapid progress in sanitary matters.

The life of the Indian peasant is one long struggle with his environment. The extremes of heat and cold to which he is subjected have led to the adoption of a type of dwelling which from the sanitary standpoint leaves everything to be desired. The question of ventilation is never considered. In both towns and villages the houses, originally crowded together for purposes of defence, still remain in the same undesirable juxtaposition even though the necessity for crowding no longer exists. Cattle and other domestic animals live in close contact with human beings, and water is used indiscriminately for drinking, washing, and bathing. Lastly it must be remembered that more than 75 per cent. of the population live " on the land," leading a hand-to-mouth existence, and being absolutely dependent on climatic conditions, especially rainfall, for their very existence. Is it surprising, therefore, that their resistance to disease is lower than that of the European, or that, when an epidemic breaks out amongst a community living under such conditions, it spreads with lightning rapidity, and is difficult to control ?

What I have written above will enable the reader to appreciate the enormity of the problems before us. Sanitary measures possible and effective in the West are not necessarily possible and effective in India. We must work out our own sanitary salvation. The difficulties before us are many. The ignorance and even hostility of the masses are still fundamental obstacles. But a thousand difficulties need not dismay us. On all sides there is evidence that the more enlightened minds in India have awakened to the importance of sanitation, and the movement in its favour is steadily gaining ground. Both in the Council Chamber and in the columns of the Indian Press constant demands are made for the three great essentials—pure water, pure food, and pure air, and, as the Hon. Mr. Sivasawmy Iyer said in a recent speech, a very hopeful feature in the situation is that the sanitary consciousness of the people themselves has been aroused.

This sanitary awakening of India I regard as one of the most important developments of recent years, and one which is fraught with infinite possibilities for the future. Once we have the people with us, instead of against us, the work of sanitary reform will advance by leaps and bounds, especially as regards

the avoidance, prevention, and suppression of those four great scourges—plague, malaria, cholera, and dysentery—in dealing with which we are hopelessly handicapped without the assistance and co-operation of the Indian public. Herein lies the importance of education of the masses. I shall devote, therefore, a few lines to a short account of certain recent developments in the educational policy of the Government of India, to which allusion was made by Mr. Montague in his Indian Budget speech on August 7 last. In a resolution dated February 21, 1913, the Government of India drew attention to three matters in which education in the past has been imperfect. One of these was the teaching of hygiene in schools and colleges, and attention to the personal hygiene of the students. With a view to remedying obvious defects and ensuring practical instruction, the Education Department has commended to local Governments a thorough inquiry, by a small committee of experts, into school and college hygiene; the scope of the inquiry to comprehend not merely medical inspection, but likewise the inclusion of practical instruction. For various reasons it is considered desirable to make these courses of instruction voluntary, at any rate in collegiate institutions, and it is felt that if such courses are voluntary it would be as well to introduce the influence of some external agency, which by its reputation and its rewards will be able to encourage private endeavour. Such an agency already exists in the St. John Ambulance Association, which might well provide the initial stimulus, appealing strongly, as it does, to both teachers and taught. Domestic hygiene is now a recognised branch of the Association's work, and on this subject useful literature and instruction could be supplied to the schools. Instruction in "first-aid" might also be given, and active workers in the provincial branches of the Association would be encouraged to afford assistance in the inspection of pupils and of school premises, and in giving practical instruction in all matters connected with personal hygiene. It is also suggested that special training in hygiene should form part of the curriculum for teachers.

The practical details of the scheme will be worked out when reports have been received from the Committees of Inquiry which may be appointed by local Governments: meanwhile the Government of India have approached the Executive Committee

of the Indian Council of the St. John Ambulance Association, saying that they would be glad to receive their views on the points raised, and asking whether the Executive Committee are willing that the Association should be enlisted in a work which it is believed may ultimately prove one of far-reaching importance in India.

As a member of the Executive Committee of the Indian Council, I know that this matter has already engaged their serious attention. I have also had an opportunity of discussing the case informally with the authorities at St. John's Gate, so that I have no doubt as to the favourable nature of the reply which will be sent to the Government of India, and I am confident that, in the near future, we shall be able to work out a scheme which will have a lasting effect upon the welfare of future generations of our Indian fellow-subjects, not only by increasing their knowledge of preventive measures, but also by improving their general standard of health and raising their powers of resistance against disease.

Meanwhile the Government of India is actively engaged not only in remedying sanitary defects, but in studying the conditions and circumstances which affect mortality and the increase and decrease of populations, as well as the relative effects of personal environment and of the social and economic conditions in the different parts of the Indian Empire. Want of space prevents me from discussing the various recurring and non-recurring grants made under the head of Sanitation or from enumerating the numerous important sanitary schemes which have been carried out during the past few years. It will suffice if I state that during this year and last year recurring grants of £261,000 and non-recurring grants of nearly £1,500,000 have been made, the bulk of which are intended for schemes of urban sanitation ; also that the Budget estimate of expenditure under this head for the current year comes to nearly £2,000,000, showing an increase of 112 per cent. over the expenditure of three years ago. Nor have the claims of rural areas been overlooked. Assignments have been made to local Governments to enable them to forgo the amounts which at present are appropriated for provincial use from the cess on land. This will increase the resources at the disposal of local bodies, and it is hoped that it will lead to a great improvement in village sanitation and especially to the provision of a pure water supply and its adequate

protection from pollution. For further details I must refer the reader to the Annual Reports of the Sanitary Commissioner with the Government of India and to the various Blue Books presented to the House of Commons, and I shall devote the remainder of this article to a description of the work done by the General Malarial Committee and the Indian Research Fund, and to an account of the inauguration of the All-India Sanitary Conferences and the reorganisation of the sanitary services.

The General Malarial Committee owes its origin to the Imperial Malarial Conference held at Simla in October 1909. Its duties are the direction and co-ordination of investigations and the selection, at the request of local Governments, of officers qualified for carrying out such investigations. A similar organisation, working in consultation with this Central Committee, is constituted in each province, and a conference consisting of the members of the Central Committee and a delegate or delegates from each local organisation is held annually at such place as may be convenient for the purpose of reviewing the work done and preparing a programme of future work. Up to the present three conferences have been held, namely at Simla in 1909, at Bombay in 1911 and at Madras in 1912, and the fourth conference will be held at Lucknow in January 1914. The value of these conferences has been proved by the interesting nature of the discussions that have taken place, by the opportunities afforded to delegates of studying malaria under varying conditions, by the stimulus given to original work, and by the valuable resolutions that have been passed and brought to the notice of Government. It is not necessary to give all these resolutions in detail, but the following summary of the conclusions arrived at may be of interest :

(1) Careful malarial surveys such as those made by Robertson and Graham in Saharanpur, Kosi and Nagina, and researches in the field such as those carried out by Bentley in Bombay and Christophers in the Andamans, prove the value of preliminary scientific investigation, and point to the probability that anti-mosquito measures may not prove so costly as was at one time feared. Moreover, although further research and expert investigation is still necessary, enough is known of the breeding habits of mosquitos, etc., to make it frequently possible for trained workers to deal with malaria in an efficient manner.

(2) Quinine prophylaxis, applied to a free population, is

difficult to carry out in the thorough way necessary for success, but notwithstanding these difficulties it cannot be too strongly emphasised that arrangements for the treatment by quinine of those sick from malaria is a matter of primary importance from the point of view of saving life, of preventing suffering, and of destroying a potent source of infection. On the other hand experience in the United Provinces and elsewhere has shown that the regular administration of quinine to school-children during the malarial season is a practical measure of proved utility and easy application.

(3) In view of the correlation which certain observers have found to exist between density of jungle in and around villages on the one hand and intensity of malaria on the other it is desirable that this question should receive the careful attention of all those working at malaria in India.

(4) In view of the fact that investigation has shown that the cultivation of rice and other crops, for which an abundance of water is necessary during growth, need not lead to the formation of dangerous breeding grounds for mosquitos, it is desirable in the interests of the Indian agriculturist to ascertain definitely the precise conditions under which such cultivation is or is not likely to be harmful.

(5) Further research is necessary with a view to ascertaining the most effective larvæcides and natural enemies of the mosquito, and which of them are best suited for use in particular localities and under different conditions of environment. It is desirable, moreover, to consider the advisability of constructing ponds in centres where permanent water can be obtained for the breeding on a large scale and the distribution of the more important of the natural enemies of mosquito larvæ.

Other resolutions deal with such subjects as educational propaganda, borrow-pits, water-tidiness, and the provision of a pure and protected water supply. But it must not be imagined that the functions of the General Malarial Committee begin and end in the passing of pious resolutions at conferences. On the contrary it is doing much practical work, and its organisation has been materially strengthened by the appointment of special malarial officers in Madras, Bengal, the United Provinces, the Central Provinces, the Punjab and Burmah. A Central Malarial Bureau, consisting of a museum, a laboratory, and a reference library, under the charge of Major Christophers, has

been started at the Central Research Institute, Kasauli, where a very fine collection of mosquitos and their natural enemies has now been arranged and is available for study. We have also organised classes of instruction in malarial technique. These classes meet twice a year, and the course lasts for two months. During the last two years the system of these classes has been modified so as to make them more practical and to render it possible for any medical officer or subordinate, who is seriously desirous of studying malaria, to gain admission to one of the classes, and it is hoped that ere long this will result in a large number of competent and keenly active workers being spread over the country—a result which cannot fail to bring about a great increase in our knowledge, not only of malaria, but of other closely allied diseases, especially those of the " Leishmania " group. In 1911 only 18 officers were trained at these classes, all from the civil side. During 1912, however, we trained 57 candidates, of whom 27 were in civil and 30 in military employ; whilst in 1913 we admitted 64 candidates, 32 military and 32 civil. In conformity too with the practical aspect of our policy we arranged that the last two classes, instead of meeting at Amritsar, should be held at Delhi, where Captain Hodgson, who was officiating for Major Christophers, was conducting a detailed malarial survey of the Imperial enclave—a survey which, by the way, proved of the greatest value to the authorities when they had to decide upon the site for the new Imperial Delhi. Thus Captain Hodgson's pupils have actually participated in a malarial survey, and are fully equipped for carrying on similar work in their own districts.

There are at the present moment eight officers on special duty in different parts of India, studying the local conditions which underlie and are causing the malaria and devising schemes for its reduction or abolition. The Government of India has set aside a sum of five lakhs for anti-malarial purposes, and, from this, special grants have been made for such investigations, and as schemes have been prepared, further grants have been given either to cover their full cost or to assist in bringing them into effect, the guiding principle being as far as possible to recommend expenditure only upon schemes which preliminary investigations have shown to be likely to accomplish definite results. Thus to Madras Rs. 28,000 has been given for a malarial survey in Ennore, and to Bombay Rs. 50,000 to assist

in carrying out Bentley's recommendations for the prevention of malaria in Bombay City. Two other investigations—one in Sind and the other in the Canara district—are also in progress in the Bombay Presidency, and for these a grant of Rs. 21,380 has been made.

In the United Provinces malarial surveys have been undertaken in the towns of Saharunpur, Nagina, Kosi, Kairana, and Meerut, and recommendations have been made for each place. In Saharunpur, Nagina, and Kosi an active anti-mosquito campaign is now being carried out with the aid of a grant of Rs. 1,80,000 from the Government of India, but the schemes for Meerut and Kairana were still under consideration when I left India in April last.

In the Punjab Rs. 35,000 has been allotted for anti-malarial measures at Palwal, which lies in a specially malarious tract. The list of work in progress is a fairly satisfactory one, but it is the intention of Government to extend their operations to other places as soon as funds and men are available. In Bengal the conditions are very different from those in other parts of India, and Stewart and Proctor have shown that in Lower Bengal there is a close connection between over-vegetation and intensity of malaria—in which respect they are in close agreement with the findings of Watson in Malaya. At the suggestion of the Government of India, the Government of Bengal has taken the matter up, and a grant of Rs. 50,000 has been allotted to them for carrying out an extensive experiment of jungle-clearing in the vicinity of inhabited areas. Should this experiment prove successful we shall have at our disposal one method, at least, of improving the conditions obtaining in small villages, specially those in the deltaic area. Indeed, I am of opinion that if with systematic clearing of jungle we combine the provision of a pure water supply, water-tidiness, the preservation of mosquito destroyers, and the distribution of quinine, it may be possible to achieve wonderful results in rural areas where financial considerations and the physical conditions render elaborate drainage schemes practically impossible. For this reason I have noted with much pleasure the formation at Jessore of a Coronation Anti-malarial Society which intends to work in villages on lines very similar to those indicated above, and I congratulate Rai Jadunath Mazumdar Bahadur on its inception. It is yet another sign of that sanitary awakening to which I have alluded above,

and I trust that it marks the beginning of that co-operation of the public, upon the necessity for which I have insisted so frequently, and without which we can never hope to achieve a victory in our campaign against malaria.

But, although jungle-clearing may prove useful in flat country, it is doubtful whether it will avail in hilly tracts intersected by ravines. Watson has found it useless in Malaya, and Major Perry, as the result of his investigations in the Jeypore Hill Tracts, confirms these conclusions. In a paper which he read before the last Malarial Conference he showed that, whereas on the 3,000 ft. plateau, jungle-clearing produces little obvious effect, on the 2,000 ft. plateau the conditions are different, and he believes that in this situation the proper clearing of jungle gives hope of the practical eradication of malaria.

Much important work has been done in India in connection with the stocking of pools and tanks with mosquito destroyers, and the observations of Sewell and Chaudhri in Calcutta, of Glen Liston in Bombay, and of Wilson in Madras have shown that this need not be an expensive or troublesome task. It is not necessary that we should import the much-vaunted "millions" from Barbadoes; we have in India numerous fish of proved utility as mosquito destroyers, especially species belonging to the four genera Haplochilus, Ambassis, Trichogaster, and Nuria.

The credit for the inception of the *Indian Research Fund Association*, which was established in 1911, is due to the late Lieut.-Col. Leslie, Sanitary Commissioner with the Government of India, whose untimely death has deprived of a valued colleague all those interested in the cause of sanitation in the East. The objects of the Association are the prosecution and assistance of research, the propagation of knowledge, and experimental measures generally, in connection with the causation, mode of spread, and prevention of communicable diseases. The nucleus of the fund was a grant of five lakhs from the Government of India, to which a similar amount has since been added, and the control and management of the Association are vested in a governing body the president of which is the Honourable Member in charge of the Education Department. The Governing Body is assisted by a " Scientific Advisory Board," of whom not less than three are members of the governing body. They examine

all proposals in connection with the scientific objects of the Association and report as to their feasibility. The members of this board are appointed for one year, but are eligible for re-election, and they have power to add to their number. The present members are the Director-General Indian Medical Service, the Sanitary Commissioner with the Government of India, the Director of the Central Research Institute at Kasauli, the officer in charge of the Central Malarial Bureau, and the Assistant Director-General Indian Medical Service (Sanitary), and Sir Ronald Ross has been elected an honorary consulting member of the board.

The scientific objects of the Association are carried out with the aid of "Working Committees," appointed by and acting under the direction of the Scientific Advisory Board—an arrangement which ensures proper correlation of research and prevents overlapping.

Under the auspices of this Fund, exhaustive inquiries into various problems connected with Kala Azar, Yellow Fever, Plague, Relapsing Fever, Cholera, and Dysentery have been conducted by specially selected officers, and several interesting and important discoveries have been made.

Kala Azar.—The researches into this disease have been carried out under the direction of a Working Committee consisting of Surgeon-Gen. Bannerman, Lieut.-Col. Donovan, Major Christophers, and Dr. Bentley, the chief points under consideration being the possible antagonism between Oriental Sore and Kala Azar, and the question of the carrier and reservoir of the parasite of that disease. The actual investigations have been entrusted to Captains Patton and Mackie and Dr. Korke, the division of labour being as follows: Captain Mackie has conducted an epidemiological inquiry into the distribution and prevalence of Kala Azar in Assam, where the conditions for the spread of the disease appear to be peculiarly favourable. Captain Patton and Dr. Korke have worked in Madras, the former devoting himself chiefly to laboratory experiments, whilst Dr. Korke undertook the investigation of the disease in the endemic area at Royapuram. Patton's results are well known. He has undoubtedly proved that under certain definite conditions the parasite of Kala Azar undergoes its full cycle of development in the body of the bug: he has not, however, succeeded in transmitting the disease from one animal to

another. The difficulty, of course, is to obtain a susceptible animal for the transmission experiments, but we hope that this difficulty will soon be surmounted. As the result of his investigations in Royapuram, Dr. Korke has discovered the interesting fact that the disease is not strictly speaking a house-infection, but that it tends to cling to communities having close social relations with one another. Another valuable experiment is that made by Colonel Donovan, in which he succeeded in infecting an Indian dog with the disease, the post-mortem examination showing extensive infection of the bone-marrow, whilst the liver and spleen were apparently healthy. This renders it necessary that we should reconsider our position as regards Indian dogs, and I am of opinion that a further series of observations, with examination of the bone-marrow, will be necessary before we can say with confidence that the Indian dog is immune to " Leishmania Donovani," and these observations are all the more necessary in view of the opinion expressed by Laveran and Nicolle, in their recent paper read before the International Medical Congress, as to the probable identity of the Mediterranean and Indian forms of the disease. It has been decided, therefore, to continue the inquiry for another year, both by laboratory experiments and investigations in the field.

Yellow Fever.—In view of the opening of the Panama Canal, it was considered to be of importance that prior to the actual opening the Government of India should obtain definite first-hand information regarding the conditions in Central America, where Yellow Fever is endemic, and in the principal ports between Central America and India, to admit of adequate measures being devised to prevent the introduction of the disease into India. Accordingly, in October 1911 Major S. P. James, I.M.S., was deputed, at the cost of the Research Fund, to proceed to the endemic area by the route that will be followed by ships coming to India when the Canal is opened. Major James returned to India last November and submitted a most interesting and valuable report, which is now under consideration. After a careful study of the trade routes, he is of opinion that the immediate danger to India on the opening of the Panama Canal is not as great as was anticipated originally. His chief reasons for his view are (1) that the very thorough precautions taken at Honolulu, which is the first port of call for the Transpacific voyage to the East, affords a strong protection

against the infection of Asia and the East Indies, and (2) that, on the usual route to Hong Kong, ships after leaving Honolulu pass northwards into latitudes not as a rule favourable to the life of the mosquito, so that there is little likelihood at present of the introduction of infected mosquitos into our ports. This, however, does not justify the conclusion that no action is necessary at this stage. Major James has made many important recommendations which are now under consideration. Meanwhile, an active "Stegomyia" survey has been made of our chief Indian ports by specially selected officers who had undergone a preliminary training by Mr. Howlett at Pusa, the object of the survey being to prove whether or no the extermination of this mosquito or its reduction to non-dangerous numbers in our seaports is really practicable. So far the preliminary reports are very encouraging. They show that *Stegomyia fasciata* is essentially a domestic mosquito, breeding in small collections of stagnant water within house limits, so that its extermination is largely a question of home sanitation, and not one involving extensive drainage operations. But from the observations made it is clear that the problem is not quite so simple as it appears. We can easily deal with discarded tins, bottles, etc., but if we are to attain success, it is necessary that arrangements should be made for a continuous water supply to the houses in the poorest localities, thus obviating the necessity for water-storage in houses, for it is the receptacles for such storage which constitute the most important breeding grounds of this mosquito. This point is now under consideration. I may also mention that, at the suggestion of the Government of India, the Government of Ceylon has arranged to conduct a similar survey of the principal ports in the island, and that for this purpose the services of Major S. P. James, on his return from Panama, have been lent temporarily to the Colonial Government.

Plague.—Space will not permit of a discussion of the many problems associated with this disease. There is, however, one point on which I wish to lay stress, and that is the large part played in the spread of plague by grain stores and grain markets. Captain White, I.M.S., in a paper read before the last All-India Sanitary Conference, showed clearly that there is a close correlation between the import of grain into each trade block and the amount of plague from which such areas have suffered in the past. Experiments have therefore been made at

the Bacteriological Laboratory, Parel, with a view to solving the problem of the disinfection of grain in bulk. There experiments have proved encouraging under laboratory conditions, but the Scientific Advisory Board consider it necessary to carry out a practical experiment of disinfection of grain on a larger scale, and for this purpose a sum of Rs. 1,000 has been sanctioned from the Research Fund. The experiment is being carried out by Major Glen Liston, and we await his report.

Relapsing Fever.—Most people are under the impression that this disease has practically died out in India, but Government has known for some time that small outbreaks occur frequently in certain districts in the United Provinces. They are not serious, and there are reasons for believing that the disease is endemic in the villages of the Jumna Kadir, where it is usually unrecognised and treated as malaria. In the spring of last year the death-rate was noticed to be rising in the Meerut district, and it was presumed at first to be due to plague. The comparatively low mortality, however, aroused suspicion, and the examination of blood films revealed typical Spirochætæ, whilst subsequent investigation showed that some seventy villages were infected with relapsing fever. At the request of the local Government, the governing body of the Research Fund have deputed Captain Brown from the Central Research Institute, Kasauli, to proceed to the United Provinces to investigate the causes of the recent outbreak. He will also endeavour to confirm the recent observations of Nicolle as to the exact mechanism of transmission by the body-louse, which, as Captain Mackie was the first to demonstrate, is known to be the carrier of the disease.

Cholera.—Major Greig, I.M.S., working at Calcutta and Puri, has during the year carried out a most important series of observations. He has shown that we can no longer regard cholera merely as a water-borne disease. The cholera vibrio will live for a long time in the gall bladder, and it is certain that not only cholera convalescents but also healthy persons who have been in contact with cholera cases can act as " carriers." Major Greig also incriminates flies. His researches will be continued for another year, and we trust that his discoveries will prove of much value to the committee which, under the presidentship of the Sanitary Commissioner with the Government of India, is now inquiring into the possibility

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of improving the sanitary arrangements at the different pilgrim centres.

It is also proposed to depute a second officer to study various problems in connection with the life-history of the cholera vibrio outside the human body.

Dysentery.—As regards this disease, which is the cause of so much sickness and mortality throughout India generally, and specially in Eastern Bengal and the Andamans, much uncertainty and doubt still exist as to the causation of its different varieties, especially the bacillary forms. It has been decided therefore that the whole subject shall be carefully and thoroughly investigated by Captain Cunningham, Assistant Director, Central Research Institute, who has been placed on special deputation for that purpose.

Water Analysis.—It is obvious that in dealing with water-borne diseases we must be in a position to say definitely whether or no a given sample of water is fit for human consumption. This is a point on which there is much difference of opinion. It is recognised that the bacteriological standards fixed for England are not always reliable in India. Moreover, samples of water sent to distant laboratories, especially during the hot months, are liable to undergo decomposition *en route*, and thus the analysis may be of little or no value. It has been decided, therefore, to hold an exhaustive inquiry into the whole subject with a view to settling (*a*) what are the most suitable methods of water analysis, (*b*) is it possible to fix definite bacteriological standards for India, and (*c*) what are the best methods of conveying samples of water to distant laboratories.

The Journal of Indian Medical Research.—Under the above title, a quarterly journal will be published, the first number of which is now in the press. It will be edited by the Director-General Indian Medical Service and the Sanitary Commissioner with the Government of India, and it will contain full accounts and reports of all work done under the auspices of the Indian Research Fund. There will be special sections for malaria, medical entomology, protozoology, etc., and all original communications will be welcomed. Such a journal will, we think, serve a useful purpose—it will take the place of " Paludism," and in it will be included many of the shorter papers by officers of the Indian Medical Service which are not of sufficient length to justify publication as separate " Scientific Memoirs."

I can only deal very briefly with the subject of the *All-India Sanitary Conferences*. The first of these was held in Bombay in November 1911, and the second in Madras in November 1912, whilst the third will meet in Lucknow in January 1914. Their popularity may be judged from the fact that whereas at the first conference twenty-nine delegates attended and the proceedings lasted for only two days, at the second conference seventy-three delegates were present and the proceedings extended over a week, with both morning and afternoon sittings. For further information as to the subjects discussed and the important resolutions passed, I must refer the reader to the published Proceedings. All I wish to say here is that the value of these conferences lies not so much in the conclusions reached as in the opportunity which they afford of informing and interesting the public, and of interchange of views between men working under varying conditions in isolated parts of India. I have already pointed out that sanitary measures possible and effective in the West may not be suited to Indian conditions. Similarly it must be clearly understood that there cannot be one sanitary programme for all India. Sanitation is rightly decentralised, and it is only by the examination of results obtained under differing conditions that we can arrive at definite conclusions as to what is suitable for a particular locality. That is why the conference is held each year in a different place. The last two meetings have been in large presidency cities; the next will be in an up-country town, where I need hardly remark the conditions are very different from those existing in Madras and Bombay.

In conclusion I must say a few words about the *reorganisation of the sanitary services in India.* In 1912 the Government of India decided to create eight additional appointments of Deputy Sanitary Commissioner. As these posts did not fully meet the needs of the provinces, the Secretary of State for India has recently approved of the addition of four appointments to this class.

The twelve appointments will be allotted as follows : three to Bengal, two each to Madras, the United Provinces and Behar and Orissa, and one each to the Punjab, the North-West Frontier Province, and Burmah.

For the present three of the twelve appointments will be held by officers of the Indian Medical Service and the remaining nine are open to officers recruited in India. Six Indians have already

been appointed as Deputy Sanitary Commissioners. The remaining three appointments have not yet been filled up.

In addition thirty-nine first-class and 104 second-class health officers are to be appointed to the municipalities, and in order to assist local Governments in organising the service a recurring grant of 2·66 lakhs of rupees has been sanctioned from Imperial revenues, in addition to an expenditure of Rs. 25,560 per annum in the North-West Frontier Province which will be met by the Imperial Government.

The Government of India are meeting the cost of the new appointments of Deputy Sanitary Commissioners on the scale sanctioned for Indians and are giving a subvention amounting to half of the pay of first and second-class health officers.

This to some sanitary enthusiasts may not seem sufficient provision, but I would point out that one must cut one's coat according to the cloth, and it is not sound policy to tax the clothes off people's backs in order to provide them with the benefits of sanitation. As one of the Indian delegates said at a recent conference, " You must feed us before you educate us," and the same remark applies here. Moreover, when funds are limited it is unwise to spend on personnel money which would be better applied in remedying obvious sanitary defects. An expensive supervisory staff is hopelessly handicapped if there be no money for carrying out the recommendations submitted. I think that what I have written suffices to justify the title of this article, and proves that the Government of India, the medical services, and the public are all alive to the value of preventive measures, and that we fully realise the important part which will be played by sanitation in the medicine of the future.

ATOMIC THEORY AND RADIOACTIVITY

By SIR OLIVER LODGE, F.R.S., D.Sc., LL.D.

IN the April number of SCIENCE PROGRESS is an article on "The Mystery of Radioactivity," signed by the easily recognisable initials H. E. A.; and in spite of the eminent services of the author of that article to Chemistry, I feel that some notice ought to be taken of it because, as it stands, its tendency is obstructive to progress. With "conservatism" I confess to a good deal of sympathy, up to a limit, but the limit is transgressed when facts are ignored and hypotheses wildly manufactured in order to retain some old and superseded exclusive and negative generalisation.

That radium has proved itself an element, to be classed with the other elements in respect of such things as a recognised place in Mendelejeff's series, a definite spectrum, regular chemical compounds, and such like, is surely a fact; and to controvert it needs something more than an etymological discussion about the meaning of the word *element*. The term would be equally applicable or inapplicable if, as has often been surmised, all the known elements turn out to be groupings of some one fundamental substance. What is certain is that the so-called elements form a definite and recognised group of substances of which radium is a member.

Moreover, it must be permissible to speak of an *atom* of radium, when dealing with its physical and atomic properties, in spite of the fact that it is an atom liable to spontaneous explosion or fission. To hesitate about this—to be afraid to use the convenient and brief term "atom" because of historical derivation—would involve a loss of this useful word altogether. It is well known in philology that significance changes, and that the meaning associated with original derivation is liable to be gradually departed from. Besides, even pedantically, we must admit that the idea of "cutting" suggests something artificial, and that the artificial stimulation of atomic break-up has yet to be discovered.

This is a minor matter, it is true, but it leads Prof. Armstrong to liken a radium atom to a molecule of nitrogen chloride, a compound which explosively resolves itself into what are called its " constituent " atoms ; although in what form the nitrogen and the chlorine exist in the compound, is a matter on which I would gladly learn from Prof. Armstrong rather than attempt to instruct him.

But it is misleading to liken the progressive disintegration-process responsible for radioactivity to the ordinary decomposition of chemical compounds. Prof. Armstrong admits that the rupture of a radium atom involves the formation of two neutral substances, the Emanation and Helium ; but he goes on to say that " it cannot be a compound of such substances, and yet they are obtained from it " ; so he supposes that " either or both must be present in it in some active form."

This guess is made merely because he is unwilling to recognise any mode of grouping other than a chemical one—*i.e.* other than a grouping of atoms under chemical affinity. Radium is truly not a chemical compound, but its atoms appear to embody a physical grouping such that definite substances result when it subdivides. This might be speculation, were it not that the emission of observed substances from radium actually occurs. In no chemical decomposition are atoms shot out with one-tenth of the velocity of light. The energy displayed is of a different order from chemical energy.

In the effort which he makes to liken this kind of volcanic disruption to chemical decomposition, on the analogy of nitrogen chloride, Prof. Armstrong is forced into hypotheses for which there is no basis whatever beyond his own speculative instinct. This is what he says :

" It is only necessary to suppose that the molecule of Helium as we know it, like the molecule of nitrogen as we know it, is composed of several ' atoms ' of—let us call it *protohelium*, and that the atoms of protohelium have *intense* affinity for one another—an affinity so intense that it is far beyond anything we have experienced in the case of any other element.

" When argon was first described in 1895 by Rayleigh and Ramsay, I ventured to assert such a view in explanation of its apparently complete inactivity. What is true of argon is true doubtless of all its companions in air—helium, neon, and krypton. . . . Protohelium apparently is the wondrous material at the root of radioactivity."

Now speculative instinct is extremely valuable as a guide among new facts, but it is not powerful enough to be able to withstand them. Prof. Armstrong feels the difficulty, and presently invents a supplementary explanation, devising for the purpose not only the as yet unknown substance "protohelium," but also another hypothetical element which he names "something else"; and by then postulating strong chemical affinity between his two imaginary materials, he manages to get along. Here are his words, beginning with a pertinent question:

"Why, as radium decomposes so slowly, does it decompose at all; why does it not all blow up suddenly, like an ordinary explosive? There is but one explanation—that, like the other *mere chemical compounds* Prof. Soddy speaks of so slightingly, it is always being decomposed reversibly—into protohelium and something else, the which products reunite more frequently than they part company and escape, the protohelium after it has united with itself; the radium does not blow up, because of the intense affinity of protohelium for its companion product of change."

This is surely an extraordinary statement for a scientific man; and we are constrained to ask, why does Prof. Armstrong strain himself into this singular attitude of gratuitous hypothesis, instead of yielding gracefully to the logic of facts? He gives the answer himself; though he is applying the criticism to other workers who have, as he says, "so long overlooked the potentialities of protohelium"; it is, he says,

"human nature to have chief affection for one's own children; to be blind to their faults and disinclined to seek virtues in those of others."

And in a paragraph already quoted he specifies the "child" he himself is fond of:

"When argon was first described in 1895 by Rayleigh and Ramsay, I ventured to assert such a view in explanation of its apparently complete inactivity."

And so he goes on to suggest that

"it were time to discard the fiction that the gases of the argon family are monatomic molecules which has so long retarded progress."

Here we come to the root of the matter; and we here discern the fundamental cause of his quixotic tilting at ascertained

physical facts, the bearing of which he fails to understand. Let me therefore explain.

The monatomic character of certain gases is physically proved by arguments deduced from an experimental determination of the velocity of sound through them. It is done by a curiously simple, and apparently to Prof. Armstrong despicable, experiment of stroking a glass tube containing the gas and a powder. Physicists thus ascertain the appropriate velocity of sound. This velocity, combined with a knowledge of pressure and density, gives the ratio of the two elasticities—the adiabatic to the isothermal ; which ratio is well known to be the same as the ratio of the two specific heats. The value of the elasticity-ratio shows how the heat generated by sudden compression is disposed of, and therefore exhibits the number of effective degrees of freedom of the molecules. For all the translatory motions go to increase the velocity of sound, while none of the rotatory motions have any effect upon it.

(This is one of the few cases where vulgar fractions, *i.e.* commensurable numbers, enter into physics : all such cases are necessarily important.) Assuming a perfect gas : if the ratio of its elasticities is $7/5$, the significance of that number is that each molecule possesses 5 degrees of freedom altogether, 2 of rotatory and 3 of translatory freedom ; so the molecule must be diatomic, having some analogy with a rigid dumb-bell.

If the ratio were $4/3$, there would be 3 degrees of rotatory freedom, and the molecule must be tri- or polyatomic.

But if the ratio is $5/3$, then all the heat goes to increase the translatory molecular motions, no rotation at all being excited by the collisions. For that to be possible the molecules must be monatomic, and must act on each other during collision to all intents and purposes like smooth spheres.

More can be said about complications introduced by incipient cohesion among the molecules—the so-called "imperfection" of a gas ; but this is sufficient. The argument is clear and only assailable either by suspecting the law of partition of energy or by insisting that ordinary molecular collisions must excite atomic vibrations. Some physicists feel a difficulty on this latter head in the case of di- and tri-atomic molecules, though I think it rather a needless difficulty, but I never heard one raised about the monatomic case.

Now for the application. On determining the velocity of

sound, the ratio of the elasticities is found experimentally to be 5/3 for argon, helium, and other inert gases; therefore they are monatomic.

If the argument does not appeal to Prof. Armstrong, physicists are not to blame; but the circumstance that it does not so appeal is evidently largely responsible for the attitude which he has consistently taken up in connection with those unwelcome, or let us rather say indigestible, chemical discoveries which have been made by purely physical processes.

THE ARGUMENT

It may possibly be helpful to indicate here the whole argument, so far as it can be done with great brevity :

Fundamental kinetic-theory-of-gas considerations, as old as Waterston, give for the molecular velocity, u, of a perfect gas, at absolute temperature T, and with absolute specific heats c^l and c,

$$u^2 = 3\frac{p}{\rho} = 3RT = 3(c^l - c)T \quad . \quad . \quad . \quad . \quad (1)$$

Equipartition of energy among the degrees of freedom available in molecular encounters, combined with the fact that 3 of these degrees of freedom are necessarily translational, causes 3/nths of the total heat imparted by any operation to go towards increase of translational velocity; where n is the whole number of effective degrees of freedom possessed by each molecule.   To express this sufficiently well we may write :

$$\frac{3}{n}(mcT) = \frac{1}{2}mu^2 \quad . \quad . \qquad . \quad . \quad . \quad . \quad . \quad (2)$$

From these two equations we immediately deduce :

$$\text{Therefore} \quad \frac{c^l}{c} = 1 + \frac{2}{n} \qquad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

which justifies the statements in the text ; for a rigid body under the circumstances of molecular collisions has 6 effective degrees of freedom or modes of motion, unless it is like a rod, when it has 5, or like a sphere, when it has only 3.

The only additional equation needed is the one required to interpret the acoustic experiment, viz. the Laplacean expression for the velocity of sound,

$$U^2 = \frac{e^l}{e} \cdot \frac{p}{\rho} = \frac{c^l}{c}RT = \frac{1}{3}\frac{c^l}{c}u^2 \quad . \quad . \quad . \quad . \quad (4)$$

NOVEL EXPERIMENTS AND FACTS
CONCERNING CORROSION

By J. NEWTON FRIEND, D.Sc., Ph.D.

Carnegie Gold Medallist

DURING the last half-century the production of iron by the civilised world has increased at a phenomenal rate; so much so that at the present time some seventy million tons of pig iron are being annually placed upon the market. Such being the case it is evident that all problems connected with the decay and preservation of iron assume increasing importance as the years roll by. The object of this article is to draw attention to some facts concerning corrosion that are not generally known, and to describe a few simple experiments capable of adaptation for class demonstrational purposes.

Inasmuch as the usual commercial forms of iron contain a relatively high percentage of impurity, it will be assumed in these experiments that Kahlbaum's pure iron foil is used; otherwise the results are liable to be irregular and uncertain. If the foil is well rubbed with finest emery and not touched with the fingers the reader should have no difficulty in obtaining fairly regular and certain results. At the same time one word of warning is necessary. The corrosion of iron is affected by so many apparently trivial factors that it occasionally happens that two experiments may be conducted under what appear to be identical conditions, and yet fail to give the same results. In many cases this is due to a variation in the metal itself. This is particularly the case with the ordinary forms of commercial iron, which usually lack the necessary homogeneity both in their chemical composition and their physical condition. Again, the same piece of iron should never be used twice for experimental purposes, otherwise abnormal results are very liable to accrue despite the most careful superficial cleaning. This is probably due to the fact that the metal is slightly porous, so that minute particles of foreign bodies, particularly solutions, penetrate to a small depth below the metallic surface and cause a disturbing

effect in later experiments. It must be remembered, too, that fluctuations in the intensity of the light and temperature, the composition of the air and the nature of the containing vessel all play an important part in determining the final results.

If these points are carefully borne in mind the reader will be saved from many disappointments and failures in carrying out the experiments detailed below.

I. DIFFERENT TYPES OF IRON RUST

Let us place a rectangular piece of iron foil in a beaker in such a manner that its four corners rest in contact with the sides and bottom of the glass, as indicated diagrammatically in fig. 1. Now cover with distilled water to such a depth that the level of the liquid A shall not fall by evaporation as low as the top of the metal B, otherwise disturbing effects will ensue.

What do we observe ?   In the course of eight or nine minutes
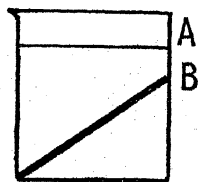
FIG. 1.

a faint yellow skin begins to make its appearance on the surface of the metal and after a short time the iron becomes covered with a thin film of brown rust. In the course of two or three days the rust thickens but remains fairly evenly distributed over the surface of the metal. The colour likewise remains fairly constant and practically no green rust appears.

If we remove the iron and gently rub it, the rust will easily wipe off and a localised thin green stain may or may not be left behind on the metal, according to circumstances. There should be no pitting.

This is the simplest or " normal " form of rusting, the brown layers consisting of a very pure hydrated ferric oxide, which will be referred to in the sequel as brown rust.

The experiment may be varied by laying the foil flat on the bottom of the beaker, and covering with water as before. After a few hours the surface of the iron becomes covered with an even layer of brown rust, but upon lifting up the foil the under-side,

which has been in contact with the glass, is seen to be mainly green. This, however, now rapidly oxidises to brown rust on exposure to air, and therefore consists of iron essentially in the ferrous condition. Although the corroded under-side of the metal may be unequally attacked, there is no pitting observable.

A very similar green appearance may be obtained by immersing iron foil in a saturated solution of a nitrate, such as sodium or potassium nitrate. In this case the iron may be entirely free from the containing vessel, save of course at the four corners of support as in the first experiment; also A B (fig. 1) should not be less than about half an inch. If, after a few days, the iron is removed and gently washed with distilled water the green rust steadily oxidises to a brown colour. There is no pitting. This reaction is interesting as being fairly characteristic of nitrates, for in most other aqueous solutions, such as those of the chlorides
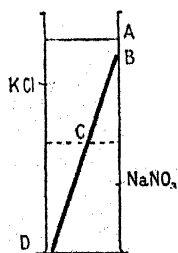
FIG. 2.

and sulphates of the alkali metals, the colour of the rust produced varies from a ruddy brown to a much darker shade with varying amounts of green, according to circumstances. A pretty experiment is as follows: Prepare two saturated solutions at the temperature of the room, one of sodium nitrate and one of potassium chloride. Pour the former into a gas jar and then add the other very carefully, either pouring on to a piece of cork floating on the nitrate, or else allowing it to flow gently down the side of the jar held in an inclined position. The chloride solution being the less dense floats on the nitrate solution. Now insert a polished strip of iron as in fig. 2. In the course of a few hours *a coating of green rust is formed on B C*, whilst C D remains perfectly bright. This is well illustrated by the photograph (fig. 3), where the dark portion represents the corroded metal, and the light the uncorroded. This is particularly interesting because we might have expected *brown rust* from B to C, and
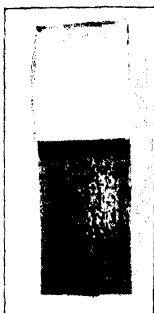
FIG. 4.
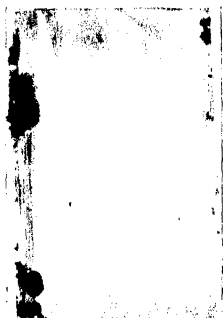
FIG. 5.
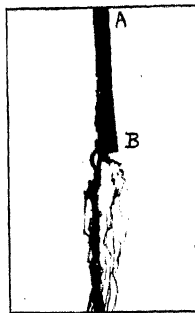
FIG. 6.
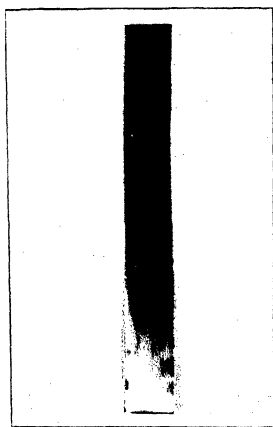
FIG. 3.
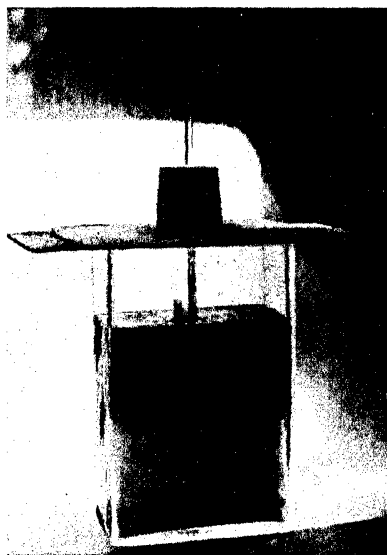
FIG. 11.

green from C to D.   If the surface, A, of the chloride solution is
very near to the top B of the iron, a little of the green rust
oxidises and the metal presents a very pretty appearance—
brown, green, and polished respectively.

2. The Influence of Partial Immersion

Quite a different type of oxidation takes place when iron is
only partially immersed in water.   The portion of the metal not
touched by the liquid may remain quite bright, whilst the
submerged portion becomes covered with brown rust.   But at
the surface of the water, where the air can dissolve most rapidly,
the corrosion is most vigorous, a thick mass of green and brown
rust being quickly formed.   Fig. 4 shows this extremely well,
the metal there figured having been removed after about forty-
eight hours of suspension in distilled water, and gently rubbed.
The upper portion retains its polish, whilst the lower is some-
what tarnished.   At the water line the metal is seen to be
heavily attacked.

3. Pitting

By the term "pitting" we understand the localisation of
severe corrosion at definite points on the surface of the metal,
whereby little hollows or pits are eaten out of the iron.   This is
undoubtedly the most serious form of corrosion, and a simple
example will make this clear.   Suppose we have a tank of water
built of steel plates.   In all probability these plates might safely
lose a few ounces in weight through uniform corrosion without
seriously affecting the strength of the tank.   But a quarter of an
ounce lost through the formation of a single pit might be
sufficient to perforate a plate and make the tank leak.

In the experiments already described there is, or should be,
no pitting with Kahlbaum's foil, although pieces of the usual
commercial metal treated in the above ways will sometimes pit
and sometimes not.

Some very beautiful pitting effects may be obtained, however,
with pure iron foil in solutions of mineral salts rendered weakly
alkaline.   Fig. 5 shows the result of immersing a piece of foil for
several days in a beaker (as in fig. 1) containing a dilute solution
of potassium chloride in about one-twentieth normal potassium
hydroxide solution.   Here the pitting is very pronounced, and
usually follows some scratch or irregularity in the metal, the

effect of which, however, is so slight that in neutral solutions no pitting is observed.

The masses of rust are mostly of the green variety, and rapidly oxidise on removal and exposure to air. The metal really looks much prettier than the photograph indicates owing to the colours ranging from light moss-green through dark green to dark brown, the edges being relieved with the ruddy tinge of ordinary brown rust.

By increasing the quantity of alkali to about twice normal, that is, 112 grams of caustic potash per litre, no corrosion of any kind will take place, whatever the concentration of the chloride.

Particularly pretty results are obtainable by suspending pieces of iron foil in weakly alkaline solutions of potassium chloride by means of glass hooks. The rust accumulates in the form of threads and hangs down from the metal like skeins of brown silk. This is illustrated by fig. 6, where A B is the corroded metal, the lower portion being rust.
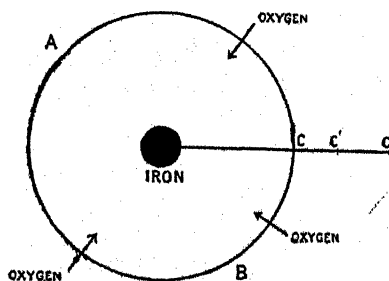
FIG. 7.

Partial immersion of iron in weakly alkaline salt solutions also yields interesting results, the corrosion occurring locally, but being particularly severe at the surface of the liquid where thick masses of green and brown rust accumulate.

4. THE CORROSION ZONE

If a sphere of iron is suspended in a tank of still water it tends to combine with the dissolved oxygen in its immediate vicinity. Fresh supplies of oxygen gradually diffuse towards the iron from surrounding layers of water until equilibrium sets in. When this has been attained a more or less spherical shell might be sketched out in the water as represented in section by the circle A B C in fig. 7 through which oxygen is constantly

diffusing, and outside of which, as at C, C,′ C,″ the concentration of dissolved oxygen is constant.   Inside this sphere the amount of oxygen will gradually fall towards the surface of the iron, at which place it will be lowest.   The same argument applies whatever shape the iron may possess, but the configuration of the shell will, of course, vary accordingly.   Such a shell is known as the " Corrosion Zone."

Now what will happen if we bring a second ball of iron into the same tank of water?   If the distance between the two spheres is greater than twice the radius of the corrosion zone, the metals will not affect each other, and they will each corrode at their maximum rate.   But if, as in fig. 8, the corrosion zones intersect, the amount of oxygen that can diffuse towards each metal ball is reduced, and corrosion is proportionately retarded. If three such balls are brought together in line, clearly the two
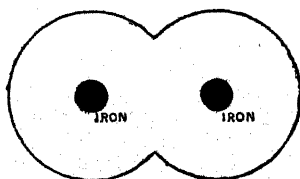
FIG. 8.

outer ones stand the best chance of corroding, for oxygen can diffuse towards the middle one in two directions only, namely from above and below.

This illustrates the importance of using tanks of sufficiently large capacity, and of having the metals a sufficient distance apart when an attempt is made to determine the relative rates of corrosion of a series of samples.

The same argument applies to the employment of series of small containing vessels in cases where only one piece of metal is suspended in each.   Air can only penetrate to the sides and bottom of the vessel from the surface; hence, if the vessel is not larger than the corrosion zone (as in fig. 9), the air at the surface will pass into the corrosion zone and be absorbed by the metal, and there is none left to replenish that at A and B, which is likewise diffusing into the corrosion zone.   In a short time, therefore, we shall have equilibrium after the manner of fig. 10, and the rate of oxidation of the metal now becomes a function of the surface area of the liquid.   It is difficult to arrange a

lecture experiment to show this because water containing dissolved oxygen has the same colour and appearance as absolutely air-free water. But the principle may be made clear by a striking experiment with copper.

Some cuprous chloride is dissolved in strong hydrochloric acid and allowed to turn black by absorption of oxygen from
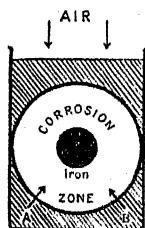
Fig. 9.
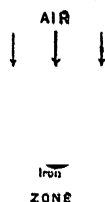
Fig. 10.

the air. The solution is transferred to a narrow rectangular glass tank and a piece of copper suspended in it by a glass rod. The top of the tank may be loosely covered with glass plates. In the course of a few hours or days, depending upon the strength of the acid, the liquid becomes clear around the copper, indicating that the oxygen has been removed. This clear portion corresponds exactly to the corrosion zone in the case of iron, and the effect is decidedly pretty. Some idea of it may be obtained from the photograph (fig. 11), which shows a condition of equilibrium closely corresponding to that indicated in fig. 10.

The ideal condition for testing the rate of corrosion of a
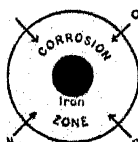
piece of metal is shown in fig. 12, and the probability is that if l is the length of the iron plate or the diameter of the iron sphere employed, the distance of the metal from the side of the containing vessel ought not to be less than about $2l$.

THE DISTURBED MOTION OF AN AEROPLANE

By W. BEVERLEY, M.Sc.

IN the following pages I have attempted a mathematical account of the forces at work in restoring equilibrium to an aeroplane possessing dynamical stability and disturbed from steady motion by periodic gusts of wind. Damping effects have also been found.

We take the centre of mass as origin and three mutually perpendicular directions through it as axes fixed relative to the aeroplane.

Let W = mass of machine in lbs. (also weight in lbs.-wt.).

A, B, C, D, E, F = moments and products of inertia.

u, v, w; and p, q, r = components of translational and angular velocities respectively.

We have $F = \dfrac{ma}{g}$ lbs.-wt. as a standard equation, where m = mass in lbs. and a = acceleration in ft./sec.^{2} λ, μ, ν are the components of angular momentum.

$$\left.\begin{array}{l} \lambda = Ap - Fq - Er = Ap - Fq, \\ \mu = Bq - Dr - Fp = Bq - Fp, \\ \nu = Cr - Ep - Dq = Cr, \end{array}\right\} \text{if } D = O = E.$$

In steady horizontal flight the axis of x is that of flight, the axis of y being vertically downwards, and the axis of z to the left for a right-handed system. For all cases of flight we take the direction of flight as the " x " axis and the others fixed relatively to it as above. In most aeroplanes z = o is a plane of symmetry and $D = O = E$.[1]

When the aeroplane is tilted downwards through an angle

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[1] *N.B.*—We assume that the aeroplane has two propellers rotating in opposite directions, so that gyrostatic effects annul each other.

θ and canted through $\dot{\phi}$ (see *Stability in Aviation*, pp. 20-27) we have by the theory of moving axes:

$$\frac{W}{g}\left(\frac{du}{dt} + qw - rv\right) = \text{accelerating force along } Gx = W\sin\theta + H - X$$

$$\frac{W}{g}\left(\frac{dv}{dt} + ru - pw\right) = \quad \text{„} \quad\quad \text{„} \quad\quad \text{„} \quad Gy = W\cos\theta\cos\phi - Y$$

$$\frac{W}{g}\left(\frac{dw}{dt} + pv - qu\right) = \quad \text{„} \quad\quad \text{„} \quad\quad \text{„} \quad Gz = - W\cos\theta\sin\phi - Z$$

$$\frac{A}{g}\frac{dp}{dt} - \frac{F}{g}\frac{dq}{dt} + \overline{C - B}\,\frac{rq}{g} + F\frac{pr}{g} = \left\{ \begin{array}{c} \text{accelerating moment of} \\ \text{forces about} \end{array} \right\} Gx = - L$$

$$\frac{B}{g}\frac{dq}{dt} - F\frac{dp}{gdt} + \overline{A - C}\,\frac{pr}{g} - F\frac{qr}{g} = - M$$

$$C\frac{dr}{gdt} + \overline{B - A}\,\frac{pq}{g} - F\frac{\overline{p^2 - q^2}}{g} = - Hh - N$$

where X, Y, Z, L, M, N are the components of air resistance, H the propeller thrust acting in general parallel to the axis Gx at a perpendicular distance h below. For simplicity F may be taken to be zero, in which case the axes are the principal axes. To X, etc., may be added terms X_1, etc., due to gusts of wind. We start with the assumption that X_1, etc., are zero—*i.e.* that the air is still.

Assuming that the plane is descending uniformly at an angle θ_0 with the horizon so that $\phi = 0$, *i.e.* it is upright, $u = v = p = q = r = w = 0$ initially. On disturbance they represent small increments. U is the steady velocity forward.

Let X_0, Y_0, etc., represent the initial resistances.

$$\therefore \ 0 = W\sin\theta_0 + H_0 - X_0 = W\cos\theta_0 - Y_0 = Z_0$$
$$= - L_0 = - M_0 = - H_0 h - N_0$$

these being the equations of equilibrium in steady motion. Where H is inclined at an angle η with the direction of flight, the first two equations reduce to

$$0 = W\sin\theta_0 + H_0\cos\eta - X_0$$
$$0 = W\cos\theta_0 + H_0\sin\eta - Y_0.$$

Now let the aeroplane be disturbed and the above increments enter. The resistances are functions of the velocities, and we have—neglecting squares of small quantities—

$$X = X_0 + X_u u + X_v v + X_r r + X_w w + X_p p + X_q q$$

with similar expressions for Y, Z, L, M, and N.

It may be shown that X, Y, N are independent of p, q, w; and Z, L, M of u, v, and r.

In a small change the angle of the tilt becomes $\theta = \theta_0 + \varepsilon$. ($\varepsilon$ small) and $\cos\theta = \cos\theta_0 - \varepsilon \sin\theta_0$, $\sin\theta = \sin\theta_0 + \varepsilon \cos\theta_0$. ϕ is small and is equal to $\sin\phi$.

Then $\dfrac{d(U + u)}{dt} = \dfrac{du}{dt}$. Substituting from the equations of equilibrium we have:

$$\frac{W}{g}\frac{du}{dt} = W\sin\theta + H - X \text{ (since } X_1 = 0) \qquad 1.$$
$$= W\cos\theta_0\, \varepsilon + \delta H - uX_u - vX_v - rX_r$$

$$\frac{W}{g}\left\{\frac{dv}{dt} + r\,U\right\} = -W\sin\theta_0\, \varepsilon - uY_u - vY_v - rY_r \qquad 2.$$

$$\frac{W}{g}\left\{\frac{dw}{dt} - qU\right\} = -W\phi\cos\theta_0 - wZ_w - pZ_p - qZ_q\,(Z_0 = 0) \qquad 3.$$

$$A\frac{dp}{g\,dt} - F\frac{dq}{g\,dt} = -wL_w - pL_p - qL_q\,(L_0 = 0) \qquad 4.$$

$$B\frac{dq}{g\,dt} - F\frac{dp}{g\,dt} = -wM_w - pM_p - qM_q\,(M_0 = 0) \qquad 5.$$

$$\frac{C}{g}\frac{dr}{dt} = -\delta Hh - uN_u - vN_v - rN_r \qquad 6.$$

Equations 1, 2, and 6 form a symmetrical group involving $(XYN)_{uvr}$, and 3, 4, 5 form an asymmetrical group of oscillations —representing translations and rotations to the left or right of the plane of symmetry, $z = 0$—involving $(ZLM)_{pqw}$.

Symmetrical Oscillations

In disturbed steady horizontal flight we have

$$\phi = 0,\ \theta = \theta_0 + \varepsilon,\ \frac{d\varepsilon}{dt} = \frac{d\theta}{dt} = r\ (= \lambda\varepsilon)$$

assuming u, v, r ε proportional to $e^{\lambda t}$ (λ to be found). For simplicity we may take $\delta H = 0$—i.e. the thrust H is independent of changes in the velocity. Substitute in the equations of motion above.

$$\therefore \left(W\frac{\lambda}{g} + X_u\right)u + X_v v + \left(X_r - \frac{W}{\lambda}\cos\theta_0\right)r = \delta H = 0$$

$$Y_u u + \left(W\frac{\lambda}{g} + Y_v\right)v + \left(\frac{W}{\lambda}\sin\theta_0 + \frac{WU}{g} + Y_r\right)r = 0$$

$$N_u u + N_v v + \left(C\frac{\lambda}{g} + N_r\right)r = 0$$

λ is therefore given by

$$\begin{vmatrix} W\dfrac{\lambda}{g} + X_u & X_v & X_r - \dfrac{W}{\lambda}\cos\theta_o \\[2ex] Y_u & \dfrac{W\lambda}{g} + Y_v & Y_r + \dfrac{WU}{g} + \dfrac{W}{\lambda}\sin\theta_o \\[2ex] N_u & N_v & \dfrac{C\lambda}{g} + N_r \end{vmatrix} = 0.$$

Multiplying the last column by λ we have on expanding an equation of the fourth degree in λ.

Asymmetrical Oscillations [1]

$$\dot\phi\cos\theta_o = p\cos\theta - q\sin\theta_o$$

but

$$\dot\phi = \frac{d\phi}{dt} = \lambda\phi \text{ (assumed)}$$

$$\therefore \ \lambda\phi\cos\theta_o = p\cos\theta_o - q\sin\theta_o.$$

The equations 3, 4, and 5 on p.　　become

$$\left(\frac{W\lambda}{g} + Z_w\right)w + \left(\frac{W}{\lambda}\cos\theta_o + Z_p\right)p + \left(Z_q - \frac{WU}{g} - \frac{W}{\lambda}\sin\theta_o\right)q = 0$$

$$L_w w + \left(A\frac{\lambda}{g} + L_p\right)p + \left(-F\frac{\lambda}{g} + L_q\right)q = 0$$

$$M_w w + \left(M_p - F\frac{\lambda}{g}\right)p + \left(B\frac{\lambda}{g} + M_q\right)q = 0.$$

Again we have an equation of the fourth degree to find λ by expanding the following:

$$\begin{vmatrix} \dfrac{W\lambda}{g} + Z_w, & Z_p + \dfrac{W}{\lambda}\cos\theta_o, & Z_q - \dfrac{WU}{g} - \dfrac{W}{\lambda}\sin\theta_o \\[2ex] L_w, & A\dfrac{\lambda}{g} + L_p, & -F\dfrac{\lambda}{g} + L_q \\[2ex] M_w, & -F\dfrac{\lambda}{g} + M_p, & B\dfrac{\lambda}{g} + M_q \end{vmatrix} = 0.$$

In both types of oscillations we have u, v, w, etc., of the form $a_1 e^{\lambda_1 t} + a_2 e^{\lambda_2 t} + a_3 e^{\lambda_3 t} + a_4 e^{\lambda_4 t}$ or $\Sigma_s(a_s e^{\lambda_s t})$.

For stability then λ_s must be such that the real part is negative or zero. If the real part is positive there is instability.

For stability $\lambda_s = -a_s \pm i\beta_s$ $\{a_s =$ or $> 0\}$. If β_s is zero there is subsidence, and if β_s is not zero there is oscillation and subsidence, two of the terms $\Sigma_s(a_s e^{\lambda_s t})$ reducing to $e^{-a_s t}(a\cos\beta_s t + b\sin\beta_s t)$ where a and b are arbitrary constants.

[1] See *Stability in Aviation*, p. 31.

u : v : w can be found from the above determinant; the ratios being equal to certain first minors and similarly for p, q, w. The equations to find λ (on reducing the respective determinants) are:

Symmetrical oscillations : $A_0\lambda^4 + B_0\lambda^3 + C_0\lambda^2 + D_0\lambda + E_0 = 0$

Asymmetrical oscillations: $A'_0\lambda^4 + B'_0\lambda^3 + C'_0\lambda^2 + D'_0\lambda + E'_0 = 0.$

For stability[1] A_0, B_0, C_0, D_0, and E_0 must all be positive and $B_0C_0D_0 - E_0B_0^2 - A_0D_0^2 > 0$, and similarly for the values A'_0 etc.

FORCED OSCILLATIONS

Those forces, $- X_1, - Y_1, - Z_1, -L_1, - M_1, - N_1$ representing gusts of wind are periodic when they set up indefinitely increasing oscillations in the aeroplane. As such they may be represented as follows, assuming also that they are continuous :

[2] Any force $= f(t) = Pe^{-kt}\cos(\lambda t + a) + P'e^{-k't}\cos(\lambda' t + a)$

$$+ \ldots . + \ldots .$$

$$= \sum_s P_s e^{-k_s t}\cos(\lambda_s t + a_s),$$

where each term in the summation is called a disturbing force— permanent or evanescent according as K is or is not zero respectively.

Symmetrical Oscillations

The equations of motion on substituting values for X_1, etc., are now :

$$\left(W\frac{\delta}{g} + X_u\right)u + X_v v + \left(X_r - \frac{W}{\delta}\cos\theta_0\right)r = - \sum_{s_1} X_{s_1} e^{-n_{s_1} t}\cos\left(p_{s_1} t + \varepsilon_{s_1}\right)$$

$$Y_u u + \left(Y_v + \frac{W\delta}{g}\right)v + \left(Y_r + \frac{WU}{g} + \frac{W}{\delta}\sin\theta_0\right)r = - \sum_{s_2} Y_{s_2} e^{-n'_{s_2} t}\cos(p'_{s_2} t + \varepsilon'_{s_2})$$

$$N_u u + N_v v + \left(C\frac{\delta}{g} + N_r\right)r = - \sum_{s_3} N_{s_3} e^{-n''_{s_3} t}\cos(p''_{s_3} t + \varepsilon''_{s_3}).$$

Since δ may be computed and operated on as any ordinary algebraic symbol may, we have :

$$u = - \left[\frac{U_1(\delta)}{\Delta(\delta)}\sum_{s_1} X_{s_1} e^{-n_{s_1} t}\cos(p_{s_1} t + \varepsilon_{s_1}) + \frac{U_2(\delta)}{\Delta(\delta)}\sum_{s_2} Y_{s_2} e^{-n'_{s_2} t}\cos(p'_{s_2} t + \varepsilon'_{s_2}) + \right.$$

$$\left. \frac{U_3(\delta)}{\Delta(\delta)}\sum_{s_3} N_{s_3} e^{-n''_{s_3} t}\cos(p''_{s_3} t + \varepsilon''_{s_3})\right]$$

[1] Routh's *Stability in Motion.*

[2] See Routh's *Advanced Rigid Dynamics*, vol. ii., chapter on "Forced Oscillations."

$$v = -\left[\frac{V_1(\delta)}{\Delta\delta}\sum_{s_1}X_{s_1}e^{-n_{s_1}t}\cos(p_{s_1}t + \mathcal{E}_{s_1}) + \frac{V_2(\delta)}{\Delta(\delta)}\sum_{s_2}Y_{s_2}e^{-n'_{s_2}t}\cos(p'_{s_2}t + \mathcal{E}'_{s_2}) + \right.$$

$$\left. \frac{V_3(\delta)}{\Delta(\delta)}\sum_{s_3}N_{s_3}e^{-n''_{s_3}t}\cos(p''_{s_3}t + \mathcal{E}''_{s_3})\right]$$

$$r = -\left[\frac{R_1(\delta)}{\Delta(\delta)}\sum_{s_1}X_{s_1}e^{-n_{s_1}t}\cos(p_{s_1}t + \mathcal{E}_{s_1}) + \frac{R_2(\delta)}{\Delta(\delta)}\sum_{s_2}Y_{s_2}e^{-n'_{s_2}t}\cos(p'_{s_2}t + \mathcal{E}'_{s_2}) + \right.$$

$$\left. \frac{R_3(\delta)}{\Delta(\delta)}\sum_{s_3}N_{s_3}e^{-n''_{s_3}t}\cos(p''_{s_3}t + \mathcal{E}''_{s_3})\right]$$

where

$$\Delta(\delta) = \begin{vmatrix} \dfrac{W\delta}{g} + X_u & X_v & X_r - \dfrac{W}{\delta}\cos\theta_o \\[2ex] Y_u & Y_v + \dfrac{W\delta}{g} & Y_r + \dfrac{W\sin\theta_o}{\delta} + \dfrac{WU}{g} \\[2ex] N_u & N_v & N_r + \dfrac{C\delta}{g} \end{vmatrix}$$

and $U_1(\delta)$, $U_2(\delta)$, $U_3(\delta)$; $V_1(\delta)$, $V_2(\delta)$, $V_3(\delta)$; and $R_1(\delta)$, $R_2(\delta)$ and $R_3(\delta)$ are the cofactors of the constituents of the first column, second column, and third column respectively.

Let

$$F_1(\delta) = \frac{U_1(\delta)}{\Delta(\delta)}$$

and consider the solution of

$$F(\delta)X_{s_1}e^{-n_{s_1}t}\cos(p_{s_1}t + \mathcal{E}_{s_1})$$

as a type. We have

$$F(\delta).Pe^{mt} = F(m).Pe^{mt}.$$

Let

$$m_{s_1} = -n_{s_1} + ip_{s_1} \text{ and } P_{s_1} = Q_{s_1} + iR_{s_1};$$

$$\text{and } X_{s_1}e^{-n_{s_1}t}\cos(p_{s_1}t + \mathcal{E}_{s_1}) = \text{ real part of } P_{s_1}e^{m_{s_1}t}$$

$$= e^{-n_{s_1}t}(Q_{s_1}\cos p_{s_1}t - R_{s_1}\sin p_{s_1}t);$$

whence we have

$$Q_{s_1} = X_{s_1}\cos\mathcal{E}_{s_1}, \ R_{s_1} = X_{s_1}\sin\mathcal{E}_{s_1}$$

$$P_{s_1} = X_{s_1}(\cos\mathcal{E}_{s_1} + i\sin\mathcal{E}_{s_1}) = X_{s_1}e^{i\mathcal{E}_{s_1}}.$$

Similarly under the same conditions of solution

$$P'_s = Y_{s_2}e^{i\mathcal{E}'_{s_2}} \text{ and } P''_{s_3} = N_{s_3}e^{i\mathcal{E}''_{s_3}}$$

$$\therefore u = -\text{ real part of }\left[\sum_{s_1}F_1(\delta)P_{s_1}e^{m_{s_1}t} + \sum_{s_2}F_2(\delta)P'_{s_2}e^{m'_{s_2}t} + \sum_{s_3}F_3(\delta)P''_{s_3}e^{n''_{s_3}t}\right]$$

$$+ \text{ free vibrations}$$

$$= -\text{ the real part of }\left[\sum_{s_1}\frac{U_1(m_{s_1})}{\Delta(m_{s_1})}P_{s_1}e^{m_{s_1}t} + \sum_{s_2}\frac{U_2(m'_{s_2})}{\Delta(m'_{s_2})}P'_{s_2}e^{m'_{s_2}t}\right.$$

$$\left. + \sum_{s_3}\frac{U_3(m''_{s_3})}{\Delta(m''_{s_3})}P''_{s_3}e^{m''_{s_3}t}\right]$$

$+$ the real part of $\sum_s\{(a_s e^{\lambda_s t}) \cdot U_1(\lambda_s)\}$

$$v = - \text{ the real part of} \left[\sum_{s_1} \frac{V_1(m_{s_1})}{\Delta(m_{s_1})} P_{s_1} e^{m_{s_1} t} + \sum_{s_2} \frac{V_2(m'_{s_2})}{\Delta(m'_{s_2})} P'_{s_2} e^{m'_{s_2} t}\right.$$

$$\left. \frac{V_3(m''_{s_3})}{\Delta(m''_{s_3})} P''_{s_3} e^{m''_{s_3} t}\right]$$

$+$ the real part of $\sum_s\{(a_s e^{\lambda_s t}) V_1(\lambda_s)\}$

$$= - \text{ the real part of} \left[\sum_{s_1} \frac{R_1(m_{s_1})}{\Delta(m_{s_1})} P_{s_1} e^{m_{s_1} t} + \sum_{s_2} \frac{R_2(m'_{s_2})}{\Delta(m'_{s_2})} P'_i e^{m'_{s_2} t}\right.$$

$$\left. + \sum_{s_3} \frac{R_3(m''_{s_3})}{\Delta(m''_{s_3})} P''_{s_3} e^{m''_{s_3} t}\right]$$

$+$ the real part of $\sum_s\{(ase^{\lambda_s t}) \cdot R_1(\lambda_s)\}$

where the "λ's" under the summation refer to the free vibrations of which values there cannot be more than four—see the free vibrational equation—with the respective four arbitrary constant values a_s.

Where λ_s is complex we have the free vibrations given by $\lambda_s = - a_s \pm i\beta_s$.

These are

$$(a_s \cos\beta_s t + b_s \sin\beta_s t).$$

The ratios $u : v : r$ will possess certain definable determinantal values easily found. The ratios $p : q : r$ possess the same qualities as $u : v : r$.

[1] Where λ_s is real we see that the free vibrations are proportional to

$$U_1(\lambda_s) : V_1(\lambda_s) : R_1(\delta) \text{ or } U_2(\lambda_s) : V_2(\lambda_s) : R_2(\lambda_s) \text{ or } U_3(\lambda_s) : V_3(\lambda_s) : R_3(\lambda_s).$$

If m_s be a root of $\Delta(m) = o$, the denominators of the forced vibrations become indefinitely small. This gives, however, a value of m_s equal to that of a free vibration. We infer, therefore, that, if any one disturbing force has a period and a real exponential nearly equal to those of any one free vibration, a very large forced oscillation will be produced in the co-ordinates possessing that free vibration.

Usually the disturbing gusts of wind are of the permanent type $P\cos(pt + \varepsilon)$. Since resistances—surface friction and head resistances—to motion enter the roots of $\Delta(\lambda) = o$ giving the free vibrations will all be complex. A real exponential is introduced into the values of λ, none of which, therefore, can equal

[1] See Routh's *Advanced Rigid Dynamics*, vol. ii.

the value of the "m's" ($= ip$) of the gusts. Stability is here retained. Again, in this case, the forced oscillations on the co-ordinate acted upon will be permanent, and will supersede the free vibrations, which in the case of stability contain a real negative exponential and are therefore evanescent, vanishing ultimately. The free vibrations, of course, decrease among themselves at varying rates depending upon the indices $- a_s$ of the exponential. a_s, a positive quantity, is the co-efficient of decay or subsidence.

The Limits of the Forced Vibrations

If $m_s = \lambda_s$ so that $U_1(m_s)\{$or $U_1(\lambda_s) = 0\}$ then λ_s represents a free vibration $a_s U_1(\lambda_s)e^{\lambda_s t}$ which therefore vanishes. The forced vibration containing the fraction $\dfrac{U_1(m_s)}{\Delta(m_s)}$ is finite, however, if there are an equal number of roots (m_s) in $U_1(m) = 0$ and $\Delta(m) = 0$. Therefore if any free vibration is absent from a co-ordinate—u, say—though present in the other co-ordinates, then a disturbing force of the same period and real exponential will produce a finite forced vibration only. We may then conclude that a disturbing force can produce a large vibration in any co-ordinate only if there be present in that co-ordinate a free vibration of nearly the same period and real exponential.

Again, if the period of a forced vibration is very small "p" in the complex value "m" is very great. There are higher powers of m and therefore of p in $\Delta(m)$ than in $U_1(m)$, etc. $\dfrac{U_1(m)}{\Delta(m)}$, etc., become insignificant. The forced oscillations are now of no serious account.

The forced oscillation on a co-ordinate vanishes when the disturbing force on that co-ordinate—u, say—is of the type

$$U_1(\delta)\{\textstyle\sum_s X_s e^{-n_s t} \cos(p_s t + \mathcal{E}_s)\} = 0.$$

[1] $U_1(\delta)V = O$ is, however, the determinantal equation which gives a free vibration constraining "u" to zero. Therefore when the type of the disturbing force which acts *directly* on any co-ordinate is the same as that of any mode of free vibration which constrains that co-ordinate to zero the forced vibration will vanish.

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[1] See Routh's *Advanced Rigid Dynamics*, vol. ii., chapter on "Forced Oscillations."

Complete Solutions

In the general case we consider $\Delta(m)$ {or $\Delta(\delta)$} has a roots equal to m_0 {$= -n_0 + ip_0$} and $U_1(\delta)$—taking a type—has β roots equal to m_0. a and β cannot be greater than 4.

[N.B.—Do not confuse a and β with a_s and β_s in $\lambda_s = -a_s \pm i\beta_s$.]

Let $m = m_0 + h$, where h is ultimately zero. Expanding we have :

$$\frac{U_1(\delta)}{\Delta(\delta)} e^{mt} = \frac{U_1(m_0)e^{m_0 t} + \frac{d}{dm_0}\{U_1(m_0)e^{m_0 t}\}\cdot h + \dots \frac{d^\alpha}{dm_0{}^a}\{U_1(m_0)\cdot e^{m_0 t}\}\frac{h_\alpha}{\underline{\alpha}}}{\Delta(m_0) + \Delta^1(m_0) + \dots \dots \Delta^\alpha(m_0)\frac{h^\alpha}{\underline{\alpha}}}$$

$$\text{where } \Delta^\alpha(m_0) = \frac{\delta^\alpha \Delta(m_0)}{\delta m_0{}^\alpha}.$$

Since there are a roots equal to m_0 in $\Delta(m_0) = 0$, then $\Delta(m_0) = 0 = \Delta'(m_0) = \Delta''(m_0) = \dots \Delta^{\alpha-1}(m_0)$. Similarly,

$$U_1{}^{\beta-1}(m_0) = 0 = \frac{\delta^{\beta-1} U_1(m_0)}{\delta m_0{}^{\beta-1}}$$

and $U_1{}^{\beta-2}(m_0) = U_1{}^{\beta-3}(m_0) = \dots U_1'(m_0) = U_1(m_0) = 0$.

$$\therefore \frac{U_1'(\delta)}{\Delta(\delta)} e^{m_0 t} = (a_0' + a_1't + \dots a_{\alpha-\beta-1}'t^{\alpha-\beta-1})e^{m_0 t} + \frac{\delta^\alpha}{\delta m_0{}^\alpha}\frac{U_1\{(m_0)e^{m_0 t}\}}{\Delta^\alpha(m_0)}$$

on putting h equal to zero.

We also see that the coefficient of $e^{m_0 t}$ is infinite, containing powers of $\left(\frac{1}{h}\right)$. It may, however, be absorbed into the free vibrations—$\Delta(\lambda) = 0$, where $\lambda = m_0$—which are

$$(a_0 + a_1 t + \dots a_{\alpha-\beta-1}t^{\alpha-\beta-1})e^{m_0 t},$$

the coefficients being arbitrary constants.

The forced oscillation on the co-ordinate u is obtained by expanding the last term, the coefficients being similar to those of a binomial expansion. It is

$$- \text{real part of} \frac{P_0 e^{m_0 t}}{\Delta^\alpha(m_0)}\left\{U_1{}^\alpha(m_0) + aU_1{}^{\alpha-1}(m_0)t + \dots \frac{a(a-1)\dots(\beta+1)}{\underline{\alpha-\beta}}U_1{}^\beta(m_0)t^{\alpha-\beta}\right\}$$

The free vibrations are given by single roots of $\Delta(\lambda) = 0$ and s' equal roots λ_0 and roots m_0 as above. The terms containing λ_0 are similar to those containing m_0 and may be included in them. β may or may not be equal to 0 and $a = s'$. The sign Σ denotes terms obtained from values similar to m_0 and λ_0, or λ_s, or m_s. The roots for the forced vibrations are m_s such that $\Delta(m_s) \neq 0$ and m_0 as above.

$$u = \text{real part of } [\underset{s}{\Sigma}\{a_s e^{\lambda_s t} . U_{_1}(\lambda_s)\} + \Sigma(a_0'' + a_1'' t + \dots . a_{\alpha-\beta-1}'' t^{\alpha-\beta-1}) e^{m_0 t} . U_{_1}(m_0)]$$

$$-\text{real part of } \Sigma \frac{P_0 e^{m_0 t}}{\Delta^\alpha(m_0)}\left[U_{_1}{}^\alpha(m_0) + aU_{_1}{}^{\alpha-1}(m_0) . t + \frac{\alpha(\alpha-1)}{\underline{/2}} U_{_1}{}^{\alpha-2}(m_0) . t^2 + \dots . + \right.$$

$$\left. \dots . + . . \frac{\alpha(\alpha-1`\dots(\beta+1)}{\underline{/\alpha-\beta}} U_{_1}{}^\beta(m_0) . t^{\alpha-\beta}\right]$$

$$-\text{real part of}\left[\underset{s_1}{\Sigma}\frac{U_{_1}(m_{s_1})}{\Delta(m_{s_1})}P_{s_1}e^{m_{s_1}t} + \underset{s_2}{\Sigma}\frac{U_{_2}(m'_{s_2})}{\Delta(m'_{s_2})}P'_{s_2}e^{m'_{s_2}t} + \underset{s_3}{\Sigma}\frac{U_{_3}(m''_{s_3})}{\Delta(m''_{s_3})}P''_{s_3}e^{m''_{s_3}t.}\right].$$

a_s, a_0'', a''_1, etc., are arbitrary, giving the free vibrations. Of course there may be free vibrations of the type given by $\lambda = m_0$ when there are no forced vibrations of that type ($m = m_0$); but if there are forced vibrations of the type ($m = m_0$) there are also free vibrations, into which they may be absorbed, of the same type.

There can be two double conjugate roots only of λ since there are only four values of λ. Similarly, there are not more than two such roots of the type m_0. The signs $'$, $''$, $'''$, etc., are merely symbols when used with "a's" and "β's" and not symbols of operation. $V_1(m_0)$, $R_1(m_0)$ are taken as possessing β' and β'' roots respectively. Hence

$$v = \text{the real part of } [\underset{s}{\Sigma}\{a_s e^{\lambda_s t} . V_{_1}(\lambda_s)\} + \Sigma(a''_0 + a''_1 t$$

$$+ \dots a''_{\alpha-\beta'-1}t^{\alpha-\beta'-1})e^{m_0 t} . V_{_1}(m_0) .]$$

$$-\text{the real part of } \Sigma \frac{P_0 e^{m_0 t}}{\Delta^\alpha(m_0)}\left[V_{_1}{}^\alpha(m_0) + aV_{_1}{}^{\alpha-1}(m_0)t\right.$$

$$\left. + \dots . \frac{\alpha(\alpha-1)\dots(\beta'+1)}{\underline{/\alpha-\beta'}}V_{_1}{}^{\beta'}(m_0)t^{\alpha-\beta'}\right]$$

$$-\text{the real part of }\left[\underset{s_1}{\Sigma}\frac{V_{_1}(m_{s_1})}{\Delta(m_{s_1})}P_{s_1}e^{m_{s_1}t} + \underset{s_2}{\Sigma}\frac{V_{_2}(m'_{s_2})}{\Delta(m'_{s_2})}P'_{s_2}e^{m'_{s_2}t}\right.$$

$$\left. + \underset{s_3}{\Sigma}\frac{V_{_3}(m''_{s_3})}{\Delta(m''_{s_3})}P''_{s_3}e^{m''_{s_3}t}\right]$$

$$r = \text{the real part of } [\underset{s}{\Sigma}\{a_s e^{\lambda_s t} . R_{_1}(\lambda_s)\} + \Sigma(a''_0 + a''_1 t$$

$$+ \dots . a''_{\alpha-\beta''-1}t^{\alpha-\beta''-1})e^{m_0 t} . R_{_1}(m_0)]$$

$$-\text{the real part of } \Sigma \frac{P_0 e^{m_0 t}}{\Delta^\alpha(m_0)}\left[R_{_1}{}^\alpha(m_0) + aR_{_1}{}^{\alpha-1}(m_0)t\right.$$

$$\left. + \dots \frac{\alpha(\alpha-1)\dots(\beta''+1)}{\underline{/\alpha-\beta''}}R_{_1}{}^{\beta''}(mo)t^{\alpha-\beta''}\right]$$

$$-\text{the real part of}\left[\underset{s_1}{\Sigma}\frac{R_{_1}(m_{s_1})}{\Delta(m_{s_1})}P_{_1}e^{m_{s_1}t} + \underset{s_2}{\Sigma}\frac{R_{_2}(m'_{s_2})}{\Delta(m'_{s_2})}P'_{s_2}e^{m'_{s_2}t}\right.$$

$$\left. + \underset{s_3}{\Sigma}\frac{R_{_3}(m''_{s_3})}{\Delta(m''_{s_3})}P''_{s_3}e^{m''_{s_3}t}\right].$$

The free and forced vibrations contain the term $t^{a-\beta}$, being magnified to the $(a-\beta)$th degree, thus confirming the conclusion previously inferred as to the indefinite increase—$\Delta(m_s) = 0$—of the forced vibrations.

The solution for the symmetrical vibrations is now complete. $U_1(m)$, $U_2(m)$, $U_3(m)$; $V_1(m)$, $V_2(m)$, $V_3(m)$; and $R_1(m)$, $R_2(m)$, $R_3(m)$ are known, and their derivations with respect to m_0 can be found.

The reader will do well to refer to the remarks made on page 213 with respect to the minors in the free vibrations, and to read Routh's *Advanced Rigid Dynamics*—" Forced and Free Oscillations."

Asymmetrical Vibrations

The equations of motion are now :

$$\left(\frac{W\delta}{g} + Z_w\right)w + \left(\frac{W}{\delta}\cos\theta_0 + Z_p\right)p + \left(Z_q - \frac{WU}{g} - \frac{W}{\delta}\sin\theta_0\right)q$$

$$= -\sum_{s_1} Z_{s_1} e^{-n_{s_1}t}\cos(p_{s_1}t + \varepsilon_{s_1})$$

$$L_w w + \left(A\frac{\delta}{g} + L_p\right)p + \left(-F\frac{\delta}{g} + L_q\right)q = -\sum_{s_2} L_{s_2} e^{-n'_{s_2}t}\cos(p'_{s_2}t + \varepsilon_{s_2})$$

$$M_w w + \left(M_p - F\frac{\delta}{g}\right)p + \left(\beta\frac{\delta}{g} + M_q\right)q = -\sum_{s_3} M_{s_3} e^{-n''_{s_3}t}\cos(p''_{s_3}t + \varepsilon_{s_3}).$$

N.B.—The values n_{s_1}, n'_{s_2}, n''_{s_3}; p_{s_1}, p'_{s_2}, p''_{s_3}, and ε_{s_1}, ε'_{s_2}, ε''_{s_3} are not necessarily the same as those corresponding to the symmetrical disturbing forces.

Consider

$$P_{s_1} = Z_{s_1} e^{i\varepsilon_{s_1}}$$

$$P'_{s_2} = L_{s_2} e^{i\varepsilon'_{s_2}}$$

$$P''_{s_3} = M_{s_3} e^{i\varepsilon''_{s_3}}$$

and $m_{s_1} = -n_{s_1} + ip_{s_1}$; $m'_{s_2} = -n'_{s_2} + ip'_{s_2}$; and $m''_{s_3} = -n''_{s_3} + ip''_{s_3}$ then

$$w = - \text{ the real part of } \left\{\sum_{s_1} \frac{W_1(m_{s_1})}{\Delta(m_{s_1})}P_{s_1}e^{m_{s_1}t} + \sum_{s_2} \frac{W_2(m'_{s_2})}{\Delta(m'_{s_2})}P'_{s_2}e^{m'_{s_2}t} + \right.$$

$$\left. \sum_{s_3} \frac{W_3(m''_{s_3})}{\Delta(m''_{s_3})}P''_{s_3}e^{m''_{s_3}t}\right\}$$

$$+ \text{ the real part of } \sum_s \{(a_s e^{\lambda_s t})W_1(\lambda_s)\},$$

λ_s being a root as on page 212, and the last term containing four terms which are the free vibrations.

p = the real part of $\underset{s}{\Sigma}(a_s e^{\lambda_s t})P_1(\lambda_s)$.

$-$ the real part of $\{\underset{s_1}{\Sigma}\dfrac{P_1(m_{s_1})}{\Delta(m_{s_1})}P_{s_1}e^{m_{s_1}t}+\underset{s_2}{\Sigma}\dfrac{P_2(m'_{s_2})}{\Delta(m'_{s_2})}P'_{s_2}e^{m'_{s_2}t}+\underset{s_3}{\Sigma}\dfrac{P_3(m''_{s_3})}{\Delta(m''_{s_3})}P''_{s_3}e^{m''_{s_3}t}\}.$

q = the real part of $\underset{s}{\Sigma}\{(a_s e^{\lambda_s t}).\ Q_1(\lambda_s)\}.$

$-$ the real part of $\{\ \underset{s_1}{\Sigma}\dfrac{Q_1(m_{s_1})}{\Delta(m_{s_1})}P_{s_1}e^{m_{s_1}t}+\underset{s_2}{\Sigma}\dfrac{Q_2(m'_{s_2})}{\Delta(m'_{s_2})}P'_{s_2}e^{m'_{s_2}t}+\underset{s_3}{\Sigma}\dfrac{Q_3(m''_{s_3})}{\Delta(m''_{s_3})}P''_{s_3}e^{m''_{s_3}t}\}.$

Instead of the free vibrations being proportional to $W_1(\lambda)$, $P_1(\lambda)$, $Q_1(\lambda)$, they could have been taken proportional to $W_2(\lambda)$, $P_2(\lambda)$, $Q_2(\lambda)$ or $W_3(\lambda)$, $P_3(\lambda)$, $Q_3(\lambda)$.

Note that

$$\Delta(m)=\begin{vmatrix} \dfrac{Wm}{g}+Z_w & Z_p+\dfrac{W}{m}\cos\theta_o & Z_q-\dfrac{WU}{g}-\dfrac{W}{m}\sin\theta_o \\[2ex] L_w & A\dfrac{m}{g}+L_p & -F\dfrac{m}{g}+L_q \\[2ex] M_w & -F\dfrac{m}{g}+M_p & B\dfrac{m}{g}+M_q \end{vmatrix}$$

and that $W_1(m)$, $W_2(m)$, $W_3(m)$; $P_1(m)$, $P_2(m)$, $P_3(m)$; and $Q_1(m)$, $Q_2(m)$, $Q_3(m)$ are the cofactors of the constituents respectively of the first, second and third columns. To prevent confusion I might mention that there is no connection between the coefficient "P" of the disturbing forces and the cofactors "P" of the determinant.

The conclusions re $-$ large forced oscillations, etc., when $\Delta(m)=0$; re $-$ the failure of $\Delta(m)$ to be zero if the gusts are of the permanent periodic type; re $-$ the vanishing or limiting of the forced oscillations, *i.e.* a forced oscillation can be large, only if there be present, in the co-ordinate directly acted on, a free vibration of the same period and real exponential as those of the disturbing force; and also re $-$ the vanishing of the forced oscillations by means of two other conditions are the same as on the pages referring to the symmetrical oscillations.

Complete Solution

In the general case $\Delta(m)=0$ may have a roots equal to $m_o\ (=-n_o+ip_o)$ and $W_1(\delta)$, say, may have β roots also equal

to m_0. α and β cannot be greater than 4. They may differ from the similar symbols of the symmetrical solution (see pp. 217–219).

$$\therefore w = \text{the real part of } [\Sigma_s(a_s e^{\lambda s^t}).W_1(\lambda s) + \Sigma(a''_0 + a_1''t +$$

$$\ldots a''_{\alpha-\beta-1}t^{\alpha-\beta-1})e^{m_0 t}.W_1(m_0)]$$

$$- \text{the real part of } \Sigma \frac{P_0 e^{m_0 t}}{\Delta^\alpha(m_0)}\Big[W_1{}^\alpha(m_0) + aW_1{}^{\alpha-1}(m_0)t.+$$

$$\ldots \frac{a(a-1)\ldots(\beta+1)}{\underline{|a-\beta}}W_1{}^\beta(m_0)t^{\alpha-\beta}.\Big]$$

$$- \text{the real part of } \{\Sigma_{s_1} \frac{W_1(m_{s_1})}{\Delta(m_{s_2})}P_{s_1}e^{m_{s_1}t} + \Sigma_{s_2}\frac{W_2(m'_{s_2})}{\Delta(m_{s_2})}P'_{s_2}e^{m's_2 t} +$$

$$\Sigma_{s_3}\frac{W_3(m''_{s_3})}{\Delta(m''_{s_3})}P''_{s_3}e^{m''s_3 t}\}$$

Again' β may be zero.

$$p = \text{the real part of } [\Sigma_s\{(a_s e^{\lambda s^t}).P_1(\lambda_s).\} + \Sigma(a_0'' + a_1''t$$

$$+\ldots a''_{\alpha-\beta'-1}t^{\alpha-\beta'-1})e^{m_0 t}P_1(m_0)]$$

$$- \text{the real part of } \Sigma \frac{P_0 e^{m_0 t}}{\Delta^\alpha(m_0)}\Big\{P_1{}^\alpha(m_0) + aP_1{}^{\alpha-1}(m_0)t$$

$$+\ldots \frac{a(a-1)\ldots(\beta'+1)}{\underline{|a-\beta'}}.P_1{}^{\beta'}(m_0)t^{\alpha-\beta'}\Big\}$$

$$- \text{the real part of}\Big[\Sigma_{s_1}\frac{P_1(m_{s_1})}{\Delta(m_{s_1})}P_{s_1}e^{m_{s_1}t}.+\Sigma_{s_2}\frac{P_2(m'_{s_2})}{\Delta'(m'_{s_2})}.P'_{s_2}e^{m's_2 t}.$$

$$+\Sigma_{s_3}\frac{P_3(m''_{s_3})}{\Delta(m''_{s_3})}P''_{s_3}e^{m''s_3 t}\Big]$$

$$q = \text{the real part of}[\Sigma_s\{(a_s e^{\lambda s^t}).Q_1(\lambda_s)\} + \Sigma(a''_0 + a_1''t$$

$$+\ldots a''_{\alpha-\beta'-1}t^{\alpha-\beta''-1}).e^{m_0 t}.Q_1(m_0).]$$

$$- \text{the real part of } \Sigma \frac{P_0 e^{m_0 t}}{\Delta^\alpha(m_0)}\{Q_1{}^\alpha(m_0) + aQ_1{}^{\alpha-1}(m_0)t.$$

$$+\ldots \frac{a(a-1)\ldots(\beta''+1)}{\underline{|a-\beta''}}Q_1{}^{\beta''}(m_0)t^{\alpha-\beta''}\}$$

$$- \text{the real part of}\Big[\Sigma_{s_1}\frac{Q_1(m_{s_1})}{\Delta(m_{s_1})}P_{s_1}e^{m_{s_1}t}+\Sigma_{s_2}\frac{Q_2(m'_{s_2})}{\Delta(m'_{s_2})}P'_{s_2}e^{m's_2 t}$$

$$+\Sigma_{s_3}\frac{Q_3(m''_{s_3})}{\Delta(m''_{s_3})}P''_{s_3}e^{m''s_3 t}\Big].$$

Again the coefficients P''_s (not cofactors), the m's, m_0, λ_0, the a's, a's, and β's are not necessarily those of the symmetrical vibrations.

The cofactors of the determinant are known from page 220. Here, again, we see that the forced oscillations can be magnified.

General Conclusions

We shall see later that X_u, etc., can be found and therefore P_s, P'_s, P''_s, for approximately ideal and at the same time practical cases, in terms of a—the angle of incidence of the air on the planes—and the forward velocity U of the aeroplane.

If for certain ranges of U and a the aeroplane is so constructed that there are no multiple roots of $\Delta(\lambda) = 0$, and also because the values of λ will be of the type $-a_s \pm i\beta_s$ or $-n_s \pm ip_s$, there being resistances, the forced oscillations will not in general become large, since the disturbing forces are in general permanent, and the terms which stand for them will have no real exponential.

In a few cases we see that, the disturbing forces being evanescent, there is danger of an indefinitely large increase of the forced oscillations whether the roots are multiple or not. Again, where for certain ranges of the velocity U and the angle of incidence a there are multiple roots of $\Delta(\lambda) = 0$, the free vibrations once set up by an impulsive gust are magnified. Such a machine would be unstable for those ranges of U and a.

In the case of indefinitely increasing disturbed motion much depends on the aviator's skill. The vibration increases indefinitely about some axis, and excessive pitching and canting will occur. The moments of inertia, entering into the equations of motion, will affect this oscillation. The aviator then elevates or depresses, and turns his steering planes until stability is restored. Instinctively he has caused the aeroplane to strike the air so that the oscillation now takes place about a new axis relative to the machine. The moments of inertia, etc., about this axis not being the same as those about the old will alter the equations of stability and give new values for λ in the free vibrations. These values of λ may not then be multiple, nor be equal to the " m " of the disturbing force. The increasing disturbances are thus damped by a skilled aviator who possesses developed instinctive steering capabilities.

THE RESISTANCE DERIVATIVES

The following is a brief summary of and reference to certain chapters in Prof. Bryan's *Stability in Aviation* [1] :

[1] See pp. 38-56.

Symmetrical Derivatives

C is an arbitrary point (xy). P is the centre of pressure, so that $CP = a\phi(a)$ (a being small) and R is the normal thrust.
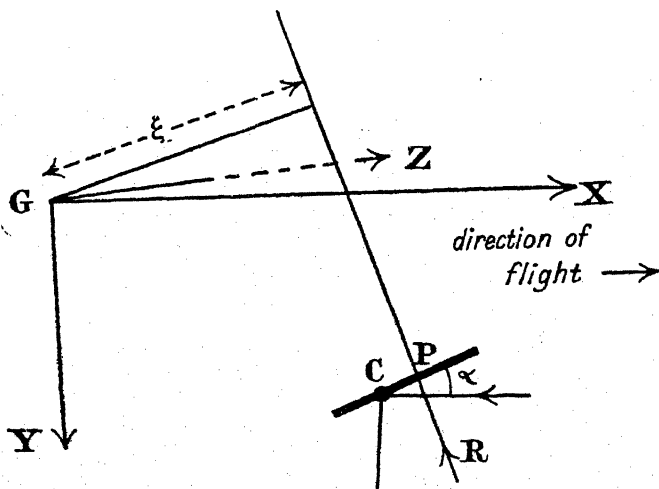
The aeroplane receives increments u, v, w, p, q, r, so that

$$\delta a = \frac{v + xr}{U}.$$

$$R = KS(U + \delta u)^2 f(a + \delta a) = KSU^2 f(a) + \frac{\delta R_o}{\delta u}\delta u + \frac{\delta R_o}{\delta a}\delta a$$

$$= KSU^2 f(a) + 2KSUf(a)(u - yr) + KSUf'(a)(v + xr)$$

$$\xi = x\cos a - y\sin a + a\phi(a) + a\phi^1(a) \cdot \frac{v + xr}{U}.$$

Prof. Bryan also finds that due to the rotation "r" of the plane about C, f(a), $\phi(a)$ are functions of $\frac{U}{r}$. He calls

$$f_r(a)\left(= \frac{\delta f(a)}{\delta r} \cdot U\right) \text{ and } \phi r(a)\left(= \frac{\delta \phi(a)}{\delta a}\right)U$$

the rotary derivatives.

$$X = R\sin a, \quad Y = R\cos a, \quad N = R\xi.$$

We then find that

$X_o = KSU^2 f(a)\sin a,$ $Y_o = KSU^2 f(a)\cos a,$

$X_u = 2KSUf(a)\sin a,$ $Y_u = 2KSUf(a)\cos a,$

$X_v = KSUf'(a)\sin a,$ $Y_v = KSUf'(a)\cos a,$

$X_r = KSU\{xf'(a) + f_r(a) - 2yf(a)\}\sin a,$ $Y_r = KSU\{f_r(a) + xf'(a) - 2yf(a)\}\cos a.$

$$N_o = KSU^2 f(a)\xi, \quad N_v = KSU\{f'(a)\xi + af(a)\phi'(a)\},$$

$$N_u = 2KSUf(a)\xi, \quad N_r = KSU\{f'(a)x - 2yf(a) + f_r(a)\}\xi +$$
$$KSUf(a) \cdot \{xa\phi'(a) + a\phi_r(a)\}.$$

See Prof. Bryan's work (p. 41).

$f_r(a)$, $\phi_r(a)$ can be found experimentally. For more than one plane we add the separate effects.

Theory to Find f(a)

Fix $CP = a\phi(a) = 0$, *i.e.* take the centre of pressure as the arbitrary point. Duchmein gives

$$R = 2R_{90}\cdot \frac{\sin a}{1 + \sin^2 a} = 2R_{90}\cdot\sin a \text{ (approximately).}$$

Prof. Bryan then obtains $f(a) \propto R$, $f(a) = \sin a$.

$$H = X_0 = \Sigma KSU^2\sin^2 a, \quad -Hh = N_0 = U^2\Sigma KS\xi'\sin a,$$
$$W = Y_0 = \Sigma KSU^2\cos a\sin a,$$

where $\xi' = x\cos a - y\sin a$, $\xi'' = x\cos a - 2y\sin a$ for brevity.

The sign Σ refers to more than one plane.

If the planes are narrow—as assumed above—$f_r(a)$, $a\phi_r(a)$, and $a\phi'(a)$ are negligible.

$$\frac{X}{U^2} = \Sigma KS\sin^2 a, \quad \frac{X_u}{U} = 2\Sigma KS\sin^2 a, \quad \frac{X_v}{U} = \Sigma KS\sin a\cos a, \quad \frac{X_r}{U} = \Sigma KS\xi''\sin a,$$

$$\frac{Y_0}{U^2} = \Sigma KS\sin a\cos a, \quad \frac{Y_u}{U} = 2\Sigma KS\sin a\cos a, \quad \frac{Y_v}{U} = \Sigma KS\cos^2 a, \quad \frac{Y_r}{U} = \Sigma KS\xi''\cos a,$$

$$\frac{N_0}{U^2} = \Sigma KS\xi'\sin a, \quad \frac{N_u}{U} = 2\Sigma KS\sin a\xi', \quad \frac{N_v}{U} = \Sigma KS\xi'\cos a, \quad \frac{N_r}{U} = \Sigma KS\xi'\xi''.$$

Allowances as to the above values must be made for the inclination (η) of the thrust H with the axis Gx, for head resistances, for propeller effects, and for the effect of the direction of flight relative to the horizon on the derivatives. See Prof. Bryan's work (pp. 75–122).

Development of the Asymmetrical Derivatives

See Prof. Bryan's work (pp. 123–164).

The law used is that of Newton. Resultant pressure on element $dS \propto$ resultant velocity $\times$ normal velocity of air relative to the machine.

An element dS is taken at (xyz) so that the direction cosines of the normal to it are l, m, and n. The velocity of the plane is U when increments u, v, w, p, q, and r are added.

$$X = \int Kl^2U^2dS + 2Uu\int Kl^2dS + Uv\int lmKdS + Ur\int l(mx - 2ly)KdS$$
$$Y = U^2\int KmldS + 2Uu\int KlmdS + Uv\int m^2KdS + Ur\int m(mx - 2ly)KdS$$
$$Z = Uw\int n^2KdS + Up\int Kn(ny - mz)dS + Uq\int n(2lz - nx)KdS$$
$$L = Uw\int Kn(ny - mz)dS + Up\int (ny - mz)^2KdS + Uq\int (ny - mz)(2lz - nx)KdS$$
$$M = Uw\int Kn(lz - nx)dS + Up\int K(ny - mz)(lz - nx)dS$$
$$\qquad + Uq\int K(2lz - nx)(lz - nx)ds$$
$$N = U^2\int Kl(mx - ly)dS + 2Uu\int Kl(mx - ly)dS + Uv\int m(mx - ly)KdS$$
$$\qquad Ur\int (mx - ly)(mx - 2ly)KdS.$$

Here we see that $(XYN)_{uvr}$ are the symmetrical group and $(ZLM)_{wpq}$ are the asymmetrical group, when the plane is symmetrical to $z = o$, and when $D = O = E$, odd powers of z and n being neglected in the above.

For planes bent up at an angle β the direction cosines of the normal are $\sin a$, $\cos a \cos \beta$, $\cos a \sin \beta$. Where $\beta = o$—i.e. the planes are normal to the plane $z = o$—these reduce to $\sin a$, $\cos a$, o, and we see that X_u, Y_u, etc., reduce to the values already found (p. 224).

Asymmetrical Derivatives $(\beta = o)$

$$Z_w = Z_p = Z_q = L_w = M_w = o.$$

Let $I =$ the moment of inertia of the plane with respect to xy similarly (density $= 1$), then $I = \int z^2 dS$.

$$L_p = KUI\cos^2 a, \qquad L_q = -2KUI\cos a \sin a,$$
$$M_p = -KUI\sin a \cos a, \quad M_q = 2KUI\sin^2 a.$$

Prof. Bryan concludes that fins are needed for stability in this case.

A Single Fin

Let its area $= T$; K' its coefficient of resistance; x_1, y_1, z_1 the co-ordinates of its centre of pressure $(l = o, m = o, n = 1$—i.e. it is parallel to the plane $z = o)$.

Then substituting in the expressions on page 224,

$$Z_w = -K'TU, \quad Z_p = K'TUy_1, \quad Z_q = -K'TUx_1$$
$L_p = K'U\{\text{moment of inertia of fin relative to the plane } y = o\}$
$L_q = -K'U\{\text{product of inertia with respect to } x = o, y = o\}$
$M_p = -K'U\{\text{product of inertia}\}$
$M_q = K'U\{\text{moment of inertia with respect to } x = o\}$
$L_w = K'TUy_1$; $M_w = -K'TUx_1$.

A Number of Small Fins. (General Case)

$T = \Sigma T_1 =$ sum of the separate areas $=$ Total area. $\bar{x}$, $\bar{y}$, $\bar{z}$ are the co-ordinates of the centre of pressure of all the fins.

M_1, M_2, and P are the moments and product of inertia respectively with respect to planes through $\bar{x}$, $\bar{y}$, $\bar{z}$, parallel to $y = o$, $x = o$ respectively. Then

$$L_p = K'U\{T\bar{y}^2 + M_1\}, \quad M_p = -K'U\{T\bar{x}\bar{y} + P\}$$
$$L_q = -K'U\{T\bar{x}\bar{y} + P\}, \quad M_q = K'U\{T\bar{x}^2 + M_2\}$$

and Z_w, Z_p, Z_q, L_w, and M_w hold good as in the last paragraph.

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Asymmetrical Derivatives for Two Transverse Planes

Let a_1, a_2 be the angles of attack, and I_1, I_2 the moments of inertia of the planes respectively.

Let $(I, 2a)$ be the vector sum of $(I_1, 2a_1)$ and $(I_2, 2a_2)$.

Then $Z_w = Z_y = Z_q = L_w = M_w = 0$ for the planes with values as above added for fins.

Take K the same for both planes. We find for such planes and fins as above that

$$L_p = KUI\cos^2 a + KU\{T\bar{y}^2 + M_1 + \tfrac{1}{2}(I_1 + I_2 - I)\}$$
$$L_q = -2KUI\cos a \sin a - KU\{T\bar{x}\bar{y} + P\}$$
$$M_p = -KUI\cos a \sin a - KU\{T\bar{x}\bar{y} + P\}$$
$$M_q = 2KUI\sin^2 a + KU\{T\bar{x}^2 + M_2 + \tfrac{1}{2}(I_1 + I_2 - I)\}$$

[Note that, from page 225,

$$L_w = KTU\bar{y}, \quad M_w = -KTU\bar{x}, \quad Z_w = -KTU, \quad Z_p = +KTU\bar{y}, \quad Z_q = -KTU\bar{x}$$

are due to the fins.]

$I_1 + I_2 - I$ may be proved to be positive. Prof. Bryan points out that M_2 increases stability, and therefore the additions— $\tfrac{1}{2}(I_1 + I_2 - I)$—to it will also do so. Two transverse planes with fins will give stability in steady motion. Frictional resistances, etc., will again effect the above values. The wash from the front plane to the back may be overcome by placing the back planes on a slightly higher level.

See pp. 150–164, *Stability in Aviation*, for $\beta \neq 0$. From these pages we may conclude that bent-up planes are equivalent to the planes $\beta = 0$ with fins, and therefore give stability.

STEREO-ISOMERISM AND OPTICAL ACTIVITY

A CRITICAL STUDY, WITH A NEW SUGGESTION

By G. S. AGASHE, M.Sc. (MANCHESTER), M.A. (BOMBAY)

PART I.—INTRODUCTORY

In the year 1808 Malus discovered the phenomenon of the polarisation of light. His pupil Arago discovered that quartz crystals possessed the power of rotating the plane of polarisation of polarised light, i.e. they were optically active. He further noticed that there were two modifications of crystalline quartz, which rotated the plane of polarisation in opposite directions.

Some years before, Abbé Haüy had noticed that there were two kinds of quartz crystals, possessing hemihedral facets on opposite sides of the crystal, thus constituting what are called enantiomorphous forms.

These two independent observations of Arago and Haüy were brought together by Sir John Herschel, who, in 1820, suggested a possible connection between the two phenomena of opposite rotation and the reversed position of facets on the crystals.

In the meanwhile (1815) Biot had discovered that many natural organic substances like sugar, oil of turpentine, and tartaric acid were optically active in the liquid or dissolved state. He also pointed out the difference between these substances and quartz, which loses optical activity, when the crystalline form is destroyed. But the suggestion of Herschel, just mentioned, was first applied to such substances by Pasteur,[1] who, in 1848, succeeded in preparing from sodium ammonium racemate (optically inactive) a mixture of sodium ammonium dextro- and lævo-tartrates, showing oppositely situated hemihedral facets, the crystals of the dextro-salt having them on the right, and those of the lævo-salt on the left.

[1] Chemical Society Pasteur Memorial Lecture, 1897.

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Having thus established the truth of the idea that asymmetry and enantiomorphism mark the property of optical activity, he went a step further, and pointed out that the asymmetry was due to the arrangement of molecules (or groups of molecules) in the crystal in the case of quartz, sodium chlorate, etc., which lost their activity with the crystalline structure, and to the arrangement of atoms in the molecule, in the case of tartaric acid, etc., which were active in the liquid or dissolved state.

The asymmetry of the crystal could be easily understood as a direct result of the presence of the facets, without any hypothesis as to the particular arrangement of the molecules in the crystal. But to explain molecular asymmetry some hypothesis as regards the arrangement of atoms seemed to be necessary.

"Are the atoms of right-handed tartaric acid," asks Pasteur, "arranged along the spiral of a right-handed screw, or are they situated at the corners of an irregular tetrahedron, or have they some other asymmetric grouping?" He is very diffident about the true answer, and remarks, "We cannot answer these questions. But of this there is no doubt, the atoms possess an asymmetric arrangement, having a non-superposable image."[1]

The step, which Pasteur hesitated to take, Van't Hoff took soon after, and explained molecular asymmetry or enantiomorphism, and consequently also optical activity in the liquid or dissolved state, by assuming the tetrahedral grouping, which is almost universally accepted at the present time.

Thus the idea, that enantiomorphism or asymmetry in a molecule is necessarily present when a substance is active in the dissolved condition, was thoroughly established, and has been abundantly confirmed by later research.

Chemists, however, have gone further. They have assumed first that enantiomorphism is the cause of optical activity, and secondly, as a corollary of this, that when a molecular configuration is asymmetric and enantiomorphous, the substance represented by that configuration must be necessarily optically active (or capable of being resolved into optically active isomers).

These assumptions seem to the writer to be quite unjustifiable.

In the first place, the evidence of crystallography, from which

[1] Alembic Club Reprints, No. 14.

all these ideas were brought into chemistry, is against them. Crystallographists recognise 230 possible point-systems, grouped in 32 classes, of which 11 classes give enantiomorphous crystal-forms. So, all optically active crystals, like quartz or sodium chlorate, belong to one of these 11 classes; but the converse of this is not true, and there are cases known where the crystals are enantiomorphous but optically inactive, *e.g.* barium nitrate.[1] This clearly shows that enantiomorphism is not always accompanied by, and cannot therefore be the cause of, optical activity.

This must hold good even in stereo-chemistry ; and thus we may get cases where the configuration of the molecule is enantiomorphous and still the substance is inactive.

Secondly, even if enantiomorphism were always accompanied by optical activity, it can hardly be regarded as the efficient cause of it. The nature of the phenomenon rather suggests something analogous to a twisted or screw-spiral structure in the substance. Not only the rotation produced by a naturally active substance can be removed by retraversing it, but also an optically active body can be, and has been, artificially prepared by piling together a number of mica plates in such a manner that the optical axis of each is turned through a definite angle with respect to that of the preceding plate. This makes it very probable that the cause of optical activity is screw-spiral structure of some sort, enantiomorphism being another simultaneous effect of the same cause.

This fact seems to have been well recognised in crystallography. It is by resorting to this that Sohncke[2] has tried to explain why barium nitrate crystals are optically inactive, while sodium chlorate crystals, belonging to the same crystal class, are active. According to him, barium nitrate possesses a point-system, in which there is no screw-spiral structure, while such a structure is present in the point-system belonging to sodium chlorate.

In stereo-chemistry, however, this fact has been entirely ignored, and we still find enantiomorphism described as the cause of optical activity. Logically speaking, if screw-spiral structure is the cause of optical activity, it must be assumed to be present in the configurations of optically active compounds.

[1] Tutton's *Crystallography and Practical Crystal Measurement*, p. 139.
[2] Tutton's *Crystals*, p. 151.

In the case of crystals, the arrangement taken into consideration was that of the molecules or groups of molecules in the crystal structure; here, of course, the arrangement of atoms or groups of atoms in the molecule itself will have to be considered.

So the problem is to suggest an hypothesis as regards the arrangement of atoms in the molecule, which will satisfy the two conditions of showing the screw-spiral structure to be present in the configuration of all optically active compounds, and showing it to be absent in that of all the inactive compounds. The following is an attempt to solve this problem with reference to compounds of carbon and nitrogen.

PART II.—COMPOUNDS OF CARBON

It has been mentioned already, that Van't Hoff assumed the tetrahedral grouping for the four radicals joined to a carbon atom, the carbon itself being at the centre of the tetrahedron. He did not commit himself as to the nature of the tetrahedron, because it was unnecessary for his purpose; the structure becomes asymmetric and enantiomorphous, when all the four radicals are different, whatever the nature of the tetrahedron, and enantiomorphism by itself apparently seemed to him quite sufficient to account for optical activity. But if it is not enough, and if some sort of screw-spiral arrangement of the radicals has to be postulated, we shall be obliged to make some further assumptions about the tetrahedron. The assumptions suggested below seem to the writer very plausible and dynamically sound.

The linkages of the carbon may be pictured as the horns of a snail; they can be pushed out or pulled in, and can also be twirled round, their orientation being determined by the four radicals attached to them. The configuration of a substance like methane or carbon tetrachloride may be represented by a regular tetrahedron. The structure will possess its full number of planes of symmetry, viz. six; the distance of each radical from the central carbon will be the same; the angle between any pair of linkages will be equal to that between any other pair; and so on.

This high degree of symmetry will gradually diminish as more and more different groups appear. Thus, for example, in a compound of the type C,a_3,b, the distance of all the a's from the central carbon will be the same, but will be different from

the distance of b from the central carbon; any pair of a's will contain the same angle, but this angle will be different from the angle contained by b and any of the a's; the structure will possess only three planes of symmetry; and so on.

Finally, when the groups are different, the structure becomes perfectly irregular, devoid of any plane of symmetry, having all distances different, all angles different.

When a molecule of such a perfectly irregular configuration lies in the path of a ray of plane-polarised light, let us suppose the direction of the ray to lie along one of the carbon-bonds. Then it is easy to see that the other three bonds will not lie symmetrically round the ray, but will be found to be twisted out of shape in such a manner that the line joining the centres of inertia of the three groups attached to them will describe a spiral round the ray, as shown diagrammatically in the figure:
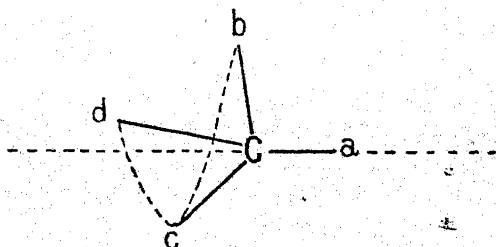
And it does not seem unreasonable to suppose that this twisting of the bonds will produce the effect of rotating the plane of polarisation, and also that the amount of rotation will be directly proportional to the degree of this twist in the orientation of the bonds round the ray.

This twist will be present along whichever of the four bonds we imagine the path of the ray to lie. Whether the twisting of the bonds in each case will be the same or not, the writer cannot say for certain. Most probably it will be equal; but even if it is not, the principle of "least resistance" will come into operation, and as the molecules are perfectly mobile they will take such a position with reference to the path of the ray as will produce minimum rotation.

Further, even if the path of the ray lies along none of the four bonds, the screw-spiral twisting will still be there; and whether the molecule will take any such position or not will depend on whether the twisting is the least or not in that position. But

this seems highly improbable. Most probably the position involving least rotation will be such as to have the path of the ray along one of the bonds.

It is obvious that the twisting will be equal but in the opposite direction in the other enantiomorph.

The orientation of the radicals, and consequently also the degree of twisting of the bonds, is, as has been indicated already, most probably determined by two factors: (1) The affinity of the central carbon to each of the four radicals, and (2) their action upon each other. Both of these may indeed be grouped together under the one heading of the chemical nature of the groups. This being the case, it seems almost impossible to find a quantitative relation between the degree of rotation produced and the nature of the groups—at least, in the present state of our knowledge; and it is no wonder that all attempts at such a co-ordination, based upon only one property of the groups, viz. their mass (which alone lends itself to a quantitative treatment, but which is nevertheless probably the least influential in the matter under consideration), have entirely failed.

With the help of this idea, it further becomes easier to understand why the amount of rotation of one and the same substance changes with the external conditions like temperature and solvent. The amount of rotation changes for the simple reason that the chemical nature of a group changes with the external conditions.

Now we shall consider the cases (1) Ca_3b, (2) Ca_2b_2, and (3) Ca_2bc.

If we take the first case, for example, we find that it is indeed possible to imagine a direction for the path of the ray through such a molecule, which will have the groups arranged in a spiral round itself; but that matters little. What we have to decide is whether there is a direction possible for the ray, which can avoid this twisting, and the consequent rotation of the plane of polarisation. Because, if there is such a direction, then the molecule being mobile will assume the corresponding position, in accordance with the principle of "least resistance." And it is not difficult to see that there is such a direction in each of the three types under consideration.

In the first case, such a direction is that of the bond between the central carbon and b.

In the second case, it is the direction joining the central

carbon to the middle points of the straight lines joining a—a or b—b.

In the third case, it is the line joining the central carbon to the middle point of the straight line joining a—a. When the ray passes along that direction, the two other groups b and c can in no sense be said to describe a spiral round it.

Thus it is clear, that substances of these types will not be optically active according to this new hypothesis; and none such are known.

The same considerations apply in cases where there are more than one asymmetric carbons.

Of these, we need only consider the apparently anomalous case of trihydroxy-glutaric acid.

$$COOH.\overset{+}{C}HOH.CHOH.\overset{-}{C}HOH.COOH$$

When in this formula the two side carbons are of opposite sign, they neutralise each other's optical effect, but make the central carbon asymmetric; but the difference in the nature of the two groups is not of a kind calculated to have any effect on the twisting of the bonds; as far as that is concerned, the substance is of the type Ca_2, b, c; the structure as a whole does possess a plane of symmetry, and thus shows no optical activity. But the isomerism manifests itself in different chemical and physical properties; it thus suggests an analogy with the cis-trans-isomerism in the alicyclic compounds.

A carbon, like the central carbon here, which is united with four radicals, which are not all different structurally, but only so configurationally, is called a "pseudo-asymmetric" carbon.

LE BEL'S VIEWS

The view of the spatial distribution of the four valencies of carbon, put forth above, comes very near to that of Le Bel. Le Bel's ideas appear to the writer to be more sound; but they were not further developed because they were more complicated than the rigid ideas of Van't Hoff. Although Van't Hoff originally made no definite statement as to the nature of his tetrahedron, all the further developments of the tetrahedron hypothesis have been based on the tacit assumption that it is regular. All this is very clearly shown in the case of

The Ethylenic Linkage

Let us consider a substance of the following configuration :

$$a\!\!>\!\!C\diamondsuit C\!\!<\!\!c$$
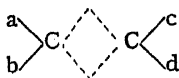
In such a configuration, according to the Van't Hoff hypothesis, a, b, c, and d all lie in one plane, which is at right angles to the plane containing the linkages joining the two carbons. So, the structure does have a plane of symmetry, and it is identical with its mirror-image ; and so no optical activity is to be expected.

On the other hand, according to Le Bel's ideas (and also according to the ideas set forth above), the four groups a, b, c, and d may not, and very probably will not, lie in the same plane. The structure thus may become asymmetric and enantiomorphous ; and the possibility of optical activity arises.

In fact it was at one time expected to get optically active substances of such a configuration ; and Le Bel[1] himself carried out a number of experiments with the hope of isolating them. Similar researches were made by Anschütz and Walden ; but all of them were unsuccessful ; and now it is generally agreed that there is no possibility of optical activity in such compounds.

It appears that this was considered as a great difficulty in the way of accepting Le Bel's views. Now please notice the tacit assumption made here, that asymmetry and the consequent enantiomorphism necessarily imply optical activity, which assumption appears to the writer to be unjustifiable. According to the ideas set forth above, there must be something else present besides enantiomorphism, viz. the unsymmetrical spatial distribution of the linkages, and the screw-spiral arrangement of the radicals round the carbon. This is obviously not the case here ; for the two linkages of each carbon, by which it is joined to the other carbon, may be regarded as acting along practically the same line. And so there is no real difficulty in reconciling the absence of optical activity, which is an experimental fact, and the presence of enantiomorphism, demanded by Le Bel's hypothesis.

The case of the acetylenic linkage is simpler still, and need not be further considered.

[1] For references to the original papers, see Stewart's *Stereochemistry*, p. 158.

The Difference between Saturated Open-Chain- and Ring-Compounds

So far the two terms "asymmetry" and "enantiomorphism" have been used as being coextensive in their denotation. This is quite true, if we define an asymmetric carbon as one that is united to four structurally different radicals, and call it "pseudo-asymmetric" if any of the radicals are structurally similar, but differ only in configuration, *but it is true only in the case of open-chain compounds.*

In open-chain-compounds of all types (except one, for which see p. 241) the following three relations hold good :

(1) The presence of an asymmetric carbon makes the whole structure both asymmetric and enantiomorphous; and conversely all asymmetric and enantiomorphous structures contain at least one asymmetric carbon.

(2) The presence of a pseudo-asymmetric carbon does not make the structure asymmetric or enantiomorphous : *e.g.* the inactive indivisible tri-hydroxyglutaric acids.

(3) And further, a meso-pair of asymmetric carbons makes the whole structure symmetric and also, of course, identical with its mirror-image : *e.g.* meso-tartaric acid, mucic and allo-mucic acids.

But these relations do not always hold good in alicyclic or saturated

Ring-Compounds

In these, the presence of an asymmetric carbon does indeed make it asymmetric and enantiomorphous ; but the converse is not always true, asymmetry and enantiomorphism being often effected by one or more pseudo-asymmetric carbons.

Let us, for example, consider the case of

Inosites

These have the constitutional formula $C_6H_6(OH)_6$. A constitutional formula of this type admits in all of nine configurations, shown below. Three isomers only are known so far ; one is of the inactive indivisible type, the other two being optical antipodes.
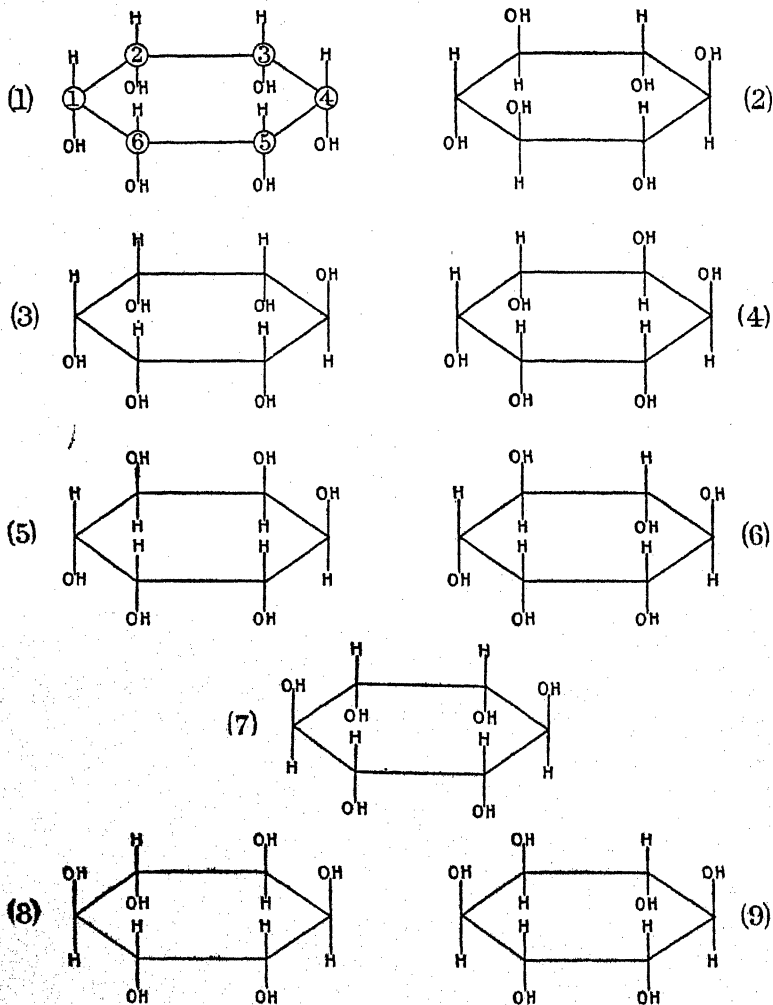
[N.B.—The numbering of the carbons is the same in all cases.]

It is easy to see that in this case there is no truly asymmetric carbon at all; but in each of the configurations all the carbons are pseudo-asymmetric. In some cases, we find one carbon neutralising the pseudo-asymmetry of another, *e.g.* carbons 3 and 5 in configuration No. 3.

In configuration No. 1 all the carbons are pseudo-asymmetric in the same sense, there being no meso-pair at all. The molecule as a whole is symmetric and identical with its mirror-image. In Nos. 2–7, also, we find the structures symmetric.

But when we come to No. 8, we see at once that here the molecule is not only asymmetric, but also non-superposable on its mirror-image, which is No. 9. Evidently these two configurations represent the two optical isomers.

Here we have asymmetry, enantiomorphism, and optical activity, without the presence of an asymmetric carbon.

Let us consider another important case among the alicyclic compounds, viz. that of molecules having the so-called

Indirect Plane of Symmetry

Ladenburg[1] was the first to draw attention to what he thought to be the exceptional character of a configuration like this :
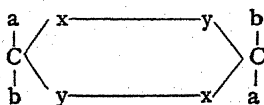
It contains two truly asymmetric carbons forming a meso-pair; but the structure as a whole possesses *no plane of symmetry*, although it is identical with its mirror-image. Here again the behaviour of a meso-pair is different from what it is in open-chain-compounds.

Several examples of this type are known : *e.g.* the keto-form of trans-succinylo-succinic acid, and trans-3,6-dimethyl-1,4-cyclo-hexadiene-1,4-dicarboxylic acid ; and they are all inactive.

There are some substances of this class known which contain two meso-pairs : *e.g.* 1,3-dimethyl-cyclobutane-2,4-dicarboxylic acid.
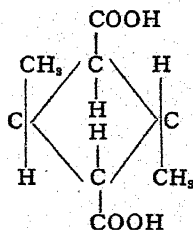
Here again the structure is asymmetric.

These examples clearly show the difference in behaviour between open-chain- and ring-compounds. The cause of this

[1] *Ber.* 28, 1995, 3104 (1895).

difference is not far to seek. It is the same which gives rise to other differences between saturated open-chain and ring-compounds, like cis-trans-isomerism ; viz. that ring-formation deprives the two end-carbons of a chain of their free rotation.

The writer has nowhere seen this difference put in the form which is here given to it. It usually appears in another form, viz. in the distinction that is drawn between ordinary asymmetry, where it can be referred as being due to a particular asymmetric carbon, and

The so-called Molecular Asymmetry

where it is not so referable, as for example, in the case of inosites. This distinction is considered by many chemists to be unnecessary and even illogical ; and so it appears, when stated in such a form ; because all optically active molecules are asymmetric, whether they contain an asymmetric carbon or not. Further, it is to be noted that all substances whose activity is alleged to be due to the asymmetry of the molecule as a whole, are ring-compounds (the only open-chain grouping, which, if realised, will fall in this category, is the allene grouping, which will be fully discussed presently). For these reasons, it appears to the writer both logical and convenient to state this difference as a difference between saturated open-chain- and ring-compounds.

This can be further illustrated by taking a concrete example, which has been a subject of great controversy recently. In 1909, Perkin, Pope, and Wallach[1] synthesised

1–Methyl-cyclohexylidene–4–Acetic Acid

$$CH_3 \diagdown C \diagup CH_2 \text{------} CH_2 \diagdown C \text{:::::::::} C \diagup H$$
$$H \diagup \diagdown CH_2 \text{------} CH_2 \diagup \diagdown COOH$$
$$(1) \qquad\qquad (4) \qquad (7)$$
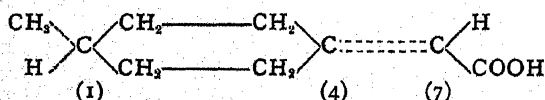
which they subsequently succeeded in resolving into optical isomers.

[In the configuration, all the linkages represented by whole lines lie in one plane, while the linkages represented by the dotted lines lie in a plane at right angles to the first, according to the Van't Hoff view, and in any other plane or planes, according to the writer's view.]

[1] *Trans. Chem. Soc.* 1909, 1789.

The structure as a whole is devoid of any plane or symmetry, and is not identical with its mirror-image; but here, as in the case of the inosites, there is no truly asymmetric carbon, although there is a pseudo-asymmetric one, viz. C (1).

The authors maintain that this is a case where the optical activity is due to the asymmetry of the molecule as a whole; while Everest[1] and others maintain that C (1) can be regarded as asymmetric, by a suitable modification of the definition. Now this latter view is only a round-about and clumsy way of putting the distinction between open-chain- and ring-compounds, which has been alluded to above. The former view emphasises this distinction more strongly (although in a different form) than the latter, and so far it is better. But what is meant by saying that the activity is due to the asymmetry of the whole molecule? We have seen that this has no meaning, that the optical activity cannot be regarded as being *produced* by asymmetry, but must be regarded as an effect of a screw-spiral structure of some sort. Such an arrangement, as far as the writer can see, can only be regarded round one particular carbon, and not round the whole ring; and that particular carbon in this case must be C (1). And this is where Everest's view is more suggestive than the other view.

To recapitulate: we started with the fact that enantiomorphism does not necessarily involve optical activity in crystals; further it was pointed out, that even if it were the case, enantiomorphism can hardly be considered as the efficient cause of optical activity, but that the nature of the phenomenon suggests something of the nature of a screw-spiral arrangement of particles as its probable cause. And then an attempt was made to apply this idea to the various types of carbon compounds, which are optically active in the liquid or dissolved state, and in which, therefore, the activity is due to the arrangement of atoms in the molecule.

So far, the new hypothesis has given us nothing essentially new. It has only satisfied what seems to the writer to be a logical necessity. This logical necessity may not perhaps obtain general admittance for the hypothesis, unless it has been put to a more concrete test. This test is fortunately supplied by the following important case.

[1] *Chem. News*, 1909, 100, 295.

Van't Hoff[1] has predicted that a molecule of the allene type

$$\underset{y}{\overset{x}{>}}C : C : C\underset{y}{\overset{x}{<}} \quad \begin{matrix} (a) \\ (b) \end{matrix}$$
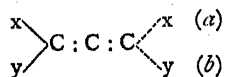
will be optically active, inasmuch as it is asymmetric and enantiomorphous. Substances of this type are very unstable and very difficult of preparation. In 1910 Lapworth and Wechsler[2] prepared a substance which they thought to be

$$\underset{C_{10}H_7}{\overset{C_6H_5}{>}}C : C : C\underset{COOH}{\overset{C_6H_5}{<}}$$

diphenyl-naphthyl-allene-carboxylic acid.
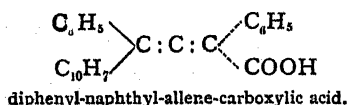
They tried to resolve it into two optical isomerides by the usual methods, but were unsuccessful. But on account of the great difficulty of handling such substances, their experiments cannot be regarded as decisive. The question is still an open one, and there is room for prediction.

According to the ideas put forth in the preceding pages, substances of such a configuration should not be optically active, in spite of enantiomorphism, for want of the necessary screw-spiral structure. The deductions drawn from the two hypotheses are at variance with each other in this case, which will therefore serve as an excellent test-case.

It is usually argued that a structure like this

$$XHC\underset{(CH_2)_n}{\overset{(CH_2)_n}{<}}C\underset{(CH_2)_n}{\overset{(CH_2)_n}{<}}CHX$$
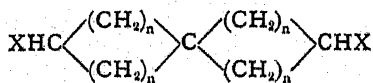
simulates the allene structure, for all practical purposes, so far as optical activity is concerned. The writer ventures to doubt this. He submits that there is a world of difference between the two. In the allene type the presence of the double bonds makes a screw-spiral structure impossible; but such is not the case in the other type, where the spatial distribution of the linkages is similar to that in the case of an ordinary carbon, thus making a screw-spiral structure possible.

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[1] La Chimie dans l'espace (1875).
[2] Trans. Chem. Soc. 1910, 38.

[SUPPLEMENTARY NOTE

On p. 235 it has been stated that certain relations hold good in open-chain-compounds of all types, except one. The exception is of such an extraordinary character that it deserves some attention in this place. It may be represented by the following general formula:[1]

$$\text{Rd}\diagdown\diagup\text{Rl}$$
$$\text{C}$$
$$\text{Rl}\diagup\diagdown\text{Rd}$$
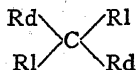
where Rd and Rl represent two enantiomorphous radicals. If a model of such a formula be constructed, it will be found that the structure as a whole is devoid of any plane of symmetry, if the symmetry of the radicals also is taken into consideration. This is shown in the following figure:
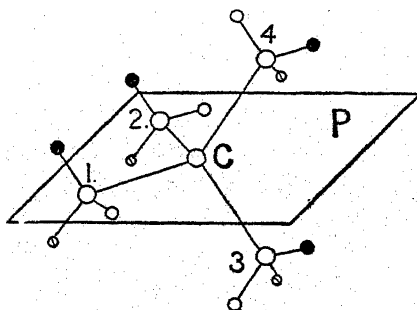
If P is a plane that passes through the central carbon so as to make 3 and 4 lie symmetrically on either side, it does cut 1 and 2 asymmetrically, as in each case the black ball is opposed by the dotted ball, the white being supposed to lie in the plane itself; and the same will be found to be the case with every plane.

But this configuration is identical with its mirror-image.

If the two pairs of radicals are, however, joined up to form two rings, so that the central carbon is a member of both the rings, we get a structure like this:

$$\text{XHC}\diagup^{(CH_2)_n}\diagdown_{(CH_2)_n}\diagup\text{C}\diagup^{(CH_2)_n}\diagdown_{(CH_2)_n}\diagup\text{CHX}$$
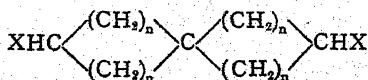
which is enantiomorphous. This again brings out the distinction between open-chain- and ring-compounds.]

[1] Mohr, *J. Pr. Chem.* [2] **68**, 369 (1903).

16

PART III.—COMPOUNDS OF NITROGEN

TERVALENT NITROGEN

When we consider a compound, in which a nitrogen atom is linked to three univalent atoms or groups, two configurational formulæ at once suggest themselves to us. The first is the plane formula, in which all the valencies lie in the same plane. The second is the tetrahedral formula, in which the nitrogen occupies one corner of the tetrahedron, and the three atoms or groups the remaining corners, the valencies being directed along the edges. Facts must decide which of these two is the more probable one.

According to the usual idea, the tetrahedral formula necessitates the existence of optical isomers, when all the three groups attached to the nitrogen are different. But all attempts made up till now to resolve substances of that kind into optical isomers have invariably failed. Neither are there any facts that give any hope of success in the matter. So the general tendency now is towards giving up the tetrahedral formula, and accepting the plane one.

On the other hand, there are many facts that tell against the plane configuration. The most important of these is the existence of two isomers in case of substances like aldoximes, ketoximes, hydrazones, etc., and the diazo-compounds. It has been conclusively proved that the isomers in each of these cases are structurally identical, and must therefore be stereo-isomers, and the hypothesis of Hantzsch and Werner [1] is generally accepted as the true explanation of the isomerism. Hantzsch and Werner assign the following configurations to the isomers in the different cases :

	Syn-form.	Anti-form.
Aldoximes	$\begin{array}{c} R\text{—}C\text{—}H \\ \parallel \\ N\text{—}OH \end{array}$	$\begin{array}{c} R\text{—}C\text{—}H \\ \parallel \\ HO\text{—}N \end{array}$
Ketoximes	$\begin{array}{c} R\text{—}C\text{—}R' \\ \parallel \\ N\text{—}OH \end{array}$	$\begin{array}{c} R\text{—}C\text{—}R' \\ \parallel \\ HO\text{—}N \end{array}$
Diazo-compounds	$\begin{array}{c} Ar\text{—}N \\ \parallel \\ X\text{—}N \end{array}$	$\begin{array}{c} Ar\text{—}N \\ \parallel \\ N\text{—}X \end{array}$

[1] *Ber.* **23**, 11 (1890).

Here the two nitrogen-bonds that join it to the carbon or the other nitrogen are supposed to be in one plane, while the third bond lies in a different plane. This creates a strong presumption in favour of the tetrahedral formula.

The hypothesis of Hantzsch and Werner, although it explained the numerous phenomena in question in a beautifully simple manner, did not make its way unopposed. Even now it is accepted by chemists with considerable reserve. The reason is, that it seems impossible to understand by what mysterious forces the nitrogen-bonds are deviated from their normal arrangement in one plane.

Evidence of an interesting kind has been recently brought forward by Mills and Bain,[1] in support of the Hantzsch-Werner hypothesis. These workers prepared the oxime of cyclo-hexanone-4-carboxylic acid :

$$\begin{array}{c} H\diagdown \qquad \diagup CH_2\text{------}CH_2\diagdown \\ \quad C \qquad \qquad \qquad \qquad C\text{---------}N.OH \\ COOH\diagup \quad \diagdown CH_2\text{------}CH_2\diagup \\ \quad (4) \qquad \qquad \qquad \qquad (I) \end{array}$$
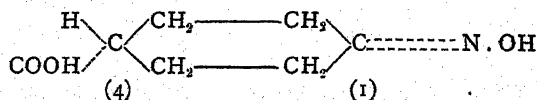
and found that this acid forms both dextro- and lævo-rotatory salts of the alkali metals. Now in this configuration, whether we consider the optical activity as due to the asymmetric C (4), or we consider it as due to the asymmetry of the whole molecule, its mere presence demands that the single bond joining OH to N lies in a plane different from that of the other two bonds. This fact obviously gives considerable support to the Hantzsch-Werner hypothesis.

So here there seems to be a dead-lock. One set of facts requires one configuration for tervalent nitrogen, another set requires another. Now let us see if the new hypothesis helps us out of the difficulty.

As has been mentioned above, it has been tacitly assumed that a tetrahedral formula for tervalent nitrogen will require the existence of optical isomers, when all the three groups attached to it are different, because it will give two enantiomorphous configurations. But we have seen already that this assumption is not valid. Besides enantiomorphism, some sort of screw-spiral structure must be present in the configuration, if it is to show optical activity.

[1] *Trans. Chem. Soc.* 1910, 1866.

So let us see whether a compound N, a, b, c has such a structure, when we represent it by the tetrahedral formula. In order to find out whether a given configuration will be optically active or not, we have simply to ask the question (as we have done before in the case of carbon-compounds), whether it is possible for the plane-polarised ray to find a direction through the molecule, such that there will be no forces round it, that will tend to twist the plane of polarisation. If there is no such direction, then the molecule will lie in such a position as will produce minimum rotation. But if there is such a direction, then the molecule, being mobile, will take the corresponding position, in accordance with the principle of "least resistance."

Now, clearly there is such a direction possible in the configuration under consideration. Suppose the ray passes along one of the three bonds; then it is clear that the remaining two groups can in no sense be described as lying on a spiral round the ray, and hence there will be no rotation.

Then there arises the further question, whether the isomerism of the two enantiomorphous configurations :
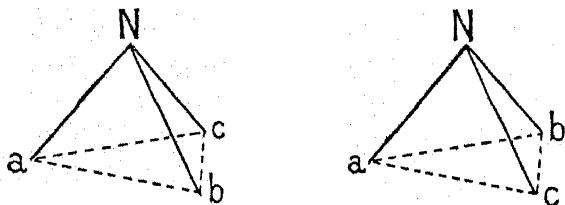
will at all be made manifest in any of the other physical or chemical properties. By analogy of carbon-compounds, it seems probable that the two configurations will be identical in physical and chemical properties. The evidence of facts has so far been of a very indecisive character; and it is too early yet to form any conclusion in the matter. But if we look at all the cases[1] of alleged differences in properties of such isomers, one fact at once strikes our notice, viz. that in all the cases the groups attached to the nitrogen are of a very complex character.

For examples see Stewart's *Stereochemistry*, p. 264.

Pentavalent Nitrogen

If one has to suggest a possible configuration for a compound containing pentavalent nitrogen, one must bear in mind all the facts which are known at present, and which it must satisfactorily explain. One has to take the following facts into consideration, viz. (1) existence or non-existence of optical isomers in the different types, (2) existence or non-existence of ordinary stereo-isomers, and finally (3) derivation from tervalent nitrogen.

Various configurations have been suggested; but we need not discuss all of them here. The one that explains the facts most satisfactorily, and is therefore most in vogue, is the pyramidal formula of Bischoff.
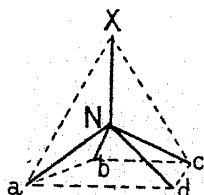
Let us see how it works out in the different cases.

For convenience, let us consider the type N, a, b, c, d, x, first. Optically active substances of this type have now been conclusively proved to exist, and Bischoff's formula, as can be easily seen, accounts for the optical activity; but the formula demands two stereo-isomers (each being divisible into d- and l-enantiomorphs) that are not yet known to exist.

X   X

N

Further, there is the difficulty of deriving it from the generally accepted plane configuration of tervalent nitrogen.

H. O. Jones[1] has attempted to explain the absence of the two

[1] *Trans. Chem. Soc.* 1903, 1403; 1905, 1721.

stereo-isomers. He points out that all the substances of this type thus far prepared have been prepared from tervalent compounds of the type N, a, b, c; and when we add a substance like d x to it, the new group d chooses that position with respect to the already existing radicals which produces the most stable configuration. He starts with the plane formula of tervalent nitrogen, and ends with the pyramidal formula for the pentavalent nitrogen. He represents the changes thus :
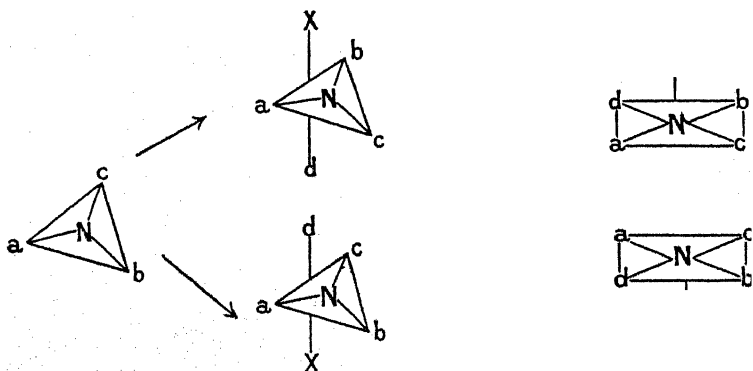
The last two configurations are enantiomorphous and represent the d- and l- modifications of the one stable isomer.

Jones's theory gives an ingenious explanation of the absence of stereo-isomers; but the weak point in his theory is that it does not give an adequate explanation of the deviation of the nitrogen-bonds from their original arrangement in one plane into two different directions as represented above. In fact, his hypothesis is open to the same objection as the Hantzsch-Werner hypothesis.

It has been shown above, while discussing the formula of tervalent nitrogen, that we may assign a tetrahedral configuration to it, if any facts demand it, in spite of the fact that no tervalent nitrogen compounds show optical activity; and so there is no difficulty about the whole question at all.

If we assume with Jones that the most stable configuration results by the addition of d x to N, a, b, c, the change from tervalent to pentavalent nitrogen, with only one pair of enantiomorphs being formed, can be very simply represented as follows :
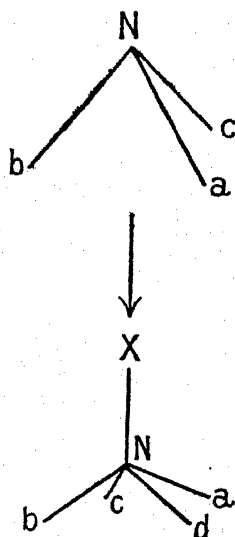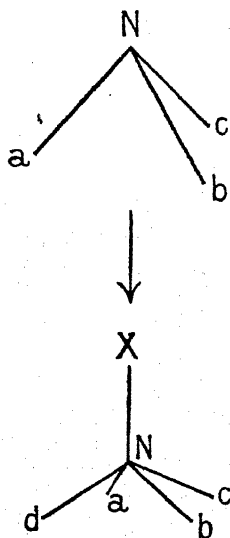
The two resulting substances are enantiomorphs, as are also their originals. But now the molecule has become more complex, so that it is no longer possible for the plane-polarised ray to find a direction through the molecule, so as to avoid having the atoms or groups arranged in a spiral round itself, because the pyramid is irregular,[1] the four groups a, b, c, and d being all different. Hence the configurations will be optically active, one being dextro- and the other lævo-rotatory.

Now passing on to the other types, we find that in the type Na_3bx no stereo-isomers are possible, and none are known. The case of trimethyl-ethyl-ammonium-iodide, which was at first thought to be a case of stereo-isomerism, is now shown to be only one of dimorphism.

As regards optical activity, that also is not possible, as the molecule certainly gives a smooth path to the ray in at least two directions. Suppose, for instance, the ray lies along x—N, then the line joining the four radicals a, a, a, and b will not be

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[1] The conception of the nature of the linkages is the same here as in the case of carbon. In the case of carbon, however irregular the spatial distribution of the linkages and the radicals, the resulting figure could always be accurately described as a tetrahedron. But here the distribution of the linkages and the radicals will often make the formation of a pyramid impossible. Still, in the sequel, the word "pyramid" has been used loosely to describe the resulting polyhedron in all cases.

a spiral because three of them are identical, and so there will be a break. The same is also true of the direction b—N.

In the type Na_2bcx, stereo-isomerism is possible, as the two identical radicals a, a may lie opposite or contiguous to each other at the base of the pyramid. The evidence of facts is inconclusive. Schryver and Collie[1] first succeeded in preparing two crystalline modifications of dimethyl-ethyl-ammonium chloro-platinate; but there is no evidence to prove that the phenomenon is not due to dimorphism, which was shown to be present in the last case. Other attempts in this direction have been equally unsuccessful.

In both the possible isomers a smooth path is possible for the polarised ray along N—x, because, in that case, the line joining the other four groups a, a, b, c will not be a continuous spiral, as two of the groups are identical, and so there will be a break. So substances of this type will not show optical activity, and none has been observed so far.

In all the three cases we have so far considered, the theory is quite open so far as stereo-isomers (other than optical isomers) are concerned. The absence of such in each case can be explained by Jones's hypothesis, referred to above, viz. that the most stable configuration is produced; but if in future facts are discovered proving conclusively the existence of such isomers, we have simply to drop this assumption, without making any other changes in the general conception.

An interesting case, apparently similar to but really quite different from the last one above considered, is that of amino-oxides:

$$\begin{array}{c} a \\ b \\ c \end{array}\!\!\!>\!N=O \quad \text{or} \quad \begin{array}{c} a \\ b \\ c \end{array}\!\!\!>\!N\!\!<\begin{array}{c} OH \\ OH \end{array}$$
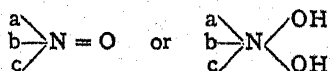
In 1908 Meisenheimer[2] showed that methyl-ethyl-aniline oxide could be resolved into two active components. But at that time it could not be decided whether the free active bases were true amino-oxides or the corresponding di-hydroxy-compounds, the general tendency of chemists being in favour of the di-hydroxy constitution. But recently Meisenheimer[3] has proved that these substances are optically active when dissolved

[1] *Proc. Chem. Soc.* 1891, 39.
[2] *Ber.* **41**, 3966 (1908).
[3] *Annalen*, **385**, 117 (1911).

in anhydrous benzene, in which solvent they can only be present as true oxides.

The optical activity in this case is easily explained. The oxygen is linked up to two nitrogen-bonds; these were originally at an angle, but may now be supposed to be practically parallel and very close to each other—in fact, equivalent to one bond as far as the spatial arrangement of groups or radicals is concerned. The whole structure thus becomes tetrahedral, exactly like the carbon structure; and as the four radicals are different, optical activity is to be expected.

Conclusion

In the foregoing pages the writer has tried to show that the idea that optical activity is not a result of enantiomorphism, but that both of them (where they coexist) are results of another structural cause, viz. the screw-spiral arrangement, although recognised by crystallographists, has been ignored entirely by chemists, in spite of the fact that what holds good in crystallography, as regards optical activity, must also hold good in chemistry, with this difference, that while the crystallographist deals with the arrangement of molecules (or some other higher units) in the crystal structure, the chemist deals with the arrangement of atoms within the molecule itself. He has further tried to show that the same idea can be successfully applied in chemistry, giving illustrations from the various types of compounds of carbon and nitrogen. For this purpose he has made certain assumptions, drawn certain deductions from them, and has even ventured on a prediction. If that prediction is not fulfilled, or if those assumptions are found to be untenable on other grounds, they will have to be abandoned; and with them the particular way, here suggested, of conceiving the screw-spiral structure must also go. But some other way must be found, or some cause of optical activity other than screw-spiral arrangement must be postulated; because we can hardly regard enantiomorphism as the cause of optical activity, in the face of the enantiomorphous but optically inactive crystals of barium nitrate.

SOME ASPECTS OF GEOLOGIC TIME

By H. S. SHELTON, B.Sc. LOND.

PART I.—GEOLOGIC PROCESSES AND GEOLOGIC TIME

IT is a fact of common knowledge that the opinion of men of science on the much-vexed question of geologic time is in a state of flux. Recent criticism and discovery have completely shattered the theories of Lord Kelvin. The collateral methods of Prof. Joly (on sea salt) and of Prof. Sollas (on the thickness of sedimentaries) have been subjected to trenchant criticism.[1] A new method has arisen in the estimates of the amount of helium accumulated in radioactive deposits.[2] A few words of introduction are, therefore, desirable, to set forward my own point of view. I would, therefore, say that, in my opinion, no single one of the methods, which, until a few years ago, were regarded by men of science as valid, and, within reasonable limits, final, is of any value whatever.[3] My own opinion is that geologic time is vastly greater than the geologist, since the days of Lord Kelvin, has thought probable. But the opinion does not greatly matter for the purposes of this essay. Here we are suggesting various methods of attacking our problem. If the suggested methods, or other new methods, confirm the conclusion of the present-day geologist, the labour will not be wasted. If, after careful study, they establish an entirely different order of time, their necessity will be all the more certain. For, even if present-day views and methods are mistaken, it does not follow that the problem is insoluble.

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[1] See my article in the *Contemporary Review*, February 1911.

[2] See particularly papers by Prof. the Hon. R. J. Strutt in the *Proceedings of the Royal Society*.

[3] I have dealt with them individually in the following papers, in addition to the one already mentioned: "On the Tidal Retardation of the Earth" (*New Quarterly*, November 1909); "The Age of the Earth and the Saltness of the Sea" (*Journal of Geology*, February—March 1910); "Secular Cooling as an Illustration of the Methods of Applied Mathematics" (*Journal of Philosophy*, September 1, 1910); "The Age of the Sun's Heat" (*Contemporary*, June 1913).

The structure of the crust of the earth contains within itself so many signs of the manner of its formation, that it is surely possible to disentangle valid methods, if only the geologist will diligently search them out.   If he will cease from following false clues, it is not impossible that he may, even now, be on the way to clearer and more certain knowledge.

Towards the accomplishment of this end, it is, as yet, impossible for any single worker to do more than to make a few tentative suggestions. As the question is seriously attacked, and as it is made the subject of careful and detailed research, new paths will open, and new methods will be discovered. Meanwhile, it will be of interest to note a number of possibilities, the full bearing of which the geologist of to-day is liable to overlook.

Let us first consider the use that can be made of the data we are supposed to possess concerning the rate of erosion. The discharge of sediment at the mouths of a number of rivers has been measured, and, by these measurements, geologists have attempted to estimate the rate at which the continents are being carried to the ocean. But difficulties arise when we attempt to obtain from our data a general average rate of denudation, especially such as it is possible to apply to previous geologic epochs. The rate of erosion must vary enormously. In a rainless district, such as the cañons of the Colorado, it is very slow. In a country of torrential rainfall, such as the Ganges basin, it is very great. The question, therefore, must be faced which conditions can be regarded as typical. The rivers mentioned by Geikie, concerning which reliable measurements exist, are the Mississippi, the Ganges, the Hoang-Ho, the Rhone, the Danube, and the Po. To these Chamberlin adds the Potomac, the Rio Grande, the Uraguay, the Nile, and the Irrawady.[1] The majority of the data measure the transport of alluvium from irrigated and cultivated soils. Small particles of alluvium are carried a short distance, and are either deposited elsewhere in the basin or in the region of the slowly forming delta. To interpret correctly what this transportation means requires careful thought and analysis. The discharge may represent the normal and average lowering of the level of the river basin. But there is another possibility which must not be

[1] See Geikie, *Geology*, p. 589 ; Chamberlin and Salisbury, *Geology*, etc., vol. i. p. 101.

overlooked. Alluvial land, irrigated and manured, is, clearly and obviously, subject to rapid denudation. The ground is porous. The roots of trees and crops are continually loosening new rock. The ground is soaked by the percolation of water. Passage is made in winter for the water to enter the rock below, to freeze and break it up. The humus acids formed by the rotting of manure are not without their effect. It is, indeed, not unlikely that much of the observed erosion is due to human-kind. We must not forget the influence of man as a geologic agent.

The data at our disposal are too scattered for us to form definite conclusions, but it is an interesting fact that all rivers with a high, or an abnormal, discharge of sediment are situated in densely populated and highly cultivated districts. Those with a calculated rate of erosion greater than a foot in 2,000 years are the Ganges, the Irrawady, the Hoang-Ho, the Po, the Rhone, the basins of all of which have been highly cultivated for generations. Those with a moderate rate of erosion (more than a foot in 7,000 years) are the Potomac, the Mississippi, the Danube, all of which drain districts of considerable cultivation. The rivers with an abnormally low rate of discharge of sediment are the Uraguay, the Rio Grande, the Nile. The Nile is ex-ceptional owing to the absence of rainfall in the lower part of its basin and to the fact that a proportion of the sediment from the upper reaches is deposited in the rainless district during the annual river overflow. The Uraguay and the Rio Grande are situated in districts of comparatively sparse cultivation. These facts are striking. The conclusion may or may not be that here suggested, namely that the discharge of sediment does not represent true geologic erosion, but merely the effect of cultivation, but, at least, the coincidence shows that the problem of the rate of erosion under diverse conditions requires further investigation.

Wider data are needed to avoid possible sources of error. We should endeavour to find river basins under conditions similar to those which existed before the earth was trodden by the foot of man. If we could obtain, for example, reliable experimental data for the Amazon (a tropical and sparsely populated district), the Colorado, and Murray (districts of scanty rainfall), the Mackenzie (a district under glacial condi-tions), and one or two miscellaneous results (such as the

Zambesi) for other districts where the population was sparse, we should throw some light on our problem. Measurements for the upper reaches of rivers would be helpful. Also we must note that, though the rate of discharge of sediment is, as yet, our best guide to the rate of erosion, it is not impossible that others may be discovered. For the present, however, we must clearly realise that such information as we do possess is scanty and uncertain. There is one other point of importance. It is highly probable that, in all normal cases, there must be some relation between true geologic erosion and the soluble content of the river. The relation would not be strictly proportionate because of the solubility of carbonate of lime, but there would, as a rule, be some relation. Now it is a suggestive fact that so many rivers which pass through districts of sparse population have a comparatively small soluble content. If we mark out those like the Colorado and the Kansas, draining " bad lands," impregnated with large quantities of saline deposits, the soluble content is unusually small. The Amazon, for example, has a soluble content of less than 50 parts per million. The rivers of Northern Sweden are remarkably pure. Other instances can be given. Though rough and inaccurate, the suggestion is one on which I lay some stress. It has been shown that the process of weathering is, largely, a chemical change, in which a portion of the substance is carried away in solution, and, by that change, the remainder is loosened and comes away in the form of sediment. Erosion and solvent denudation must always be interrelated.

Other circumstances that point to the conclusion that the rate of erosion has probably been overestimated are the long periods, in all climates (except the neighbourhood of large manufacturing towns), during which inscriptions will remain legible. Some, not deeply cut, will last for many thousands of years. Once again, it is well known that we can still see, on the rocks in mountainous regions, striæ which date back to the last glacial epoch. If this occurred (say) 30,000 years ago, several feet of strata must, according to current theories, have been removed in the meantime. How anything of the kind could happen and leave the striæ as we now find them requires some explanation. It thus seems probable that the rapidity of land erosion may be smaller than our data would tend to show. This suggestion I put forward for what it is

worth. In any case, we require more experiments, and more carefully chosen experiments, before we can lay any stress on the results that have been obtained.

The principal point it is necessary to emphasise is that the rate of erosion, when we have got it, is a very useful guide to the rapidity of geologic process. Unfortunately it is the case that the enormous variations that are known to exist are not yet correlated with the configuration of the country or with any other known cause. Thus we cannot, with any confidence, apply our averages to particular cases. But, taking our present information for what it is worth, it is surprising that geologists do not apply it directly, instead of indirectly. The formation of sedimentary rock is a variable and uncertain process. It is liable, not only to extreme variations, but to actual reversal, without always leaving obvious indications. The rate of erosion is, comparatively, a constant quantity. Let the geologists, therefore, endeavour to ascertain the amount of erosion which has occurred at particular places and in particular geologic epochs. Instead of measuring deposition, let us measure erosion. We shall not then be encumbered by insoluble conundrums concerning the ratios of the areas of denudation and deposition.

Some facts are now available which bear directly on this particular problem. One very interesting research dates back to 1845. In the course of a thorough survey of a district in South Wales, the late Sir Andrew Ramsay discovered evidence of extensive denudation. His arguments are somewhat difficult to follow, and the conclusions concerning erosion are not clearly classified and tabulated, but a chance example will show how extreme erosion has been. It is stated that unconformable beds of New Red marl overlie strata which show a denudation of at least 5,000 feet between that time and the laying down of the Carboniferous limestone. It is stated as probable that some thousands of feet of coal measures may also have been eroded. This has taken place in only a part of two adjacent geologic epochs. This is, unfortunately, local, as distinguished from general or average erosion, but if we allow more than double the very highest estimate of general erosion, and assume that it took place at the rate of a foot in a thousand years, we have a minimum of 5,000,000 years for less than a single recognised geologic epoch.

Other evidence of long-continued erosion is found in the existence of " faults." In times of terrestrial upheaval, the crust of the earth has been twisted in all directions. Strata, laid down horizontally in the bed of the ocean, are upheaved into gigantic folds. Locally, the series will break. Younger strata, in the course of time, will be thrust upwards over older formations, and the consequent " faults " often imply a vertical displacement of many thousands of feet. Where, as is usually the case, the fault has been subject to subsequent erosion, so that there is no trace of it in the conformation of the country, and its presence is only indicated by the juxtaposition of strata of different ages, we have definite evidence of prolonged denudation. The depth of the fault is shown by comparing the structure of the strata on opposite sides, and we are able to infer that the total erosion has been much greater than the thickness of the fault. The ground on the lower side must also have been eroded, and the depth of the fault merely shows the excess of the erosion of the upper over the lower levels.

One striking example we owe to the researches of Prof. Judd. He has shown that, at Movern in Scotland, since the Miocene epoch, a fault of no less than 2,000 feet has been formed, and the upper side has been denuded so that Miocene basalts lie against Silurian gneiss. Assuming the erosion on the upper side of the fault to be twice as rapid as on the lower side, 4,000 feet will have been removed. At Prof. Sollas' rate of denudation, this would take more than 10,000,000 years. Allowing every possible weight to the advocates of a minimum of geologic time, we could indicate a minimum of 5,000,000 years for Pleistocene, Pliocene, and a small fraction of the Miocene.

Many other instances have been brought forward by the late James Croll. Near Dunbar, there is a fault of no less than 15,000 feet, eroded between the Silurian and the Carboniferous. In the Appalachians, a region has been eroded to the extent of no less than 35,000 feet. Nearly 10,000 feet of strata have been removed between the Millstone Grit and the Permian.

Present knowledge, as yet, does not allow us, from such data as these, to make definite numerical conclusions, but here is a method of research which should be developed by geologists. If they can first find the rate of erosion under a great variety of conditions, and then discover the extent of erosion and the conditions under which it took place in particular instances, during

this or that geologic epoch, the addition of the various results should give some clue to geologic time.

Further information could be obtained if we possessed fuller information concerning the extent of particular local formations. The structure of coal beds will illustrate my meaning very well. If and when it is possible to map out the extent and structure of particular beds, and of the intervening strata, it might be possible to put together a connected history of that particular tract of land. For this we require detailed information. We require to know where and how a particular bed commences, its extent, its manner of grading into other strata, and many other details. We require to be able to make a model of the ground so as to show the configuration of its strata in as much detail as possible. We want a geologic map of some special tract of country which will show, not only epochs, but small individual formations. The detailed sections of various parts of a district require comparison and co-ordination. Then its history can be written. Then we can compare the processes of the past with those now going on, and form some idea of how, and in what space of time, they occurred. The estimate of time would be rough, but, at least, so far as it went, it would be by the reconstruction of actual events.

The idea will be made clearer if I utilise an example which I have mentioned before.[1] I refer to coal beds. The view has now received general acceptance that a considerable proportion of these have been formed *in situ*. There are, no doubt, such things as drift beds, but many of the coal beds, especially the seams that are large and workable, undoubtedly represent the actual sites of the old Carboniferous swamps which flourished so largely and were so widespread. Some of these seams are of enormous extent. There is, for example, the " Pittsburg," in Pennsylvania, at least 12,000 miles in area. Why should it not be possible to map out a coal-field in detail, to show roughly where each particular seam begins and ends, where each divides, to indicate the extent of each intervening layer of sandstone, shale, or limestone, if and when the latter occurs ?

Each successive coal bed indicates an advance and a recession of the sea. If and when this has taken place over large areas, events have occurred to which a minimum of time can be assessed, or, at any rate, some idea of the necessary time can be

[1] See article in *Contemporary*, Feb. 1911.

put forward. We have several historical instances of advance and recession of the sea. Winchelsea was a port in Norman times. Hudson Bay is disappearing at a measurable rate. Estimates of geologic periods, on lines like this, are, at any rate, based on events that actually occurred. They may vary, but they can only do so within reasonable limits. When we have no idea, or a false idea, and can only be guided by the maximum thickness of sediment, estimates may vary to any degree.

I mention coal beds for two reasons. In the first place they represent the most important of the few strata, which are, for commercial purposes, actually bored. Borings for purely scientific investigation are far too costly to be undertaken on a large scale. Consequently, in the mapping of most strata, the geologist must confine himself to the outcrops. Such a method does quite well for the tracing of the strata of the larger epochs, but it is very doubtful how far it would suffice for mapping out small beds. The borings in the coal fields are already made, and a suggestion such as this will not present insuperable diffi- culties. The second reason is to put a doubtful or disputed point, in one specific instance, beyond the range of controversy. If we have two successive coal beds of known large area, with a layer of shale in between, there can be no possible doubt, grant- ing that the beds were formed *in situ*, of an advance and a recession of the sea. That such events have continually taken place in the ordinary strata, I thoroughly believe. That even the maximum thicknesses were formed intermittently with con- siderable intervals of emergence from the sea masking the great epochal submergence, is a fixed opinion of my own. But proof, as a general rule, is not easy. Fortunately, the structure and arrangement of coal beds make the speculation, for certain times and conditions, a certainty.

As the science of geology progresses, and as more and more detailed facts are discovered, new methods will come to light, and such suggestions as these will be trite and obvious. There is, in the study of the rocks, a wealth of material which requires only careful and intelligent study to solve many problems now obscure. But such careful study will not be the work of a day.

Until the science of geology attains greater clearness and exactness, some other lines of investigation may assist in giving a clue to the order of the result. One of these is found in the

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chemical structure of the Earth's crust. Of the geochemical methods, the best so far discovered is probably that based on calculations concerning the amount of limestone in the rocks of the Earth. As is well known, limestone rock is not, and cannot be, a part of the Earth's original crust. It has been slowly dissolved out of the primitive and the newer igneous rocks, carried to the sea in solution, and there used by the various marine organisms for the formation of their shells. These minute shells have either formed comparatively rapid local concretions of coral reef, or have gathered, at a rate inconceivably slow, in the abysses of the ocean. Geologic time must have been great enough to admit of the removal of all this substance from its place of origin and its deposition in the conditions where we now find it.

The geologist whose name is most intimately associated with the question of the evolution of carbonate of lime is the late Mr. Mellard Reade.[1] Mr. Reade did not attempt to fix any actual figures. He did not think the subject was ripe for such exactitude; but he maintained strongly that these data proved that the Earth had existed for a much longer period than the mathematical physicist of his time had thought to be possible. The results of the *Challenger* expedition have enabled us, within a reasonable degree of accuracy, to map out the character of the ocean floor. In the neighbourhood of land, the sediments are, in the main, composed of detritus from the rivers. In the greatest depths, the carbonate redissolves and the floor is composed of "red clay." Between these two limits, the main covering of the ocean floor is carbonate of lime.

Mr. Mellard Reade made deductions from the calculated amount of carbonate of lime, and the time that it would take for this to be evolved from igneous rock. From that amount, he inferred that the process must have been going on for at least 600,000,000 years. This calculation I believe to be substantially sound, though the details will require revision in the light of more recent knowledge. It is, I believe, possible to assert a probable minimum of the order of 500,000,000 of years. The number is a minimum for two reasons. In the first place, igneous action, whether at the surface or deep-seated, is con-

[1] See various papers in the *Geological Magazine*, also papers read to the Geological Society. A most important pamphlet is republished under the title of *Chemical Denudation*.

tinually re-absorbing carbonates, with the probable evolution of volcanic carbon dioxide.[1]   In the second place, Mr. Reade made a very modest estimate of the limestone buried under the ocean.

Another aspect of the same subject is found in the masses of marine limestone found in the sedimentaries of particular geologic epochs. According to the data of Sir John Murray, there is brought down to the sea each year roughly 2,000 million tons of calcium carbonate. This, if evenly deposited over the ocean floor (say 150 millions of square miles), would raise its level to the extent of only a foot in 90,000 years. Sir John Murray has calculated that carbonate deposition is actually taking place over only a third of that area. It therefore follows that, at the present time, under the sea-floor, vast areas of limestone are being laid down at the rate of about a foot in 30,000 years. We must note that we have here merely the order of the result. The very deepest sediments are formed more slowly, because, in the vaster abysses, the pressure of the water causes the re-solution of the more delicate of the shells of the foraminiferæ which make the bulk of the oceanic lime deposits. On the other hand, local deposits, and particularly coral, are often formed much more rapidly. We must notice, however, that excess in particular places implies that the rate of formation in the ordinary deep sea deposits must be slower by a corresponding amount.

There remains the question whether the vast masses of mountain limestone found in the strata of so many different ages are marine in this sense of the word. Let us, as an example, take the Cretaceous and the Carboniferous deposits. There has been some dispute as to whether these are oceanic, or were formed in shallow water. From the point of view of rapidity of formation, however, it does not greatly matter. What is important for our purpose is whether or no strictly contemporaneous limestone deposits are widespread. Let us, therefore, consider the Carboniferous in greater detail. Early Carboniferous limestone, attaining sometimes to several thousands of feet in thickness, underlies newer rock in nearly all the area of Great Britain. It outcrops in several places, and constitutes the greater part of the bulk of the Mendips. Sir

[1] It has been stated that Mr. Reade overestimated the *proportion* of limestone. If so, his estimate is liable to a reduction on that account.

Archibald Geikie states that a continuous formation can be traced over 750 English miles from the Western headlands of Ireland into the heart of Europe. How far it extends, or once extended, under what is now the Atlantic, and its extreme limits north and south do not appear to have been determined. Contemporaneous limestone (though interstratified with coal beds) is stated to be found in Scotland, Silesia, Central and Southern Europe, Spain, and the Urals. Limestone of the same era is found in China, in the Central Himalayas, in Morocco, Algeria, and other parts of Africa, and also in Australia. In America, early Carboniferous (Mississippian) limestone (in some places mixed with sedimentary) underlies a large portion of the United States. It is 5,000 feet thick in the Canadian Rockies, and is extensively developed in Alaska. The known area of the formation must be reckoned in millions of square miles. If we add to this an estimate for countries as yet geologically un-explored, for that which is now under the ocean, for that which has been eroded in the vast period which has elapsed since early Carboniferous times, there can be no doubt that it was deposited under oceanic conditions. For the essential point is the area and thickness of the formation. If we can reckon the area of contemporaneous limestone at many millions of square miles, the current controversy whether it was deposited under deep or under shallow water conditions becomes of small importance for the purposes of our argument. Under no circumstances is it possible for the ocean, which contains an infinitesimal pro-portion of carbonate of lime, to deposit, for any prolonged period, more than is brought down by the rivers to the sea. Let us, therefore, assume ordinary marine conditions, and assess the probable average thickness of early Carboniferous limestone at the very low estimate of 1,000 feet, and let us allow as the probable rapidity of formation three times the present average, a foot in ten thousand years, we thereby obtain a minimum of ten million years for only a portion of a recognised geologic epoch. Such figures as it is possible to give are, of course, very crude guess-work, and no stress is laid on them, but they will serve to point out a useful line of research.

The only important query which is likely to be raised, and which, indeed, has been raised, is whether, in past times, the proportion of carbon dioxide in the atmosphere might not have been excessive, and so the amount of carbonate carried to the

sea might have been larger than it now is. This suggestion, at first sight, seems probable. The erosion of the chalk hills and their conveyance to the sea in solution by the rivers is certainly occasioned mainly by the carbon dioxide which falls to the ground in the rain. The same cause is an important factor in all erosion. For that reason the factor must be briefly considered. Here it is hardly possible to dogmatise either way. Nothing is easier than to make rash and unfounded theories. It is certainly difficult to imagine causes which would enormously increase the carbon dioxide in the air for a particular geologic period. Where it would come from, and why it should vanish, are, at least, problems which require careful consideration. It will suffice, however, to make two comments.

In the first place we must note that we have, in the sea, an enormous reservoir which acts as a giant fly-wheel on the composition of the atmosphere. Those who accept this theory must account, not only for the production of the carbon dioxide to fill the atmosphere, but also for that enormously greater amount which would dissolve in the ocean. The amount of carbon dioxide in the atmosphere and in the ocean is in approximate equilibrium, and the amount in the atmosphere is only a small fraction of that contained in the sea. In the next place, we must note that the suggestion only affects the time necessary to evolve the limestone from igneous rock in so far as it affects nearly all the recognised methods of estimating geologic time. It is, of course, true that a more acid rain would more rapidly dissolve the lime from the igneous rock, and so increase the total mass of terrestrial limestone, but the same factor would hasten all the processes of erosion and deposition. Rock would more quickly be crumbled, and carried away in sediment by the rain. The dissolved sodium would more quickly reach the sea. Thus, if this hypothesis seek to harmonise any discrepancy that may be supposed to exist between the evidence of limestone and that supplied by other methods of denudation, the suggestion will utterly fail. It cannot too strongly be emphasised, in all geologic speculation, that it is necessary to try to disentangle the full bearing of many correlated factors.

It must be admitted, however, that, for a special period, which would not greatly affect general averages, the factor might not be without its effect on the rate of formation of particular deposits, such as those we have noted at some length

in the Carboniferous. Carbon dioxide has a special solvent effect on limestone, over and above all other kinds of rock, and so far as this was exposed on hill-tops and in cliffs facing the sea, the solvent effect might be much greater. Some small allowance would probably be required for greater erosion in underground caverns. But, to all this, there is a very definite limit. The erosion could not, except under special conditions, affect the limestone so as to take it below the level of the surrounding country. If this happened, lakes would form, and the remaining limestone would be covered with a protecting layer of shale. The dependence of special erosion on general erosion is shown by the fact that salt beds are so extensive and so numerous. No possible conditions could make the solubility of limestone approach that of salt in water. Yet salt beds are very slowly removed to the sea, and it seldom, if ever, happens that we can detect their presence by the greater salt content of river water. With these remarks, the objection must be left. Like so much other geological controversy, it appears to have been made because of the supposed necessity to "hurry up" geologic phenomena, so as to make them fit the dogmas of the physicist. But the assumption of comparative uniformity is the soundest that can be made.

Without, however, dogmatising concerning details such as these, we must note how important, in its relation to geologic time, is the question of the evolution of carbonate of lime, both in general and in special geological epochs. It is a consideration on which considerable stress should be laid.

Very brief mention must suffice for the one other method that is now attracting attention. I refer to the estimation of the amount of helium and of lead in minerals containing appreciable quantities of uranium. The elements uranium and thorium, as the modern chemist has abundantly shown, are slowly disintegrating and giving rise to other elemental forms. Assuming that the helium found in these minerals is obtained from the radioactive elements contained in them, an estimate of the time that has elapsed since they were formed can be made. The work of Mr. R. J. Strutt[1] has placed beyond doubt that, on that assumption, the time that has elapsed since geologic epochs, not the most ancient, must be measured in hundreds of millions of years. But accurate and entirely self-consistent results have not

[1] See various papers in the *Proceedings of the Royal Society*.

yet been obtained. On a very few assumptions, the actual measured results must be regarded as minima. But there is much research yet to be accomplished before we can be quite sure what value to place upon them. So far as they go, however, they support the main contention of this paper. The radioactive method must be accepted as another valuable line of research.[1]

PART II.—ORGANIC EVOLUTION AND GEOLOGIC TIME

A. BIOLOGIC THEORY AND GEOLOGIC TIME

In the whole history of human thought, it would be difficult to find two topics so intimately connected as evolution and geologic time. In the days of catastrophic cosmogony, no theory of evolution was possible. The discoveries of the early geologist paved the way for the superstructure of the evolutionist. When we discovered that the earth dated back to a remote antiquity, and that, during this lapse of time, the forms of life were continually changing, the naturalist was then able to investigate the causes of the change.

Thus the evolutionary ideas of Darwin were founded on the uniformitarian geology of Hutton and Lyell, which postulated an indefinite lapse of time, a postulate of which Darwinian theory took full advantage. A number of philosophers, Lamarck and Herbert Spencer in particular, had anticipated Darwin in the advocacy of evolution, but had differed in their opinion of its causes. By a strange coincidence, the theory of Darwin demanded a vaster extent of time than had the ideas of any previous worker. By laying such great stress on natural selection, by postulating that, in the main, the changes in the forms of animal and vegetable life were due to the selection of minute and imperceptible variations which happened to be of advantage in the struggle for existence, he required the assumption that the time must be of the order that commended itself to the geologists of his day. So much was this the case that, when

[1] In view of the possibility that too much stress may be laid on this, as distinguished from other lines of research, I think it well to say that, in my opinion, though detailed criticism is outside the scope of this article, attempts to assess exact times from consideration of bad ratios, are, to say the least, premature. There are so many causes of uncertainty. The most that we can now infer is a moderate minimum of time, a result that is given equally well by other data if properly handled.

the late Lord Kelvin dogmatically asserted that geologic time must be compressed within 100 millions of years, Darwin was seriously perturbed, not so much on account of the truth of the crucial fact of evolution, as of his own particular theory of natural selection. The cause for alarm has now been removed, but it still remains true that the subjects of geologic time and of methods of evolution are closely interrelated.

If we consider the interrelation from the biological standpoint, and endeavour to ascertain what light can be thrown on our subject with the aid of the bare facts of that science, we discover that very little information is available. We soon find ourselves arguing in a vicious circle. We know (for example) that man has developed from a pithecanthropoid form since the Pliocene, and that the horse has evolved from a beast with five small hoofs on each spray foot since the early Eocene. But if we desire to state the time in figures, we can only say that the Pleistocene is the period that has been required to develop man, and that man has developed during the Pleistocene. The biologist has no independent standard of time. Vague as are the data of the geologist, those of the biologist are still more uncertain.

It is, of course, possible to utilise the fact that no considerable natural change has been observed, during the historical period, in any organic form, and from this fact to posit a minor limit. Here, however, the Mendelian theorist, who has been so prominent of late years, will assert that evolution proceeds by jerks, and that the observed forms of life are in the resting phase. Improbable as such speculations may seem, there are no plain and obvious facts by which they can be refuted, so, here again, the biologist is referred to geological data. As in the time of Kelvin and Huxley, so to-day, it still remains for those who deal in physical and geological data to find the measure of time to which the biologist must fit his theories. There is so much theory in modern biology.

A number of biologists, of whom Prof. Poulton is the most prominent, admit this statement, so far as it deals with known fossiliferous rocks, but express the opinion that biologists can confidently assert that these represent but the last phase of an evolution which represents a vaster vista of time, an evolution of which all record has been lost.[1] As Prof. Poulton has shown, all the known phyla of the animal kingdom are found in the

[1] *Essays on Evolution*, pp. 1-45.

early Paleozoic deposits, and, of these, a considerable number of genera and orders are of a remote antiquity. Thus, four out of nine orders of insects have been found in the Carboniferous, crustacea in the Cambrian and pre-Cambrian, arachnida in the Silurian. From these facts he infers that pre-Cambrian evolution must have occupied a time vastly greater than that of which we have a record.

Though I am of opinion that this line of argument contains a great amount of truth, I am bound to demur that all that can definitely be asserted is an antecedent probability. If we assume, as appears to be the case, that these invertebrate forms, at the commencement of the period of the known fossiliferous strata, had attained to correspondence with conditions that have remained approximately constant during geologic time, we have insufficient data on which to make definite assertions concerning the time that preceded it. Let us put the matter more concretely. It is very probable that all vertebrate life has developed from a single type since the lower Cambrian. No phylum approaches the vertebrates in the complexity of its ramifications. What reason have we to assert that, when in process of active evolution, each phylum found in the lower Cambrian could not have been formed in an equal time? And what reason have we to assert that all these other phyla were not developed contemporaneously? If we give to the argument its utmost value, we are unable to assert that pre-Cambrian time has been greater than post-Cambrian. The assertion that it is of equivalent length, which is all the argument is worth, will help us very little. Such an assertion is highly probable on other grounds.[1] A maximum thickness of more than 100,000 feet of strata can definitely be assigned to pre-Cambrian times, and the primitive Archæan undoubtedly contains a large amount of metamorphosed sediment. Such a conclusion is all we can obtain from this broad aspect of biologic fact. Whatever time may be proved to have been required to form Cambrian and post-Cambrian strata, to it must, probably, be added at least an equal time for pre-Cambrian strata. Whatever we may think concerning probabilities, it would be rash dogmatism to assert more.

The futility of dogmatism is also shown by the scarcity of

[1] Recent researches are showing the probability that pre-Cambrian time is, probably, considerably greater than post-Cambrian. See address by Prof. A. P. Coleman, *British Association Report*, Sheffield, 1910.

fossil forms in the pre-Cambrian. Although a very considerable bulk of pre-Cambrian rock has been examined, the remains of life are few and far between. In the Torridon sandstone, laid down under the calm and peaceful conditions so graphically described by Sir Archibald Geikie,[1] no fossils have been found. Crustacea have been found in the Proterozoic. There is a limestone deposit, which may or may not be organic, at the base of the Huronian, but the comparative scarcity of life is a striking and interesting fact. There is no evidence of metamorphism, and there is no apparent reason why fossils should not have been found. Though reasoning from the absence of such remains is a very risky proceeding, the contrast between this scarcity and the relative abundance in later strata at any rate suggests the probability that the known forms of life were then local and in process of establishment as world-wide types. If this were so, it is easy to point out that the relatively rapid change of conditions connoted by our hypothesis is a strong presumption in favour of a rapid process of evolution.

There are one or two other speculations to account for this interesting fact. One is that the early seas were acid, and that the organisms were therefore unable to form protective coatings by the secretion of carbonate of lime. The very early date of some limestone deposits will require explanation on this hypothesis. If the speculation were accurate, lime deposition could only take place locally in lakes when the process of deposition had gone far enough to neutralise the prevailing acid, or, when such lakes had not been part of the sea, in places where the influx of the rivers would not be neutralised by the acid of the sea. The speculation is somewhat wild, but some light would be thrown on it if and when we have discovered whether or no the earliest limestone deposits are invariably lacustrine.

Whether this or some other reason be the explanation, it is interesting to note that a very considerable proportion of such pre-Cambrian fossils as have been discovered are chitinous rather than calcareous; and whether this fact be due to deficiency in carbonate of lime, or whether it be due to the fact that the species at that time had not acquired what has been described as the lime habit, the facts point to the probability of a comparatively rapid pre-Cambrian evolution. Whether or no the reasons that have been given are sufficient, it will be generally

[1] See address to British Association, 1899.

admitted that the formation of exterior protective lime coatings is likely to render further developments both difficult and unnecessary. The one notable instance of the higher development of such invertebrate forms, the cephalopods, has only taken place as and when the protective coating has obsolesced. Thus we have further evidence in favour of our conclusion that this aspect of the relation between organic evolution and geologic time is not likely to give us tangible and certain conclusions. The probability we have already noted, that pre-Cambrian time is at least of the same order as post-Cambrian, is, however, a valuable result to glean from a first cursory glance at main principles.

B. Geologic Time and Biologic Theory

Our results, so far, are interesting but scanty. The biologist can give us much useful information, but his conclusions must not be pressed too far. It will now be interesting to consider the converse, i.e. the effect of our knowledge of geologic time on biologic theory. Much has been written of late years concerning theories of evolution, and recent speculations on geologic time have been used as a controversial weapon. The arguments of a class of biologist runs somewhat on the following lines: Natural selection, as postulated by Darwin, requires a great vista of time in which to work. Use-inheritance, which was accepted, not only by the early evolutionists and by Herbert Spencer, but by Darwin himself, has been thought to have been disproved by Weismann and his followers. Therefore the theorist, to escape from the dilemma, has made the inference that evolution has proceeded discontinuously by a succession of "sports" which have happened to be of advantage in the struggle for existence. The inference receives some support from the discoveries of Mendel, which have recently been brought into such prominence by Prof. Bateson and others.

We cannot here discuss the evidence for and against use-inheritance. In case the reader should suspect bias on grounds not stated here, it may be as well to state that I should classify myself as neo-Lamarckian, and that I do not attach great importance to Mendel's discoveries—at any rate, in their relation to the problem now before us. While there can be no doubt concerning Mendel's facts, and the interesting light they throw on some problems of heredity, the evolutionary and theoretical

superstructure erected on them by some theorists appears to me to be unsound. Here, however, it is only possible to note the inference that has been made from modern ideas of geologic time. That inference falls entirely to the ground. There is now no recognised maximum limit to geologic time. There are no valid arguments which enable us to limit the time for organic evolution to less than a thousand million of years. And that period would suffice for any known theory of evolution. Consequently, whatever may be said for or against the neo-Mendelian theory of sports, this particular argument is invalid. It is desirable also to state that the argument from geologic time is not available for the neo-Lamarckian as against the neo-Darwinian. I am not aware that any recognised neo-Lamarckian controversialist has made use of it, but if he has, it is invalid. Our knowledge of geologic time is equally consistent with any and every theory of evolution. The conclusion of this aspect of our subject is purely negative. Biologists and others who have made use of the geologic argument must abandon it, and must reconsider their theories, in view of the fact that recent and current speculations on geologic time have broken down.

C. A Suggestion concerning Physiological Infertility

Although the first crude and obvious arguments that arise from attempts to correlate the sciences of geology and biology are of little value, it does not therefore follow that the use of biological data is impossible. But the data must be used more fully and more carefully than has yet been done. Many ways of combining our data are, no doubt, theoretically possible. For our present purpose, however, it will suffice if we call attention to one aspect of evolution—on which Darwin, in his *Origin of Species*, and Spencer, in the *Principles of Biology*, laid considerable stress, yet which has been overlooked in recent biological speculation. We have already noted the problem of the time required for the making of new species. As we have already seen, nothing of the kind has been observed. Nor is this statement an example of reasoning in a circle. It might be contended that changes which we have produced by breeding and cultivation are not called species changes, for the simple reason that we have observed them. With regard to some forms of life there is substance in the argument. Darwin, in his famous investigations on cirripedes, found great difficulty in deciding

what exactly were species and what were merely varieties. Other naturalists have been involved in the same difficulty. But with regard to the higher forms of animal life we have an independent criterion. It is generally recognised that the mutual infertility of nearly allied animals is a test of species difference. In the rare exceptional cases, such as the horse and the donkey, when hybrids can be formed, the hybrids are infertile.

We shall, therefore, do well to leave the morphological side and to pay more attention to the aspect of physiological fertility. It is hopeless to attempt to decide what degree of morphological change does or does not constitute species difference. The difference in shape between the horse and the donkey is comparatively small, yet a fertile cross cannot be obtained. On the other hand, notwithstanding the enormous differences between the varieties of domestic dogs, differences of size, shape, proportion, colour, character of coat, these varieties are mutually fertile.[1] The variegated types of domestic pigeons, notwithstanding enormous differences, are not only mutually fertile, but, if left to themselves, revert to the ordinary rock pigeon from which they are descended. Yet the differences, were they found in fossil forms, would probably be classed as greater than species difference.

Such facts as these throw some light on the course of organic evolution. Physiological infertility is evidently not correlated with accidental differences in shape, colour, or form, but connotes an essential, deep-seated organic change. It seems probable, therefore, that this may not be obtainable by artificial breeding, but that it may be a natural process, which, for its accomplishment, requires a prolonged time. It has certainly not been found among the multitudinous races of human-kind. If this theory were actually proved (as yet it is only a speculation), it might give us a minor limit for the time required for the production of species.

It is interesting to note that the discoveries of Mendel can, without undue straining, be made to fit into the same hypothesis. It has not yet been proved that all inheritance can be described in Mendelian terms. Mendelism may account for inheritance in mixed races, such as the Caucasian and Negro half-breeds, but even this is doubtful. Certainly, in ordinary human inheritance,

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[1] For obvious reasons, it would hardly be possible to obtain a first cross when there was more than a certain difference in size, but this is not true physiological infertility.

we see all degrees of blending, and there seems no possibility of expressing it as a sorting out of minor characters.

Let us, therefore, look at the matter from another standpoint. Let us look at Mendelian inheritance, not as the normal form of inheritance, but as a modified form of mutual infertility. Mendelian inheritance is the characteristic of stocks that do not truly blend. The various varieties emerge from the process of inter-crossing practically unchanged. This clearly tends to fix the types of the crossing varieties. It accomplishes, in a different way, the same purpose as the mutual infertility of allied species. Does it not, therefore, seem a plausible suggestion that this is merely a step on the road towards species formation, that the practically complete blending of ordinary inheritance, the emergence of unaltered types from the process of Mendelian crossing, the partial infertility of the equidæ, the entire mutual infertility of other allied species, may be but parts of a continuous process, the formation of distinct physiological species?

This is, of course, merely a speculation, and will require considerable confirmation before it is possible to make use of it, but I put it forward as an illustration of the necessity of avoiding undue dogmatism concerning the possible methods of deter-mining geologic time. Because biological data have, as yet, thrown no light on this subject, we must not be too ready to assume that such may not be available in the future. It is therefore, of interest once more to raise the question: has a truly infertile physiological species ever been formed within the time of human observation, or has, indeed, any series of varieties been formed which will intercross in a definitely determinable Mendelian manner? Changes of form are produced quickly, whether by selective breeding or by change of conditions. But the problem of physiological species is still unsolved and it may be that a great lapse of time is required to form them.

D. Fossils as an Index of Geologic Time

Suggestions such as those referred to in the last section are problems for future research. For the present, pure biological methods, particular as well as general, have yet to be found. We shall, therefore, now glance at the more obvious line of advance found in the co-ordination and correlation of biologic and geologic data. In its broad outlines, the method has been

carried out since the dawn of geology. Geologists, in determining the age of strata, are almost entirely dependent on the biologist. But for the discovery of characteristic fossils, they would, in many cases, be without the slightest clue to the age of particular formations. And, by this method, it has been possible to divide geologic time, not only into the broad recognised epochs, but into a varying number of zones. This line of investigation appears to be open to further development.

A useful and striking example, which has recently been very ably popularised by Prof. Sollas,[1] is found in the famous Oppel zones of the Jurassic. No less than thirty-three distinct zones have been identified by observing the structure of fossil ammonites. Each species is found in a particular zone, and nowhere else. It has been proved that the sub-divisions are world wide. Everywhere, in Europe, India, America, Australia, they follow each other in the same succession. Types like this do not arise in a day. They are not distributed over the whole world in a short time. Previous types are not displaced all at once. In particular regions, species may be exterminated rapidly, but surely not all over the world. It will be noted that these ammonites are definite and distinctive types. The manner of their evolution does not appear to have been determined. The minute grades by means of which they must have been evolved from preceding creatures have not been found. Such have probably been formed locally, in some specialised and confined area, and, when the barriers have been removed, the species would gradually penetrate all over the world. We know little as yet of the rate of the evolution of life, but the suggestiveness of these facts in connection with our subject does not require to be pointed out. Such facts as these have a very cogent bearing on our subject. In the first place, the very existence of this continual succession of organic forms is itself striking. Prof. Sollas, who is committed to an unusually small estimate of geologic time, thinks that these forms have succeeded each other with unusual rapidity. His suggestion cannot be rejected on *a priori* grounds. So small is our knowledge of the possible rapidity of organic evolution, that we are unable to say that species may not, as he surmises, have succeeded each other at intervals of 25,000 years.[1] The study of recent strata does not appear to have disclosed any similar case of rapid evolution, but the hypothesis cannot be

[1] *Age of the Earth*, pp. 273 *seq.*

called absolutely impossible. It does, however, show an ante-cedent probability in favour of a much vaster vista of time.

I think, however, if the data be examined more closely and are duly correlated, they might throw some light on our basal problem, and the methods by which our knowledge can be advanced are but a continuation of those which Prof. Sollas himself has so graphically described. Prof. Sollas is of opinion that the fossil ammonites were not, as a rule, deposited where we now find them by ocean currents, but that their occurrence in any strata, in any considerable quantity, implies that they actually lived in that region. One point, therefore, needs emphasis. The difficulties with regard to the origin and development of species are, by these discoveries, greatly magni-fied. All over the world, in a small zone of the Jurassic, roughly one thirty-third of the whole period, a species appears, lives, disappears. How was it evolved, and what are the stages in its evolution ? We must note the strong probability that the species was evolved since the end of the period indicated by the last zone, but how and where ? Where are the intermediate stages by which it was developed from pre-existing types ?

This aspect deserves special consideration. The sudden appearance and disappearance of world-wide species is striking, and gives rise to considerable speculation. The fact that such a succession of commonly found species is continually found without intermediate stages might, at first sight, tempt us to deny the hypothesis of evolution and to say that intermediate forms do not exist. Fortunately, however, it does sometimes happen, particularly in the fossil forameniferæ, which make up the main substance of the chalk cliffs, that the change of organic forms is so gradual that division into distinct species is difficult. We must assume that the missing intermediate forms existed. But where are they ? Here is an ocean species, as Prof. Sollas so pertinently remarks, like our contemporary spirula, the shell of which is one of the commonest objects on the seashore. It is found fairly plentifully in a particular zone of the Jurassic. Yet, apparently, it arises from nowhere, and disappears suddenly. Such a problem calls for investigation. The sudden disappearance may, perhaps, be due to the advance of some predatory enemy. But what about the appearance ? And would they suddenly disappear all over the globe ? Assuming the facts to be as stated, we have an admirable guide to help us to piece together

the changes in the earth structures of early times. The point I am specially concerned to urge is this : If, in any group of strata, one species suddenly vanishes, and another allied species suddenly takes its place, which is exactly what does appear to occur, there is *prima facie* evidence for a considerable gap in the succession of the rocks.

The remarkable succession of " Oppel's zones " gives rise to many interesting questions. The more detailed information we can get the better. We require, from the researches of specialist geologists, a clear answer to a series of questions such as the following :

(*a*) Is the species marking what we will call a zone identical at its base and at its summit ?

(*b*) Is the species identical at the base in India and at the base in Europe, at the base in India and the summit in Europe ? If not, what, so far as can be discovered, is the extent of the variation for time and space ?

(*c*) In each district containing a fairly complete series of Jurassic beds, what zones are present and what are absent ?

And so on.

Facts such as these are probably known. The zonal classification probably merely implies that certain dominant forms occur in a definite order. When such a classification is made the essential point occurs in locating a gap. The fact that certain zones are missing in certain groups of strata in certain districts has a clear and definite meaning. But the point of greatest interest is found in the gaps, and particularly in gaps that appear to be world-wide. Here we come somewhere near bedrock in our co-ordination of organic evolution and geologic time. If in certain strata we find a sudden disappearance of form (*a*), and a sudden replacement for it of form (*b*), and we find no strata in which form (*a*) and form (*b*) are found together, the natural inference is that a considerable interval of time has elapsed between the two depositions. If anywhere in strata roughly contemporaneous we can discover a filling of the evolutionary gap, either the two forms occurring together or the existence of forms intermediate between the two, the problem of the intermission is partially solved. If nowhere in any strata are intermediate forms to be found, and if, as appears to be the case, fossils (*a*) and (*b*) are plentiful in their respective zones, are never found together, and inter-

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mediate forms have yet to be discovered, the probable conclusion is that, in all districts of the world where seemingly from a cursory reading of the signs sedimentation may have proceeded continuously, there is a gap implying a large lapse of time. The conclusion that emerges is that between the deposition of the two sets of strata there have been considerable and world-wide changes in the configuration of land and sea. And if that be so, it does not seem absurd to suggest that there may, after all, be a very close relation between the amount of change in any dominant form and the time that has elapsed, respectively, in the formation of sediments and in the unknown era represented by the intervening gaps. The elucidation of the precise relation demands careful research of some particular period, and that the numerous facts known concerning graptolites and ammonites (to mention the groups principally used in zonal classification) should be correlated in a more intelligent manner.

The few suggestions contained in this essay are tentative and illustrative. They are but anticipations and indications of the manner in which the twin subjects of organic evolution and geologic time can be more intimately connected. To do more would be difficult in the present state of scientific knowledge and opinion. For fuller information the great necessity is careful, detailed, and independent research. It is necessary that the fundamental problem of geology should be deemed more worthy of time and attention than the minor questions which everywhere receive such detailed treatment and which result in so many carefully written and voluminous monographs. The subject is as yet hardly touched, and a clearer and more wonderful science of geology can be built up by those who apply to it true methods of scientific investigation.

What we are entitled to say on the evidence before us, biological, geological, and physical, is this: It would be absurd to attempt, on very insufficient data, to give an estimate of the probable lapse of geologic time. But there is, at the present day, no reason whatever why it should not be a thousand million of years or a time even greater. The hundred-million maximum of the old physicist and geologist is now exploded. To make any estimate in the place of that which has been shown to be invalid will only be possible after long and careful research. It is hoped that the criticisms and suggestions contained in this paper may do something to show on what lines such research should proceed.

THE SIGNIFICANCE OF THE PILTDOWN DISCOVERY

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IT is often the fate of technical words to serve their purpose and become obsolete. It was so with the word " Invertebrata." The earlier naturalists saw that there was a great group of animals clearly related to one another by the possession of a vertebral column. And it appeared to these earlier scholars that the lower organisms which lacked this characteristic might be regarded as akin to one another and thrown together into a single sub-kingdom called the "Invertebrata." But with the progress of zoology it came to be realised that the various divisions of the invertebrates differed from one another quite as much as, and in some cases more than, each differed from the Vertebrata; and hence the term "Invertebrata" was altogether discarded by zoologists.

The recent advances in prehistoric anthropology have been so remarkable that it seems probable that a like fate will overtake the word "Paleolithic." When in the year 1865 the late Lord Avebury (then Sir John Lubbock) proposed that the Stone Age should be divided into two periods, his suggestion very aptly expressed the facts of prehistory as they were then known, at least so far as Europe is concerned. The people of the later or Neolithic division lived in our own geological period; they were certainly our own direct ancestors; and they were semi-civilised, building huts, understanding agriculture, and possessing divers domestic animals. Behind these Neolithic peoples, separated from them in many places by a great interval of time —the so-called "hiatus"—and living under very different geographical circumstances, various entirely savage races were known to have existed. These flourished during the Pleistocene or Glacial Period, being consequently surrounded by extinct animals such as the mammoth, the cave-bear, the cave-hyena, *Rhinoceros antiquitatis* and others; they dwelt mainly in

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caves; they were entirely ignorant of husbandry; they knew nothing of domestic animals; and unlike their Neolithic successors they never polished, but only chipped their stone implements. It will be seen, however, that these Paleolithic savages were, like the invertebrates, grouped together merely on negative grounds. They all lacked the cultural characteristics of the Neolithic Iberians and Aryans.

This classification was for the time being a satisfactory arrangement, but the Paleolithic Period as so defined was, of course, of very indefinite extent. Indeed, theoretically it comprised all the vast and little-known ages of time which elapsed from the moment when our ancestors first deserved to be called human down to the time when the Neolithic immigrants made their way into Europe. And it has always been evident that so soon as any considerable knowledge was gained of the pre-Neolithic epochs, some other classification would have to be adopted. For on the Darwinian theory of continuous or gradual evolution, it is abundantly clear that the first men must have differed from the late Paleolithic hunters, anatomically, mentally, and socially, far more than these same Paleolithic hunters differed from ourselves.

As a makeshift arrangement the Early Stone Age has been recently broken up into "Early Paleolithic" and "Late Paleolithic" divisions, but even this modification inadequately expresses the newly discovered facts, and in the opinion of the present writer the term "Paleolithic" will have to be carefully redefined or perhaps entirely abandoned.

Let us briefly recapitulate what is now known of the pre-Neolithic men. The Paleolithic Period, as already stated, lies within the Pleistocene or Glacial Period of the geologists, the period of the earth's history immediately preceding that in which we live, or, in other words, the penultimate of the sixteen periods into which it is customary to divide the story of life on the globe. It is now known that in Central and Western Europe the Pleistocene was not a period of continuous glaciation, although in Scandinavia the conditions were in all probability perpetually arctic. In Britain, France, and Germany there were several, probably at least four, glacial cycles; that is to say, there were four ice-ages or "glacial episodes," with consequently three warm interglacial periods between them. Although a number of subdivisions of the Paleolithic Period are now

generally accepted, the exact relationship of these to the phases of the Pleistocene is still in dispute. The names of these Paleolithic epochs are as follows: (10) Azilian, (9) Magdalenian, (8) Solutréan, (7) Aurignacian, (6) Mousterian, (5) Acheulean, (4) Chellean, (3) Strépyan, (2) Mesvinian, (1) Icenian, reading from above downwards, that is, from the latest to the oldest age.

Of the ten divisions, the Azilian certainly extends into Post-glacial times, and in many places this epoch bridges to some extent the hiatus between the Paleolithic and Neolithic Periods, which has already been mentioned. The first two divisions have still a somewhat uncertain status. The epochs are defined, of course, in accordance with the character of the stone (or bone) implements which are discovered at the several levels, the implements being preserved as a rule either in river-gravels or in cave-deposits. The Mesvinian implements have often been described as "eoliths," that is, as alleged stone implements which antedate the paleoliths, and whose authenticity is still questioned by some authorities. The Mesvinian implements are, however, on a somewhat different footing from other eoliths, since they are more widely accepted.[1] The Icenian implements are also in a rather dubious position, especially as some of them are stated by Reid Moir, Ray Lankester, and others to be Pre-glacial, but some at least of these appear to be genuine (particularly the later or Pleistocene specimens) and they will probably be accepted eventually. The implements of the third, fourth, and fifth ages have been found chiefly in drift left by rivers, those of the subsequent epochs chiefly in caves; hence the now discarded expressions "river-drift man" and "cave-man."

As already stated, the Paleolithic epochs and the Pleistocene phases have not been finally correlated with one another, but it is probable that the Aurignacian Age lies wholly within the last Interglacial phase, that the Magdalenian extends on to the very end of the Pleistocene, that the Mousterian begins in the second or middle Interglacial episode and overlaps the Aurignacian, and that the Strépyan, Chellean, and Acheulean cultures flourished during the Middle Interglacial.

Now the greatest break in the story of man in Europe occurs not between the Stone Ages and the Metal Ages, and not between the Paleolithic and Neolithic Ages, but between the Mousterian and Aurignacian divisions of the Paleolithic.

[1] Notably by Prof. Sollas, who is a keen critic of eoliths.

During the last four Paleolithic Ages several distinct races inhabited Europe, which may or may not have left survivors into Neolithic times, and which may or may not, therefore, have been our own direct forefathers. But whether or not these peoples were exterminated by the incoming Neolithic tribes, they differed in minor characters only from ourselves, and differed from one another less than the divergent races still living in different parts of the world. In a word, they belonged to our own species, *Homo sapiens*. In their anatomy they were entirely human, and in their culture they were less rude than some savages of the Nineteenth Century.

When, however, we pass back from the Aurignacian into the Mousterian Age the scene entirely changes. We find ourselves on utterly unfamiliar ground, and in surroundings where the analogy with the lowest living races no longer affords a very safe guide. Europe was inhabited during Acheulean and Mousterian times, and possibly earlier also, by the famous Neandertal race, who, it is now realised, constituted a distinct species, named *Homo neandertalensis* or *Homo primigenius*. This extinct species is now familiar to us from a number of discoveries, of which the most important are those at Neandertal itself, Gibraltar, Spy in Belgium, Krapina in Hungary, and La Chapelle-aux-Saints, Le Moustier, and La Ferrassie in the south of France. As is well known, the Neandertaler differed from *Homo sapiens* in having an extremely receding forehead, with enormously developed brow-ridges, and in having his cranial axis disposed at a somewhat different angle. Moreover he exhibited a heavier and stouter development of bone in all parts of his body, and his brain, although as large as that of the living species, was distinctly more simian in structure.

These Neandertalers were contemporary for a short time, but probably only for a short time, with the very different Aurignacian races. It is natural to suppose that the brutish Mousterians were exterminated by the higher type, and so different are the two species that it is more than doubtful whether it was physically possible for any miscegenation to have occurred. The displacement of *Homo neandertalensis* by *Homo sapiens* was probably not a very long process. It is true that from time to time various "discoveries" have been announced in which skeletons of the modern type of man have been found in strata older, sometimes much older, than the Aurignacian. As a rule these skeletons

have been remarkably well preserved, and under these circumstances it is not surprising that they have been received with a great deal of scepticism, and that it has been suggested that they are probably interments. One of the most recent of these finds is the so-called Ipswich skeleton, which was unearthed by Reid Moir, and has found a powerful advocate in Prof. Keith. This discovery has, however, recently been subjected to most severe criticism by W. H. Sutcliffe[1] and others; and it may be taken as certain that all the supposed discoveries of pre-Aurignacian *sapiens* will not bear close examination. And, indeed, it appears very unlikely that true man can have inhabited Europe for long before the Aurignacian epoch, because we know that the Neandertalers lived here, probably in considerable numbers, before that age, and it is improbable that the higher and better armed type, if it had then been living in this part of the world, would have tolerated the presence of its bestial relative.

Thus it will be seen that the Paleolithic Period includes within itself very dissimilar elements. The gap which separates the Mousterian from the Aurignacian is more profound than any break which occurs in all the succeeding ages from the Aurignacian to the Twentieth Century. The Aurignacian and all that comes after it constitute the era of *Homo sapiens*, of true man; before the Aurignacian we are back among kindred but unfamiliar creatures. It is clearly an irrational arrangement to group the earliest true men together with the various extinct species under the title "Paleolithic"; and even if it be argued that the prehistoric periods are founded upon cultural not upon racial considerations, the break between the Neandertalers and the artistic and much more skilful Aurignacians is very great—and, in any case, an event of such importance as the appearance of true man should be expressed in classification.[2]

Passing farther back behind the Aurignacians, our knowledge of the extinct members of the Hominidæ has been greatly extended by the epoch-making discovery at Piltdown, Sussex, which we owe to the enterprise and patient research of Mr. Charles Dawson and Dr. Smith Woodward. This discovery has given us a fifth species of the Hominidæ. The Neander-

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[1] *Proceedings of the Manchester Literary and Philosophical Society*, 1913.

[2] I make the suggestion that the Aurignacian and three subsequent ages should be classed together as *Deutolithic*, and the previous epochs grouped as *Protolithic*.

talers are not certainly known to extend back farther than the Acheulean Age, but behind them we are acquainted with the existence of three still more ancient species. Unfortunately each of these is only known from a single discovery, as follows: the Ape-man of Java, *Pithecanthropus erectus* (Dubois); the Heidelberg man, *Homo heidelbergensis* (Schœtensack); and the Piltdown Race, *Eoanthropus dawsoni.*

The Javan specimen is very distinct from *Eoanthropus*, from the Neandertalers, and still more, of course, from man—so distinct indeed that the creature may even be nearer to the *Simiidæ* than to the *Hominidæ.* As for *H. heidelbergensis*, only one lower jaw of the species has been discovered, so that it is impossible to speak with any confidence¦ of the characters of this type. The jaw has indeed been variously described as akin to *Pithecanthropus* (by Duckworth), and as the first and most primitive of the Neandertalers (by Keith). Only a small fragment[1] of the Javan animal's jaw was found, but so far as it is possible to judge it seems probable that *heidelbergensis* claimed closer affinity with the Neandertalers than with *Pithecanthropus.* The Heidelberg mandible is not very unlike the various jaws of *neandertalensis* that have been unearthed, but it would, of course, be unsafe to assume from this that the complete skeletons of the two types were also similar, and it is not even possible to be absolutely certain that this most ancient mandible was associated with the very receding forehead which is so characteristic alike of the Neandertalers and of *Pithecanthropus.*

It is, however, when we compare the well-preserved Heidelberg jaw with the right half of a mandible that was found with the skull at Piltdown that we find ourselves face to face with certain most remarkable facts. These two jaws are utterly unlike one another. And in various respects each diverges more from the other than either differs from a human jaw. At first sight this is perhaps not very surprising, since it might have been foreseen that in the last stages of the upward evolution of the Primates towards humanity, as in the earlier stages, side branches would have been thrown off. When, however, the differences between the extinct species are examined in close detail, the problem becomes puzzling in the extreme. The inter-relationships of the several kinds in the family tree

[1] The fragment has not yet been described.

are very difficult to discern. No doubt this is due to the very meagre amount of evidence available. But as this evidence is likely to remain scanty for many years to come, it is worth while following up the suggestions that Dr. Woodward has thrown out in regard to the genealogy of the Hominidæ.

The Piltdown skull has now been fully described by Woodward,[1] and the brain case proves to be thoroughly human, differing from man only in the extreme thickness of the bones and a few minor features. The cranial capacity is very low (about 1,070 ccm.), but not below that of the lowest modern savages, the Tasmanians. The forehead is fairly steep and there are only small brow-ridges, so that in this respect *Eoanthropus* resembles *H. sapiens*, not *H. neandertalensis*. The facial parts were not found, and their form can therefore only be inferred from the mandible. It is, however, mainly in the mandible that the new genus differs from man. As in other ancient jaws, the ascending ramus is wide and the sigmoid notch (the concavity in the dorsal border of the ascending ramus) is relatively shallow. The chief peculiarity occurs, however, in the region of the symphysis, where the jaw is strengthened by a horizontal plate, or flange, which constitutes, in fact, a very short bony floor to the jaw (see fig. 1). This flange is completely absent in man, and is, indeed, an entirely simian structure, the chimpanzee possessing an identical piece of bone. From the presence of this flange it is evident that the genio-hyo-glossal and genio-hyoid muscles took their origin in a deep pit, and were therefore presumably weakly developed ; and it is a legitimate inference from this, and from the related fact that the mylo-hyoid and internal pterygoid were also weakly developed (as proved by the markings on the inner face of the ramus), that the Piltdown race was almost or quite speechless. The upper part of the front of the jaw was broken away, so that the anterior teeth can only be filled in by intelligent guesswork. It is clear, however, that whether or not the teeth were quite as large as Woodward makes them, they must have been considerably bigger[2] than those of any other known member of the Hominidæ, with the possible exception of *Pithecanthropus*.

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[1] *Quarterly Journal of the Geological Society*, March 1913.
[2] Note added to press : This statement is confirmed by the discovery at Piltdown on August 30 of a canine tooth, which is only slightly smaller in size than the hypothetical canines in fig. 1.

Woodward founds his new genus mainly upon the form of the mandibular symphysis, which he contrasts with that of the three species of *Homo*. The contrast in this respect between *Eoanthropus* and *heidelbergensis* is, however, less striking than Woodward seems to imply, for there is a clear vestige of the flange in the Heidelberg jaw, and in the latter, as in the Piltdown mandible, the genio-hyo-glossal and genio-hyoid originate in a pit. In fact, as Prof. Sollas has well remarked, in the structure of its symphysis the Heidelberg jaw "stands
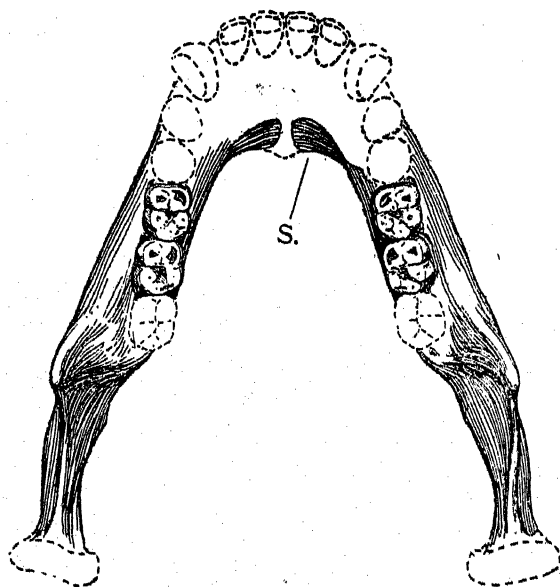
FIG. 1.—The Piltdown jaw, as reconstructed by Smith Woodward. S = the horizontal flange. The parts shaded are those actually known.

(Reproduced by kind permission from the *Quarterly Journal of the Geological Society*.)

midway between man and the anthropoid apes," and therefore midway between *sapiens* and *E. dawsoni*. As regards date, there is little reason to doubt that Dawson is right in believing that the Piltdown skull is contemporaneous with the Paleolithic implements which were found near it. These implements are late Chellean or early Acheulean. It is certainly remarkable that a creature with such a simian jaw should have been living in Chellean times, but it is probable, as Woodward suggests, that the representatives of the Piltdown race living in what is now Britain were a surviving remnant of a very ancient stock.

They were no doubt exterminated eventually either by *heidelbergensis* or *neandertalensis*. The Heidelberg jaw is certainly not later than the Mesvinian Age (which must be placed in the first Interglacial phase), and it may be earlier; it is thus at least one glacial cycle older than the Piltdown specimen.

The characters of the five species may therefore be tabulated as below :

	Pithecanthropus.	*Eoanthropus.*	*H. heidelbergensis.*	*H. neandertalensis.*	*H. sapiens.*
Cranial capacity .	850 ccm.	1,150 ccm.[1]	—	1,400 ccm.	1,500 ccm.[2]
Forehead . .	Receding	High	—	Receding	High
Brow-ridges .	Large	Small	—	Large	Small
Teeth. . .	Large (?)	Large	Small	Small	Small
Ascending ramus of mandible .	—	Intermediate	Very wide	Intermediate	Narrow
Sigmoid notch .	—	Intermediate	Very shallow	Intermediate	Deep
Symphysis . .	—	Simian	Intermediate	Almost human	Human
Date . . .	Pliocene	Middle Pleistocene	Early Pleistocene	Middle Pleistocene	Late Pleistocene and Recent

Now, it used to be generally believed that *neandertalensis* was directly ancestral to *sapiens*, a belief that was in no way inconsistent with the sudden appearance of *sapiens* in Europe, for the evolution from one type to the other might well have taken place in another continent, whilst the Neandertalers in Europe were in a stagnant condition. This theory has been recently losing ground, however, and it is now more commonly held that the Neandertalers represent a side branch, showing some signs of what is loosely called degeneracy, and leading nowhere. Woodward adopts this latter hypothesis, and develops it further. He believes that the discovery of *Eoanthropus* proves that the high forehead is a primitive character of the Hominidæ, and that the low forehead and great brow-ridges of *neandertalensis* are therefore a secondary acquirement, and he proceeds to expound the view that because the young of all the anthropoid apes have likewise a relatively high forehead, therefore (on the recapitulation theory) the apes too are to be regarded as descended from animals with a steep, manlike,

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[1] The specimen found is that of a female, and therefore below the average for the race. (Prof. Keith's estimate, recently given at the International Medical Congress, is higher.)

[2] Europeans.

cranial arc. Thus Dr. Woodward inclines towards Prof. Klaatsch's famous heresy that the apes are descended from creatures who were in many respects almost human, although of course he does not countenance the more extravagant part of Klaatsch's hypothesis, in which that authority associates particular apes with particular races of men—the gorilla with the negro, and the orang with white men.

Due weight should certainly be attached to the fact that in the case of *Eoanthropus* a high forehead is associated with such a primitive jaw. And the manlike skull of the young ape is certainly a curious feature, although a tendency towards the same rounded form may be seen in the fœtus of many other mammals besides apes, which robs this fact of much of its importance. It must be remembered, however, that a low receding forehead is universal among the lower Primates, and hence was indubitably present in the more distant ancestors of both *Hominidæ* and *Simiidæ*. Thus convincing proof is necessary before we are justified in interpreting the low forehead of the apes, of *Pithecanthropus*, and of *neandertalensis* as a secondary acquirement; for there is of course the alternative explanation that all these animals possess a low forehead merely because their ancestors never had anything else. This is at first sight the simpler theory, and the importance of the mandibular symphysis, as a sign of kinship, is not strengthened by an examination of the Heidelberg jaw. The Piltdown and Heidelberg mandibles are compared in fig. 2. It will be seen at once that the "ascending" (or vertical) part of the ramus is much wider in the German specimen, and that the whole conformation of the two bones is entirely dissimilar. The first and second molar teeth are the same size in the two jaws, but the Sussex specimen is much the larger anteriorly, hence the larger front teeth. Now, Woodward derives both *sapiens* and *neandertalensis* directly from *Eoanthropus*. It is not quite clear where he would place *heidelbergensis*, but since he is content to leave the latter species in the genus *Homo*, he presumably regards it as a twig of the branch which gave rise to the other two species. Now, it may be possible to derive the relatively narrow jaw of a Neandertaler from the type of mandible exhibited by *Eoanthropus*, but it is difficult to see how *heidelbergensis* could have been evolved from the same source. It has long been known that a shallow sigmoid notch and a

powerful wide ascending ramus are characteristic of all the lower human jaws. And now we are faced with the curious paradox that the Heidelberg mandible possesses a somewhat shallower sigmoid notch and a much wider ascending ramus than the Piltdown jaw. If, therefore, *heidelbergensis* be descended from the Piltdown race, the ordinary course of evolution was reversed, and the wide ascending ramus of *heidelbergensis* must be regarded as a secondary acquirement. It would be rash to say that this is an impossibility, but it is certainly a curious conclusion. The family tree constituted on this hypothesis is represented in fig. 3. *H. heidelbergensis* is here conceived to be
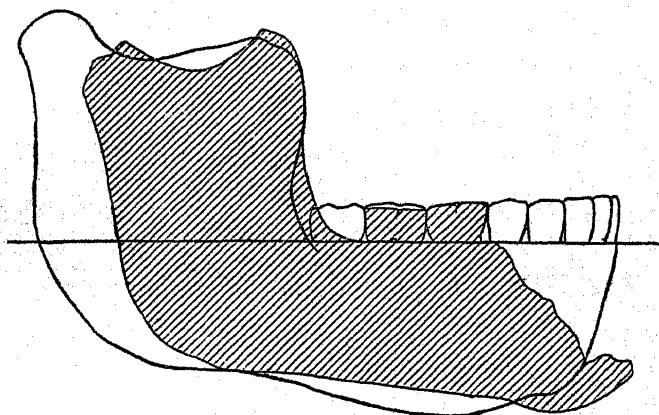
FIG. 2.—Mandibular ramus from Piltdown superposed on that of *Homo heidelbergensis*. Two-thirds of the natural size.

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a " degenerating " branch, given off from the main stem at a point where the symphysis had become half-human.

The only further comment that it is necessary to make on this theory is that it is fatal to the conception that *heidelbergensis* is directly ancestral to *neandertalensis*; it would be too much to believe that the immensely wide ascending ramus was acquired and then lost again.

If, however, we abandon the hypothesis that *Eoanthropus* is directly ancestral to *Homo*, another explanation of the characters becomes possible. Why, it may be asked, should not *heidelbergensis* and *Eoanthropus* be descended from a not distant ancestor which combined the primitive features of each,

that is, combined the massive ramus of the one with the large teeth and simian mandibular symphysis of the other? The genealogy of the four species concerned would then be as shown in fig. 4. This second hypothesis obviates the necessity of assuming a reversed evolution in the case of the Heidelberg jaw, and the low forehead of the Neandertalers may be once more explained as degeneracy, it being assumed that "X," like *Eoanthropus* and *sapiens*, had a high forehead. But the theory encounters formidable difficulties. It is clear,

H.sapiens.

H.neandertalensis.

H.heidelbergensis.
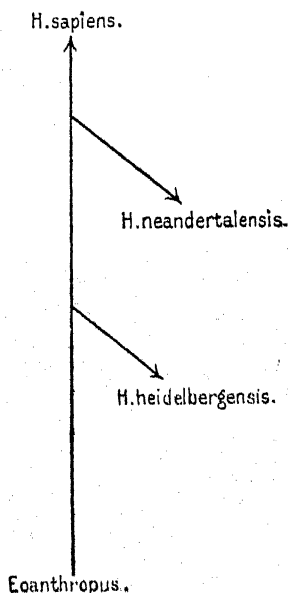
Eoanthropus.

FIG. 3.

for instance, that if it be true, the narrower ascending ramus and the deeper sigmoid notch were acquired independently by *Eoanthropus* and *sapiens*—that is, that these similarities are no sign of kinship, but are due to parallelism in development. In this connection it is interesting to notice that if the outline of the jaw of a European be superimposed upon that of *heidelbergensis*, the chin region of the European's jaw projects beyond the front of the ancient jaw, just as that of *Eoanthropus* projects in Fig. 2, only rather less so. Since, however, modern jaws have a chin prominence, which *Eoanthropus* certainly had not, the front curve of the jaw passes backwards again as it passes upwards,

the teeth of *sapiens* being as small as, or smaller than, those of *heidelbergensis*. The chin prominence of modern man is usually explained by the rapid contraction of the alveolar surface in accordance with the reduction of the size of the teeth during the latest stages in human evolution, and the chin is therefore a hint, though only a hint, that true man has a very recent ancestor with teeth larger than those of *heidelbergensis*. Or, in other words, it is easier to derive the human jaw from one that was large anteriorly and small posteriorly, than to derive it
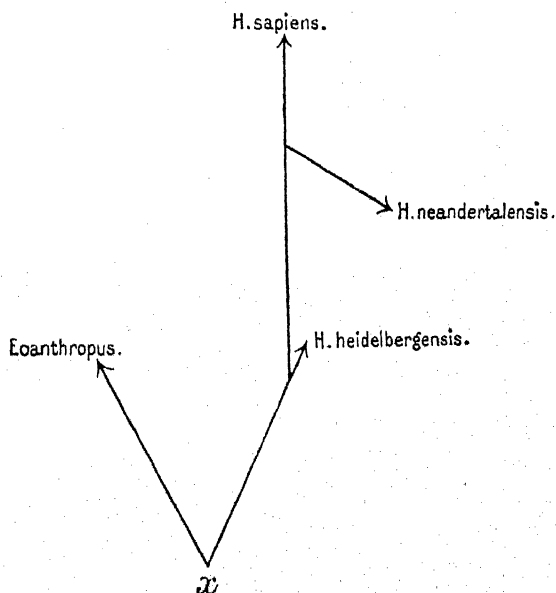
FIG. 4.

from a mandible of the Heidelberg type, and Woodward's Piltdown jaw has just the form required by theory. Amidst the maze of uncertainties, it appears that Woodward is wholly right in claiming a close relationship between the Piltdown Race and true man.

Thus both these theories, though not impossible, are difficult to reconcile with the extraordinary differences between the two most ancient jaws. But the facts are susceptible to another interpretation of a totally different kind. Is Dr. Woodward right in the importance he attaches to the mandibular symphysis as a sign of relationship? May not the absence of a flange,

and the greater development of the tongue-muscles therein implied, have been developed independently in more than one branch of the evolving Primates? The phenomenon of convergence, or parallelism in evolution, is one that has long been familiar to naturalists. There is the famous case of the cephalopod eye, which simulates so closely in its structure the eye of a vertebrate. In two widely separated branches of the animal kingdom the same need was met in the same way. Again, there is in Australia a little animal called the pouched mole. This creature is a marsupial and is consequently allied to the kangaroos. But it lives underground, and in its appearance, and in the adaptation of its limbs and form to a subterranean mode of life, the little beast exactly resembles the real moles of Europe. In this case, too, the same need has been met in the same way. With our present knowledge of the early Hominidæ it is of course impossible to speak with confidence of the factors at work in the evolution of those creatures, but it is quite likely that this principle of convergence played some part in that process. Our second hypothesis, indeed, necessitated it in certain minor respects. But when we recall in imagination the conditions under which the divers sorts of half-men lived, we can see that convergence may have been a most conspicuous phenomenon in their progress. They were highly gregarious animals, whose very survival must constantly have depended upon the power of the individuals efficiently to combine. And to combine effectively it was before all things necessary that they should be able to communicate with one another. The power of speech was a crying need of the advancing Primates—a want no less urgent than muscular fossorial limbs to the marsupial of mole-like habits. It was language that transformed the horde into the tribe. The creatures were probably widely dispersed over the earth whilst they were yet speechless. And rudimentary powers of speech may thus have been acquired independently by more than one species; and this, not blood-relationship, may be the explanation of the man-like symphysis of the Heidelberg jaw. And those who are impressed with the neandertaloid features of that specimen might go farther and re-establish the connection between *heidelbergensis* and *neandertalensis*. The descent would then work out as shown in fig. 5.

On this last hypothesis the common ancestor, "X," is con-

ceived as possessing a simian mandibular symphysis, a massive jaw, large teeth, and probably a low forehead. *Pithecanthropus* may possibly have exhibited all these primitive characters. If this interpretation of the phenomena were established, it would of course become necessary to remove *heidelbergensis* (and possibly *neandertalensis* also) from the genus *Homo*.[1]

The suggestions thrown out in this paper suffice only to show how little is certainly known of the inter-relationships of the fossil Hominidæ. It would be altogether premature to
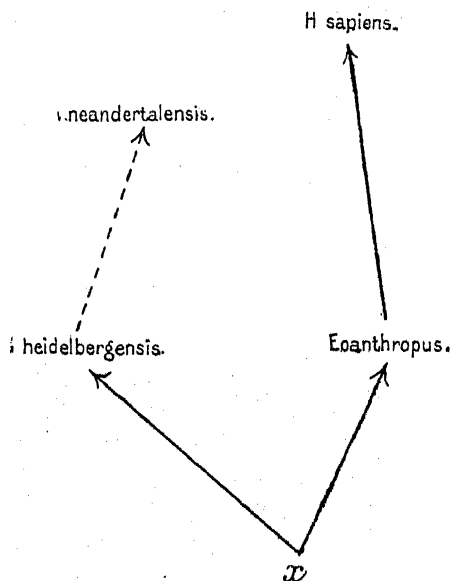
FIG. 5.

attempt to dogmatise upon the rival possibilities; none is free from difficulties. I am, however, strongly inclined to think that both the apes and *Pithecanthropus* have a low forehead not because they are degenerate, but because they are immediately descended from monkeys. And even in its more plausible application to Neandertal man, I view the degeneracy theory with considerable suspicion.

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[1] Whilst the present article was in the press, Mr. W. H. Sutcliffe kindly sent me a copy of his above-mentioned paper, of which I had only seen a preliminary report. His main theme is a convincing criticism of pre-Aurignacian *sapiens* and of eoliths, but I find that incidentally he adopts what I have called Hypothesis 3, although without giving any reasons for his belief.

New light may be thrown upon man's origin from an entirely different direction. One school of naturalists, including the most erudite of experimental biologists, now deny that there is any evidence that evolution has ever taken place gradually, in the manner Darwin supposed. They believe that living organisms have progressed not by imperceptible stages, but by sudden mutations or transformations. Certainly students of human paleontology are not in a position to refute such a statement. As far back as the Aurignacian everything is only too familiar ; behind the Aurignacian all is mystery.

LECTURE I [1]

NATURE AND NURTURE IN MENTAL DEVELOPMENT

By F. W. MOTT, M.D., F.R.S.

Pathologist to the London County Asylums

THE problem of nature and nurture in mental development is one that has recently acquired importance for various reasons, such as the increase of certified insanity and the enormous sums of money spent on asylums for housing lunatics; and the recognition by the public that insanity, epilepsy, and feeble-mindedness are in great measure due to inheritance has lead to a widespread feeling that some check should be placed upon propagation of the mentally unfit. This is becoming daily more manifest from two causes: The migration and emigration of the mentally and physically fit from the rural districts and the sedimentation of the unfit in the slums of our large cities where degraded pauperism exists to so great an extent.

The rapid growth of population in this country commenced with the growth of industrialism and the rise of towns and cities with inhabitants engaged in factories and manual occupations, where individualism necessarily became subject to collectivism. Just as in the human body there is differentiation of structure and function, so there is in the modern complex social organism; and just as in the human body the failure of function of one organ may disturb the harmony of function of the whole body and mind, so in the social organism a strike, even by a humble section of it, may lead to disorganisation of the whole.

The collection of large numbers of people in towns and cities who were previously accustomed to individualism in matters of sanitation led to a most deplorable state of affairs, and Sir Edwin Chadwick, a pioneer in sanitary science, in whose honour these lectures were given, was the first to call attention to the necessity of legislation to remedy the growing evil.

In 1842 a report was published by him on "The Sanitary Condition of the Labouring Population of Great Britain." In

[1] The Chadwick Public Lectures, 1913.

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this he called attention to the filthy conditions under which the English labouring classes lived. To remedy this, collective responsibility undertook the first stage of social reform by cleansing, lighting, and policing of the streets, and by establishing systems of water-supply and drainage in our cities and large towns.

The second stage of social reform was factory legislation, for regulating the conditions of work in factories, for protecting those employed in unhealthy occupations and industries, and for restricting the work of women and restraining the work of children. Like many other essential social reforms, it met with much opposition.

The third stage was the nationalisation of education in 1870 and the extension of the meaning of education has so far progressed that it now includes not only mental but also physical development, the exercising and even feeding of children where necessary, the care of the feeble-minded by the formation of special schools, medical inspection and notification of infectious diseases, treatment of children's ailments, and attention to the eyes, ears, and teeth at the school-age.

Last to occur, the effort to guard the child before the school-age, even as soon as it is born, even before birth through attention to the future mother. There is yet one other educational method of far-reaching importance to the masses, and that is the scout movement and officers' training corps, by which boys and youths are trained to become self-reliant yet unselfish, and submissive to discipline without losing individuality. That spirit of *esprit de corps* which is the striking feature of our public schools and universities is by this movement extended to the boys and youths of all classes, and it cannot fail to have an important influence upon development of character. Each of these stages has supplemented and reinforced the other; yet we hear on all sides the pessimistic cry of the degeneration of the race set up by a few unthinking people who advocate a "laissez-faire" or the so-called "better dead" theory of all those who are unable, through inborn lack of vitality, to resist racial diseases. Are we to listen to these pessimists? No! Rather should we look with pride to what has been done in the last fifty years to better the condition of the people.

In respect to tuberculosis I will quote the words of a great French scientist uttered at the International Congress of Tuber-

culosis held in London in 1901. Professor Brouardel, of Paris, said in his address : " You have diminished the mortality in England from tuberculosis by 40 per cent.," and he attributed this decline to the numerous Acts of Parliament and measures promoted by private individuals to render more salubrious the dwellings of the poor and the conditions under which they live and carry on their occupation in factories, mines, and workshops throughout the kingdom. We can from this realise what a great work Sir Edwin Chadwick did in combating this racial disease by his pioneer work in sanitary science.

The housing of the poor is now the bed-rock of physical and mental hygiene and still calls for all the efforts which Parliament and private enterprise can exert. By energetic amelioration of the present conditions, especially those of the casual workers in cities, and of the rural population, more can be done than by any other means to " diminish " the death-rate from tuberculosis, the contamination of the morals of the poor and the infant mortality. The social reformer justly recognises that much good raw material may be spoilt by a bad environment; he recognises also the fact that a healthy mind can only exist in a healthy body and that an inborn virtue may by evil surroundings and imitation be the source of contracted vices. The ardent and enthusiastic social reformer should recognise the fact that you do not gather grapes from thorns nor figs from thistles ; that the children of feeble-minded parents will, in spite of good nutrition and favourable surroundings, tend to be more or less feeble-minded ; that the most dangerous form of feeble-mindedness, now that Nature is no longer left to itself to select by survival of the fittest, is the higher-grade imbecile, who is fertile and able under the easier conditions of survival brought about by social reform to multiply and infect good stocks. Seeing that we cannot prevent this occurring, the only hope is that the Mental Deficiency Bill which has now passed a second reading may become law; its object being to segregate early mentally defective children in their own interests and in the interests of the community. Inasmuch as feeble-mindedness occurs in all classes, I should advocate notification of all mental defectives ; and where parental responsibility has failed, then in the interests of the child the Government should take up the responsibility of guardianship as a protective measure—due precautions being taken and every opportunity given of restoration to social

privileges, should it be found desirable by the properly con-
stituted authorities. Some of these practical problems concern-
ing mental hygiene will, I trust, be better understood by the
public, if they will consider the subject from the physiological
and medical points of view, as well as from the economic and
political.

MENTAL HYGIENE FROM A PHYSIOLOGICAL STANDPOINT

Structure and Development of the Brain.—The most striking
anatomical distinction of man from the anthropoid apes is the
enormous increase in the development of the great brain—the
cerebrum—and this increase in size is due almost entirely to an
enlargement of that part of the great brain which occupies the
cranial vault and gives to man a dome-like shape to the skull.

Gall, the phrenologist, more than one hundred years ago,
was the first to point out that that part of the brain with which
the higher mental activities are connected must be the cerebral
hemispheres. He said: "If we compare man with animals we
find that the sensory functions of animals are much finer and
more highly developed than in man ; in man, on the other hand,
we find intelligence much more highly developed than in animals.
Upon comparing the corresponding anatomical conditions we
see," he said, "that in animals the deeper situated parts of the
brain are relatively more developed and the hemispheres less
developed than in man ; in man the hemispheres so surpass in
development those of animals that we can find no analogy." Gall
moreover studied the brains of imbeciles and demented persons,
and was the first to point out that the disorder and deficiency of
mind of one, and the disorder and loss of mind of the other,
should be correlated with the deficient development of the hemi-
spheres in the feeble-minded imbecile and the destruction of the
hemispheres in the demented lunatic.

Unfortunately Gall's imagination outstripped his judgment
and he wrecked his fame as a scientist by associating mental
traits of character with conditions of the skull ; then, encouraged
by a wide-spread wave of popular sympathy in the endeavour
to materialise and localise the functions of mind, he launched into
speculative hypothesis unsupported by facts. His doctrine of
phrenology was shown to be absolutely illogical ; but the
importance of his work in showing that the brain was the organ
of mind has since been recognised.

"*Body and Mind.*"—Although the brain is the organ which stores the recollection of past experiences and the bonds that unite them, thereby enabling the individual to adapt himself to environment, yet strictly speaking the mind is directly dependent upon the vital activities and harmonious interactions of all the organs and tissues of the body; for of what use would the brain be without the peripheral sense organs and the nerves which connect them with the spinal cord and brain? These are the avenues of intelligence, as was clearly recognised by Aristotle in his famous dictum: "Nihil in intellectu quod non fuerit prius in sensu." But another fundamental function of the brain besides perception of the external world and its surroundings is the consciousness of the individual's own personality, his appetites and desires, which are due in great part to the organic sensibility of the nerves of the body and internal organs, which without cessation are continually carrying messages to the brain, making us aware of our existence and our needs. The quality of the blood and the presence in it of subtle bio-chemical substances produced by secreting glands and the viscera have a profound influence upon states of consciousness and mental activity. It is the consciousness of feelings connected with the preservation of the individual and the preservation of the species which constitutes the fundamental biological source of all vital activity, and is thus poetically expressed by Schiller in the following lines:

Durch Hunger und durch Liebe,
Erhält sich die Weltgetriebe.

The mental states concerned with the consciousness of appetites and desires and the control of the instincts and habits associated with their gratification, the avoidance of pain and the obtaining of pleasure essential for the preservation of the life of the individual and reproduction are the mainspring of human activities, passions, and emotions.

Plan of a Simple Nervous System.—Let us now consider for a few moments the general plan of a nervous system.

The nervous system of all animals with a nervous system is constructed on the same plan. As we rise in the zoological scale it consists of more and more complex systems and groups of neurones. A neurone is a nervous unit which consists of a nerve-cell with branching processes; one process becomes

the axial core of a nerve fibre: this is termed the axon, the others are termed dendrons. All nervous action is reflex, and the simplest reflex act is the first term of a series, of which the most complex volition is the last. Therefore before proceeding to discuss the brain, the most complex organ in nature both as regards structure and function, let me call your attention to the simplest form of nervous system illustrated in this diagram. You observe S (fig. 1) is a sensory nerve-cell with
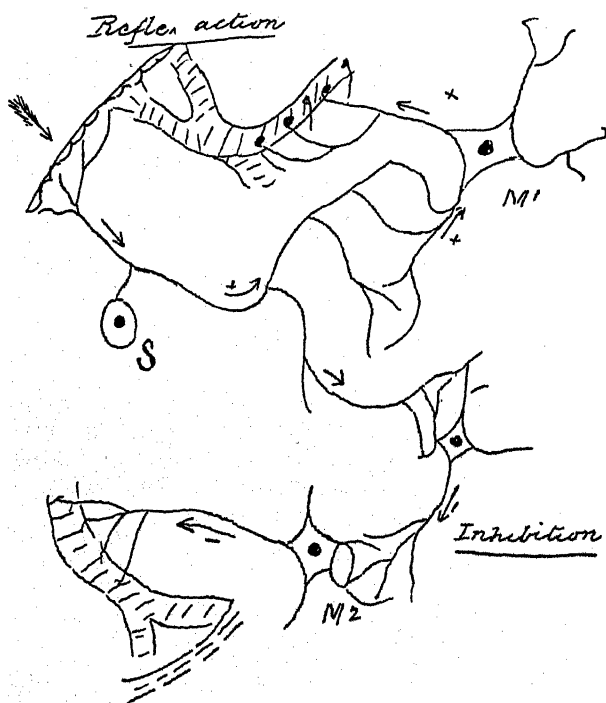
FIG. 1.

branching processes; one branch ends in the skin, the other branch proceeds centrally, and this you see breaks up into a number of fine terminals which are brought into relation with the branching processes of M^1, a motor cell; proceeding away from this cell is a process, the motor nerve, which terminates in a muscle connected with the sensitive skin. Stimulation of the sensory nerve in the skin, it matters not whether it is chemical or physical, produces what is known as an afferent nervous stimulus, which travels in the direction shown by the arrow to

PLATE I
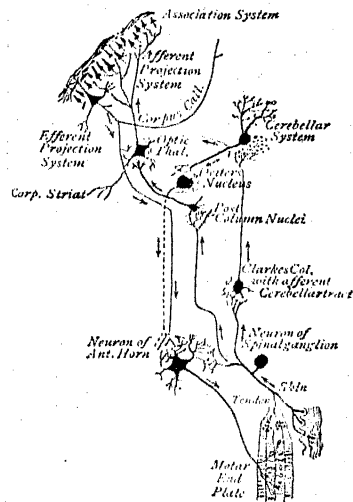
FIG. 1.—The three systems of afferent, efferent, and association neurones. Spinal, cerebellar, and cerebral necessary for perfect conscious voluntary movement.

It will be observed that when a muscle contracts under the influence of voluntary stimuli from the brain, alterations in tension of the skin, muscle tendon and structures of joints cause afferent impulses (kinæsthetic) to pass up to the brain. Every movement is associated with ingoing and outgoing currents. The cerebellar system which is indicated by afferent and efferent systems is especially concerned with reinforcement of muscular action.

the terminals of the sensory neurone S, where it excites the terminals of the motor neurone M, giving rise to an outgoing efferent current which stimulates the muscles and causes its contraction.

Let us suppose the stimulus to be a painful and therefore a harmful one, the effect of the neuro-muscular mechanism will be a protective reflex action, the contracting muscle withdrawing the skin surface from the cause of the pain. You will observe that the diagram shows that the sensory neurone consists of a cell with a process which divides into two branches; one proceeding to the skin—this is the sensory nerve—the other branch dendron proceeding centrally to end in a terminal arborisation. The current of nervous action resulting from the stimulus always proceeds towards the centre; it is afferent; the fine terminals of the central projection of the nerve cell are in physiological (that is functional) but not anatomical connection with the branching processes, dendrites of M, the motor cell· This alterable functional connection is spoken of as the synapse; the motor cell, M^1, gives off one process which becomes the essential conducting axial core of a motor nerve fibre which ends in the muscle; and the current of nervous action along this is always outgoing or efferent. We have thus two systems of neurones: (a) afferent sensory, (b) motor efferent. There is yet another neurone, A, which you observe associates the synapse of S and M^1 with a second motor neurone element M^2, which innervates another muscle that is antagonistic in its action to that supplied by M^1. Stimulation of the sensory nerve in the skin may give rise not only to reflex contraction of the muscle supplied by M^1, but also through the association neurone A, to relaxation by inhibition of contraction of the muscle supplied by M^2.

The special function of the brain is inhibition or control of instinctive reflex action, and this is done by its associative memory of past experiences.

The neurones, I have said, are independent nervous units; they are in anatomical contiguity but not in continuity. The cerebro-spinal and sympathetic nervous systems are made up of neurones which we may regard as complex highly differentiated cells obeying, however, the same laws of nutrition, repair, and waste as other cells of the body.

The neurones are the essential nervous elements, and they,

together with the supporting connective tissue elements, neuroglia cells, blood vessels, and lymphatics, form the central nervous system. Functionally speaking there are three systems of neurones in the brain and spinal cord: (1) afferent projection system; (2) efferent projection system; (3) association system (Plate I, fig. 1).

The Convolutional Pattern of the Brain.—If we look at a human brain we see that the surface of the hemispheres exhibits a number of folds and fissures giving rise to a pattern which I will speak of as the convolutional pattern (Plate II, fig. 1). A section through any of these folds or fissures shows that the external surface or cortex, as it is called, is of a pinkish grey appearance contrasting with the dead white of the subjacent part of the brain. Now a microscopic examination of the grey matter and the white matter explains why there should be this difference in colour. When highly magnified a thin section appropriately stained by dyes shows the grey matter to consist of innumerable ganglion cells to and from which conducting fibres proceed. The microscopic architecture of the grey cortex exhibits a cell and fibre structure of extraordinary complexity. The diagram (Plate III, fig. 2) of a section of an adult brain is to illustrate this cell and fibre architecture. You observe that the cells are arranged in six layers, and there are also layers of fibres, some of which run horizontally and some have a radial direction. The horizontal conduct association impulses. Although there is a general similarity in the cell and fibre structure of the cortex of the brain, yet the whole surface of the brain can be mapped out into territories of different cell and fibre architecture (Plate II, fig. 2); and physiology and medical science teach that there is a corresponding difference in function.

I have remarked that the grey cortex has a pinkish colour because (relatively to the white matter) the blood supply is very abundant. Now the subcortical matter is white because the nervous processes of the cells of the grey matter are surrounded with a sheath of myelin or phosphoretted fatty substance. The bio-chemical processes incidental to all nervous action, therefore to the mental activity of the brain, take place in the cell structure of the neurone. The cortex is the seat of consciousness and mental activity, and the functions of the cortex require a continuous supply of oxygenated blood. Un-

PLATE II
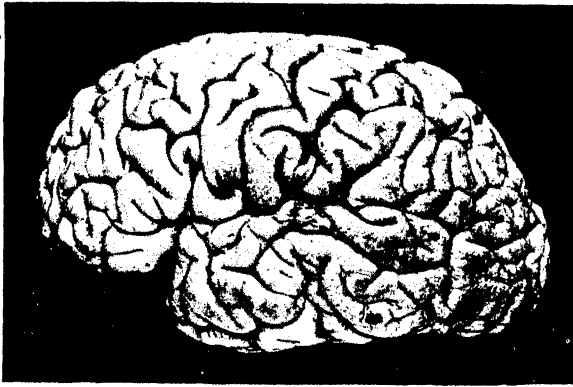
FIG. 1.—External surface of the left hemisphere of brain of an intellectual man showing a complex convolutional pattern.
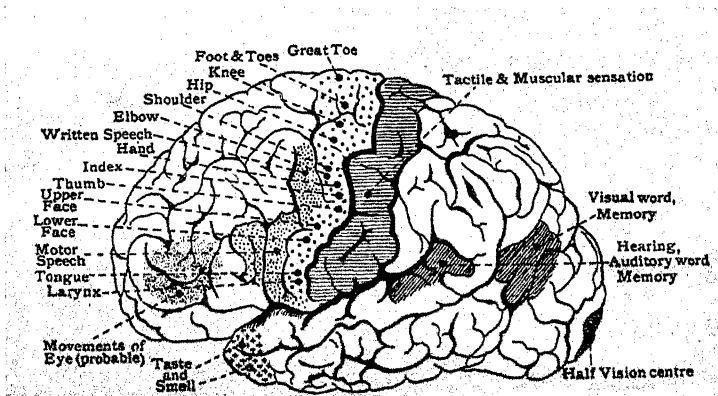
FIG. 2 – The same hemisphere as fig. 1, to show the various areas of ascertained definite physiological function.

The coarse black dots in the precentral region indicate points which when electrically excited give rise to definite movements. Behind the central fissure the cross shading indicates the region of tactile muscular sense. A large part of the auditory centre cannot be seen as it forms the floor of the posterior part of the sylvian fissure. The greater portion of the half vision centre lies on the mesial surface and cannot be seen. The sensory speech centres are indicated by oblique shading ; the motor speech centre of Broca is indicated by fine dots, and above it the centre for writing. Destruction of these centres causes motor aphasia and agraphia.

consciousness occurs if the blood supply fails for a few seconds, hence we understand why the superficial cortex of the brain is pinkish and receives so abundant a supply of blood.

Now if we look at a child's brain before birth at an early period, the surface is quite smooth and there is no internal white matter. As the embryo grows, primitive folds and fissures appear, and a month or so before birth we have a brain characteristic of the species; at birth we have the brain of the individual; the convolutional pattern formed by the folds and fissures (as with the physiognomy) may bear a resemblance to other individuals, but will exhibit features which differ from other individuals (Plate IV, figs. 1, 2). No two patterns are identically similar any more than two faces are identical; but just as the faces of relatives are likely to be similar, so Karplus showed that the pattern of the brains of infants who were related exhibited similarities; and Dr. Edgar Schuster, at my suggestion and from material with which I provided him, has carefully investigated and recorded the similarities in the brains of adult relatives.

Now we may ask : Why should the brain exhibit these folds and fissures ? The blood vessels which supply the brain lie in the fissures and are thereby protected from pressure ; but probably economy of space determines the balance between the dynamic forces which determine the growth of the skull and the growth of the brain, and by throwing into folds the grey matter, its area is increased enormously without increasing the size of the head. A very small head means a small brain and mental deficiency, but the simpler the convolutional pattern (that is, the fewer the folds and fissures) the less will be the extent of the grey matter and consequently the fewer the number of neurones. It is not surprising, therefore, to find that not only are the brains of idiots and imbeciles deficient in the relative proportion by weight of the cerebral hemispheres to the rest of the brain, but the convolutional pattern is simple, consequently the superficial area of cortex is diminished (vide Plate IV, figs. 3, 4). The degree of amentia or congenital absence of mind is proportional to the failure of superficial extent of the grey matter of the cortex— the anatomical basis of mind. Savage man has a superficial area three times that of the gorilla, but a microcephalic idiot's brain weighed only eight ounces (Plate IV, fig. 3). Not infrequently an idiot or imbecile has a large head caused by distension of the

cavities of the brain with fluid—hydrocephalus, popularly known as "water on the brain"—or there may be overgrowth of the connective tissue causing arrest of development of the nerve cells and fibres—the essential structures of mind.

Microscopic Examination of the Brain of the Child before Birth and after Birth, and What it Teaches.—There is no white matter in the cerebral hemispheres before birth because the myelin sheath of the nerve fibres has not been deposited around the axial processes of the afferent, efferent, and association fibres proceeding to and from the cortical grey matter. Appropriate staining of thin sections of the brain shows no evidence of myelin sheath formation. Now when the myelin sheath is formed an indication is afforded that a particular system of nerve fibres is capable of functioning by conducting nervous impulses. We shall see that this important fact has been made use of by Flechsig for showing certain fundamental principles connected with the development and correlation of structure and function in the growing infant's brain after birth. But before proceeding to discuss this I will consider the structure of the grey matter—the cortex—of the child's brain before birth. Examined microscopically, we see that it consists of six layers of cells, as the diagram of the adult brain shows, with individual differences in different parts; but these differences are not so marked as in the adult brain. In fact, Brodmann has shown from his studies of fœtal brains that the six-layer type is the characteristic type.

We also observe that the cells are very simple in their form and that they are closely packed together, forming columns and layers. They increase in size and they grow and develop by pushing out processes which extend like the branches of a tree (fig. 2). There are two types of neurone : the first type, the larger, in which a process of the cell called the axon leaves the grey matter; it becomes covered with myelin and forms a nerve fibre. In the other, the second type, the axon never leaves the grey matter. It is probable that these two different types of neurones have fundamental differences in function. The small second type is especially numerous, forming a dense layer in the sensory regions of the cortex of the brain. The sensory projection system of fibres conveying nerve currents from the muscles and special sense organs to the brain terminate in the layer of small neurones.

PLATE III
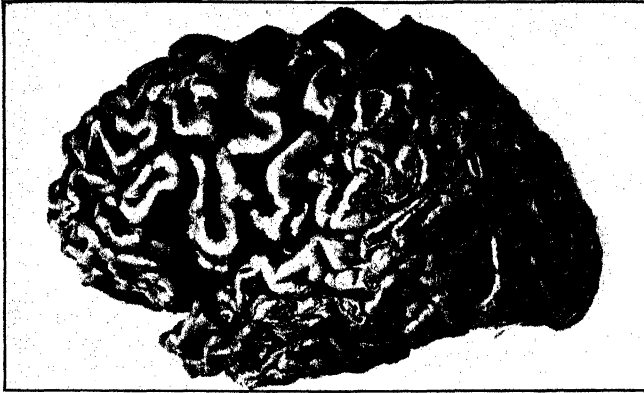
FIG. 1.—The left hemisphere of the brain of a chronic lunatic who has become grossly demented.

Observe the broad deep fissures caused by the wasting of the grey matter of the cortex, particularly of the frontal lobes. The convolutions are shrivelled, and a microscopic examination of them would show chiefly a destruction of the cells and fibres constituting their microscopic architecture.
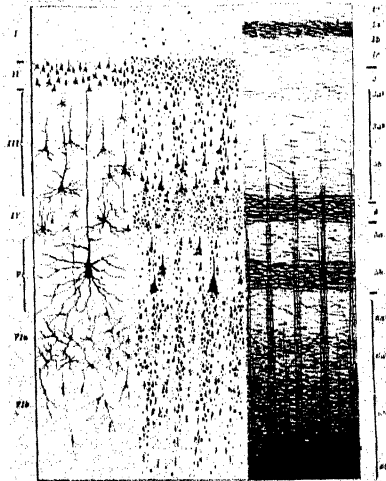
FIG 2.—Diagrammatic illustration after Brodmann of the cell and fibre architecture of the cerebral cortex.

There are six layers of cells and six layers of fibres. To the left are exhibited the different types of cells in the successive layers stained by the silver method, which only picks out a few cells. In the next column all the cells are stained by the Nisol method. Number IV layer consists of small granules, and above this are three layers of pyramids. Below the granules are larger pyramids in the layer V. Beneath this in the sixth layer are multiform cells. In the next column is represented the fibre structure; the vertical fibres are projection fibres carrying impulses afferent and efferent to and from the brain cortex. The layer of pyramids above the granules is especially connected with the function of associative memory. The horizontal systems of fibres are association systems.

300]

The new-born male brain weighs 321 grams; the female 361 grams. In the course of the first nine months the weight of the brain is doubled, and microscopic examination shows why this is. The myelin insulating material has been deposited around a large bulk of the axon processes of the neurones and the white matter has in consequence greatly increased. The neurones have not increased in numbers, they have increased in
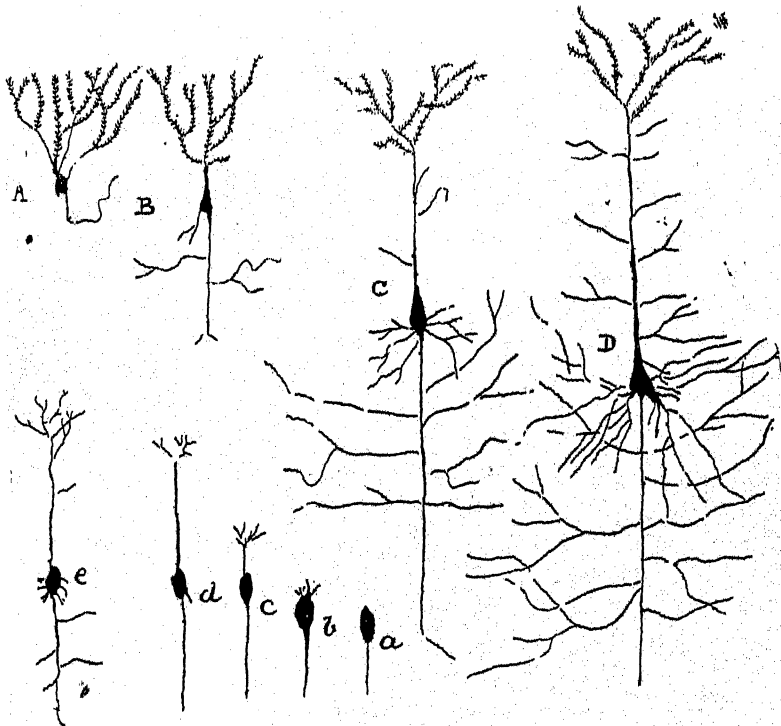
FIG. 2.—Diagram after Ramon y Cajal to show the phylogenetic and ontogenetic development of a psycho-motor neurone.

A, frog; B, newt; C, mouse; D, man. It will be noticed that in ascending the zoological scale there is an increase in complexity of the neurone and in the multitude of points of contact produced especially by an increase in the dendrons and dendrites, also but to a less degree by the collaterals of the axon. a, b, c, d, e, show the development of a psycho-motor cell in the human embyro as it grows. Neurones may be arrested in their growth, and in the brains of idiots an arrest takes place.

complexity and preparedness for function. The weight of the brain still continues to increase for the same reason, and in the course of the first three years the weight is treble that at birth. After this the addition to the brain weight gradually diminishes in amount and only slowly continues to increase in the male sex up to nineteen or twenty; in the female up to sixteen to

eighteen. After sixteen the increase in brain weight is very slight. In old age the brain tends to lose weight.

Myelination and Preparedness for Function.—Now let me call your attention to these diagrams after Flechsig (Plate IV, fig. 5); see, the dots on these two diagrams are situated around the primary fissures which physiological experiments and observations on the brains of human beings suffering from disease show to be the arrival and departure platforms of the sensory and motor impulses. The portion of the brain where voluntary motor impulses are generated for the control of movements of the opposite side of the body lies in front of the central fissure; behind the central fissure is the central station for the reception of impulses from the skin, muscles, joints, and tendons and the general organic sensibility of the body. The half-vision centre occupies the posterior part of the brain; only a small portion of this cortex is here seen because the greater portion is deeply situated in the floor and walls of the calcarine fissure on the mesial surface. The centre of hearing sounds received, especially in the opposite ear, is also in great part hidden from view, occupying the posterior part of the floor of the Sylvian fissure; likewise the cortex having for its function the sense of smell is almost completely hidden; the sense is shown as occupying a region at the tip of the temporal lobe.

The Association Centres.—The portions of the cortex indicated by dots situated around the primary fissures are, according to Flechsig, the arrival and departure stations for afferent and efferent stimuli. He terms them Projection Centres. But it will be observed that the greater part of the surface grey matter of the brain in Plate IV, fig. 5 shows no dots indicative of projection systems; these areas Flechsig terms the association centres; and although in man the afferent sensory and motor efferent projection centres occupy a larger surface area than in the highest anthropoid apes, it is especially the great development of the association centres which accounts for the fact that the cerebral cortex of a savage, even, is three times as extensive as that of the gorilla. Now how do we know by a study of the brain of the new-born child compared with the brain at later periods of growth that the projection systems are localised in the regions indicated? I have already told you that by appropriate staining the myelin sheaths of nerve fibres can be detected in microscopic sections of the brain. I have

said that the cerebral hemispheres at birth only show staining indicating preparedness for function in the base and stem of the great brain. The structures which are stained in Plate V, fig. 1 are the systems of neurones essential for the perform-ance of the complex, automatic, co-ordinate movements of the new-born child, viz. breathing, crying, sucking, swallowing. Occasionally anencephalous monsters are born in which this is the only portion of the brain present, the cerebral hemispheres being absent. Such monsters are capable of breathing, crying, sucking, and swallowing by the preorganised nervous mechanism in the stem of the great brain which is present in these creatures. The first appearance of myelin staining after birth is in the regions about the primary fissures—the sensory afferent pro-jection systems, the avenues of experience and intelligence; later the motor efferent projection system is myelinated. You observe that these several sensory perceptual centres of vision, hearing, smell, taste, and tactile-motor perception are independent. At this stage of development the child is capable of experiencing a simple elemental sensation, but later as the association neurones take on function as indicated by myelination of their fibres, the independent perceptor centres are physio-logically connected and functionally associated. That being the case the child is no longer capable of a simple sensation. You have only to watch an infant follow with its eyes a bright object; it makes very clumsy efforts at first, it does not recognise what the object is; but after a time and numbers of experiments it learns to stretch out its hand to get it, and if it succeeds it will take it to its mouth; nutrition is its object. If the spoon contains sugar the infant, having experienced the pleasure of sweet taste, at the sight of the spoon exhibits satisfaction and attempts to grasp it; this means that the visual centre has been associated with the motor centre and the successive movements it makes successfully to grasp the spoon cause sensory impulses from skin, muscles, tendons, and joints to be registered in the sensory tactile-motor sphere, so that after numerous experiences association for the eye and hand is effected. Suppose the infant is subsequently given a powder in the spoonful of sugar, the sense of taste and smell is excited and disgust produced, with signs of nausea, spitting out, and crying. A new experience has been made and the sight of the spoon, instead of awakening pleasurable feelings, will arouse

disgust and aversion by associative memory. As Galton in his inquiries into the human faculties truly remarks : " The furniture of a man's mind chiefly consists of his recollections and the bonds that unite them. As all this is the fruit of experience it must differ greatly in different minds according to their individual experiences."

A glance at this diagram of a section of the brain of a three months' child shows you that the whole of the white matter now contains myelinated fibres and all the primary projection centres are associated one with another (Plate V, fig. 2).

The Anatomical Substratum of Mind.—The proportional weight of the stem of the brain and cerebellum to the whole brain should be as 1 to 8. In the case of the idiot, the imbecile, and the dement the proportion is much lower, viz. 1 to 6 or even less. In the idiot and imbecile the superficial area of grey matter is greatly diminished; in the dement the grey matter is wasted and destroyed. Not only do we see these obvious defects, but if we compare the microscopic appearances of a section of the normal brain, stained so as to show the cell and fibre architecture, with a section of the brain of a congenital feeble-minded person and the sections of the brain of a lunatic who is demented or has lost his mind, we shall find in the case of the ament born with deficient mind a deficiency of cells and fibres in his cortex; the superficial pyramidal cells which give rise especially to the association fibres are poorly developed and deficient in numbers ; the cells have but few branching processes and are incomplete in their development, and there is not only, as I have said before, a parallelism between the diminished superficial extent of the cortical grey matter, but there is also a parallelism between the depth of the mental deficiency and the failure in numbers and development of the nerve cells and fibres. Correspondingly, in the loss of mind of a chronic lunatic there is a parallelism between the decay and atrophy of the cortical grey matter and the degree of dementia; the deeper the dementia (loss of mind), the greater are the number of nerve cells and fibres destroyed or undergoing decay and destruction (fig. 3). I think then I have shown you sufficient evidence to prove that the cortex cerebri is the material basis of mind.

Causes of Mental Failure.—We must recognise the two great groups of causes of mental deficiency or failure of the brain to develop : (1) Germinal or gametic, an inborn failure of the

PLATE IV

FIG. 1.—Left hemisphere of seven months' foetus, showing the primary fissures.
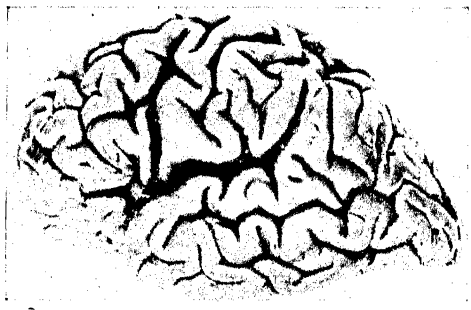
FIG. 4.—Left hemisphere of a low-grade imbecile; there is a great failure of development of the parietal lobe and the convolutional pattern is very simple.
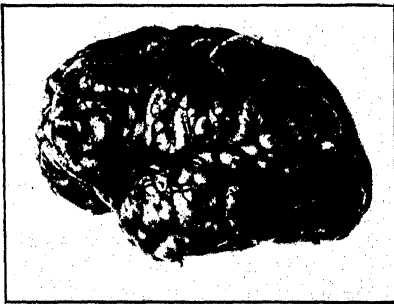
FIG. 2.—Left hemisphere of new-born child, full term.

FLECHSIG'S ASSOCIATION CENTRES

FIG. 5 is a diagram showing the projection and association centres of Flechsig as seen on the external and internal surfaces of the right hemisphere.

FIG. 3.—Brain of microcephalic idiot. Notice that the cerebellum is almost entirely uncovered.

germinal determinants of the cortical neurones, whereby the neuroblasts or primordial cells from which the neurones develop may, in consequence of an inherited defect, be deficient in numbers or deficient in specific energy, consequently they do not grow and develop. "Like tends to beget like," and the cause arises in most cases from defective progenitors. If one
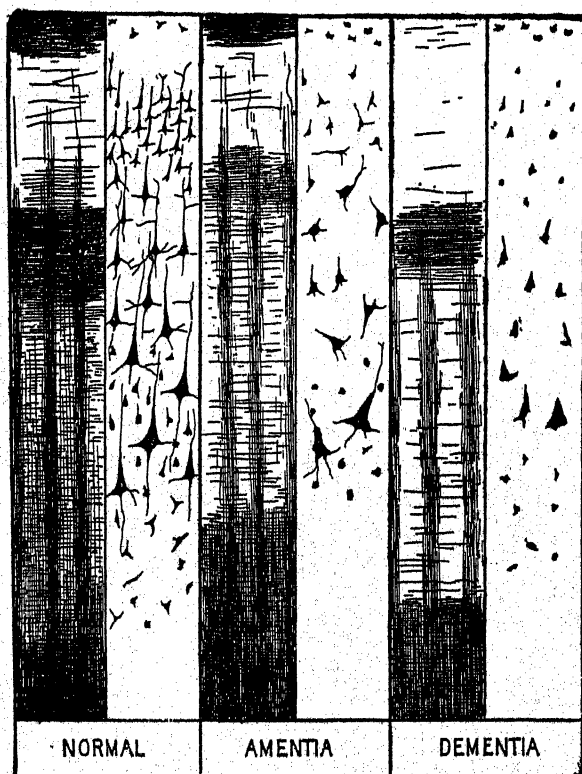
FIG. 3—Diagram to illustrate the comparative architecture of the cortex, of the healthy normal brain, of the brain of the feeble-minded (inborn amentia), and of the brain of the dement who has lost his mind.

Observe that the cells have lost their processes and are shrunken and irregular in form, also note the comparative poverty of fibres especially of the horizontal association fibres in Amentia and Dementia.

parent be feeble-minded, only some of the offspring will be mental defectives. If both are feeble-minded, the chances are the whole of the offspring may be more or less feeble-minded. It was calculated by the late Dr. Ashby, a very experienced children's physician, that 75 per cent. of the mental defectives

owe their mental deficiency to inborn germinal defect. Mentally defective children of this type may be born to normal parents, but the chances of such occurring are extraordinarily less than if a parent is feeble-minded, epileptic, or insane, or exhibits other signs of the neuropathic inheritance. (2) Mental deficiency from other causes occurs in 25 per cent. of the cases, and this includes pre-natal, natal, and early post-natal conditions. The pre-natal conditions are those associated with disease of the mother especially from such poisons as syphilis (giving rise to congenital syphilis), lead and alcohol, injuries, falls and depressing conditions by which the developing offspring is imperfectly nourished, and absence of the thyroid gland, which gives rise to myxœdematous cretinism. Natal or post-natal causes are difficult labour, fevers and poisoning in early infancy, which cause arrest of the development of the brain cortex; its damage may also be occasioned by rupture of blood vessels and tumours. It is extraordinary how well the brain is protected from injury and failing nutrition of the body. In starvation all the tissues of the body waste away, yet the brain loses hardly any weight at all. Donaldson at the Wistar Institute has clearly shown by a large number of experiments on white rats that the growth of the brain is hardly at all impaired by insufficient food. He took litters of white rats and divided them into two groups; one group he fed well, the other insufficiently. Although there was a great difference in the weight of the bodies of the two groups, the brains showed hardly any appreciable difference; proving that all the tissues of the body may suffer in order that the brain may grow. This shows that the neurones have normally a great inborn specific energy, as they should have, for they are perpetual cells of the greatest importance for the preservation of the commonweal of the social organism of the body. All the neurones are present at birth with all their latent potentialities; some are fully developed; the majority, especially the neurones of the grey matter of the surface of the brain, are in their infancy; those which in the process of evolution have been the latest to appear —the association neurones—will be the latest to complete their growth by extension of their processes. I have said these cells are perpetual cells; by this I mean that in a healthy brain they are endowed with a durability to function during the life of the individual. Unlike the cells of the body generally, neurones destroyed cannot be replaced. They are the master-cell-

elements for the preservation of the individual, as the repro-
ductive cells are the master cells in the preservation of the
species, and they are functionally interdependent.

LECTURE II

THE INBORN POTENTIALITY OF THE CHILD

BY the inborn potentiality of the child I do not mean altogether
what the child is born with, for it might be born with a disease
or defect which was really not inherited but due to injury or
disease acquired by the developing embryo before birth. Now
in order to make the distinction between hereditary conditions
and congenital conditions of the child quite clear to you, it is
necessary for me to explain some essential facts concerning
heredity.

All the broad facts concerning heredity were known to the
ancients, as is clearly shown by the poet and philosopher
Lucretius, who in *De Rerum Naturæ* says: "Sometimes, too,
the children may spring up like the grandfathers, and often
resemble the forms of their grandfathers' fathers, because the
parents often keep concealed in their bodies many first be-
ginnings mixed in many ways, which, first proceeding from
the original stock, one father hands down to the next father;
and then proceeding from these, Venus produces forms after
a manifold chance, and repeats not only the features, but the
voice and hair of the forefathers; and the female sex equally
springs from the father's, and males go forth equally from the
mother's body, since these distinctions no more proceed from
the fixed seed of one or other parent than our face and bodies
and limbs. Again, we perceive that the mind is begotten
along with the body and grows up together with it and grows
old along with it." It was the custom, you remember, of
noble Romans to carry in their triumphant processions the
masks of their ancestors; consequently many of these facts
became apparent to them.

Of the broad principles of human heredity we know very
little more than this ancient philosopher. Science, aided by
the microscope, has taught us much concerning the material

basis of inheritance; it has shown that plants and animals are reproduced on the same common plan of a dual inheritance from the male and female germs. Let us briefly consider the union of the male and female germs of fertilisation in the higher animals, for it will help you to understand some of the problems of inheritance.

The male germs are formed in countless millions in the male reproductive organs. The female germ-cells, ova or egg-shells, are contained in the ovaries; they are about 40,000 in number at birth, and the germ which constitutes the material basis of
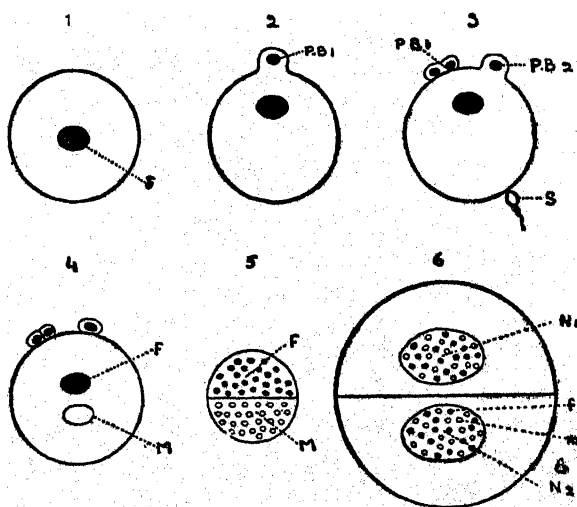
FIG. 4.

1) Diagram of egg-cell before ripening. (2) Maturation or ripening of the ovum casting out of half of the nucleus to form the first polar body. (3) Formation of second polar body and entry of spermatozoon (S) into egg. (4) Approximation of (M) male and (F) female germs. (5) Enlarged diagram of the two germs (F and M) before the first cleavage of the egg. (6) Enlarged diagram of egg after first cleavage. P. B1, first polar body; P. B2, second polar body; S. sperm; N1 and N2, nuclei of first two cells of the organism containing representative particles (germinal determinants) of (F) the female germ and (M) the male germ.

inheritance is a minute round body in the cell (fig. 4, F). When the ovum ripens (2, 3), which occurs periodically, one half of this germ is cast out of the cell. Why is this? It is to make way for a union with the incoming male germ, the bearer of the potential inheritance from the male, as the female germ is from the female. These two germs constitute the woof and the warp of the material basis of inheritance; while the male germ brings in a body called the centrosome, which acts as the shuttle which weaves the woof into the warp. The main substance of the

PLATE V
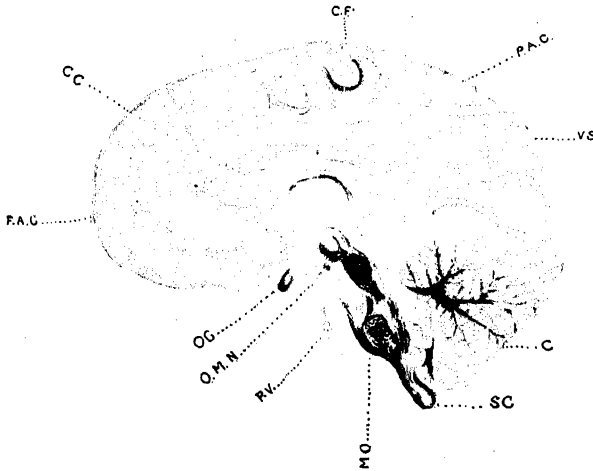
FIG. I.—Diagram of vertical section through the brain of a new-born child stained by the Weigut-Hæmatoxylon method to show myelination of the fibres.

All the parts which are dark contain myelinated fibres. Attention is particularly directed to the staining about C.F., the central fissure which corresponds to the tactile-motor area. It will be observed that the remainder of the cortex is unstained. M.O. medulla oblongata; P.V. pons varolii; O.M.N. oculo-motor nerve; O.C. optic commissure; F.A.C. frontal association centre; C.C. corpus callosum; C.F. central fissure; P.A.C. posterior association centre; V.S. visual sphere; C. cerebellum; S.C. spinal cord.
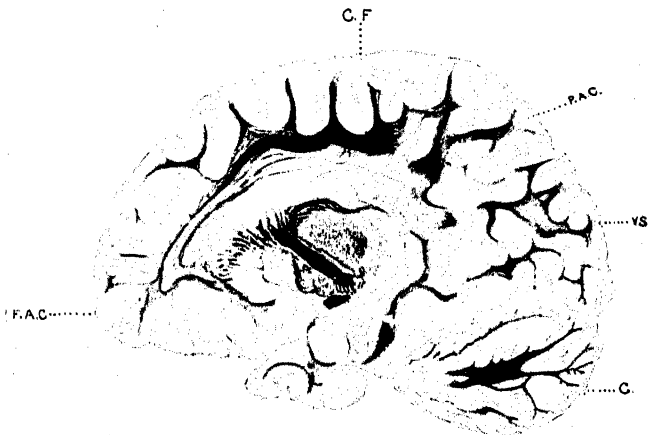
FIG. 2.—Diagram of vertical section of the brain of a child of five months.

The greater part of the brain now shows, by the staining, myelination of the white matter, thus indicating functional activity of the association centres. F.A.C. frontal association centre; C.F. central fissure; P.A.C. posterior association centre; V.S. visual sphere; C. cerebellum. It will also be noted the corona radiata and internal capsule which were not myelinated in fig. 1 are now myelinated, as shown by the staining in the basal ganglia.

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egg-cell surrounding the germinal substance or nucleus provides the material out of which fresh nuclear material is built until division of the nucleus occurs (6); the cell then divides, and the process is continually repeated. In the case of other eggs—*e.g.* that of the chicken, there is sufficient material to build up the young chick; in animals, however, the fertilised egg-cell receives its nutrition after a short time from the blood of the mother.

The reason why I have endeavoured, in simple language, to explain these facts to you is in order to make you better understand the essential biological fact of reproduction and how it is necessary to the perpetuation of the species; also to explain the differences between congenital disease and true hereditary disease. As soon as the fertilised ovum, which is to form first the embryo and then the child, is nourished by the blood of the mother, it is liable to be affected by poisoned states of her blood. The best example I can offer of this is syphilis affecting the maternal blood, whereby the embryo is killed or the child is born with congenital syphilis. But you may ask: Can the male germs be in no way affected by the fact that the man had had syphilis, or that he had been a chronic drunkard, or suffered with chronic lead poisoning? This is a crucial point in the study of heredity. "The neo-Lamarckian doctrine of the inheritance of acquired characters is a question of great social importance. It does not assert that a change produced in an individual by functional activity or external conditions is inherited at once and completely by that individual's offspring; but what the neo-Lamarckians mean is that when a certain functional activity produces a certain change in one generation, it will produce it more readily in the next and so on—until ultimately structural modifications will appear in the young even before the function which has produced them has commenced, and the process may go on indefinitely until the structural character in question will be inherited for many generations after the exercise of such a function has altogether ceased." (Cunningham.)

The majority of biologists deny the possibility of the transmission of an acquired character, and I would agree up to a certain point that there is no evidence or proof that an acquired character can be transmitted. That a father who drinks heavily and sees his wife and family starving transmits the *desire* to drink in his offspring is illogical and unproven; but he may

transmit that inborn character which will lead to his offspring drinking, viz. lack of moral sense and feeble will. You naturally ask: Are the Scriptures wrong in saying that "the sins of the fathers are visited upon the children even to the third and fourth generation"? and when I come to deal with the question of Insanity and how I believe Nature is continually striving to end or mend degenerate stocks you may ask: What then is the reservoir which is continually supplying degeneracy? Is it a continuous fresh generation of poor types consequent upon the pathological factors of modern social conditions, or is it that natural selection and survival of the fittest are less effectual in weeding out poor types? How far is medical science, legislation, and collective responsibility replacing family responsibility, thereby interfering with natural selection and survival of the fittest? Let us view the question from a physiological stand-point. I will take the male germs which are continually being produced in countless millions for the greater part of a man's life. Each germ is the bearer of an extraordinary specific potential energy; and it produces effects far more complex and wonderful than the emanations of a similar sized speck of radium. The reproductive organs that produce these germs are contained in the body and nourished by the same blood and lymph. Although physiology proves that Nature in a marvellous way has protected the brain, which is essential for the preservation of the individual, and the reproductive organs, which are essential for the preservation of the species, and has established, by subtle bio-chemical influences in the blood, a correlation of functions of the two, yet it is a fact that in prolonged conditions of poisoning of the blood the brain suffers permanently in the production of specific energy, as shown by failure of its higher functions, and the male germ cells, which are continually building up the male-germs out of constituents taken from the blood, may by analogy suffer in their specific energy and vitality. If this devitalising agency caused by a poisoned condition of the blood is carried on in several successive generations, and especially if reinforced by a similar loss of specific energy in the female germs from similar and other causes, weakly types of offspring will be produced, and these weakly types, being more susceptible to infective diseases, will be cut off early by invading microbes, especially by tuberculosis. But is the transmitted lack of vital energy

generally enough to account for mental degeneracy? Mental energy is mainly used up in the exercise of will-power and attention in acquiring knowledge and making new adaptations to environment and controlling and regulating the instincts and desires to the best advantage of the individual in the struggle for existence in the social life. Now a healthy mind can only exist in a healthy body, and the proper storage of mind-energy and its liberation, as well as recuperation necessary for a well-balanced mind, are largely dependent upon an inherited good and virile constitution: whereas the higher functions of the mind on the side of feeling, viz. imagination and the affective nature, are specifically inherited, and more dependent upon inborn variation from the normal average mind.

I have not time to discuss Galton's Law of Ancestral Inheritance nor Mendel's Law; I will only say in respect to Galton's Law that it only applies to the average inheritance of masses of people and not to the individual, and this was clearly recognised by Galton himself, for he says: "Though one half of every child may be said to be derived from either parent, yet he may receive a heritage from a distant progenitor that neither of his parents possessed as personal characteristics." Again, speaking of particulate inheritance he remarks: "All living beings are individuals in one aspect, composite in another. We seem to inherit, bit by bit, this element from one progenitor, that from another; in the process of transmission by inheritance, elements derived from the same ancestor are apt to appear in large groups, just as if they had clung together in the pre-embryonic stage, as perhaps they did." They form what is well expressed by the word "traits"—traits of feature and character. The offspring of parents possess a mosaic of inheritance bearing usually a more or less similarity, yet the mosaics of characters, whether bodily or mental, are not in any way identical except in the case of identical twins. Probably nothing has shown more conclusively the dominant influence of heredity on character than Galton's inquiries on the history of twins. He found that similar twins living in a different environment nevertheless remained similar in temperament and character, while dissimilar twins brought up and living in the same environment remained dissimilar. These dissimilar twins, however, were the product of two separate ova, whereas

identical or similar twins were the result of fertilisation of one ovum containing two germs of identical substance; which proves conclusively how untrue is the theory that all persons are born with equal mental capacities, the differences of development being due to education.

The Mendelian doctrine of heredity is proved as regards segregation of unit characters in the human subject; but even Bateson (the champion of Mendelism) does not claim that Mendelian proportions have been proved as regards human characters except in the case of eye-colour and certain abnormalities and defects. He himself admits that as regards mental characters the factorial analysis is so complex that proof is still wanting.

Primitive Emotions and Instincts independent of Education and Environment.—In considering the inborn potentiality of the child's mind, it is necessary to recognise that there is a pre-organised nervous mechanism in the brain and spinal cord which acts independently of education and social environment. This pre-organised nervous mechanism presides over the instincts and emotions essential for the preservation of the individual and of the species. The instincts are of the same nature in man as in animals, and the primitive emotions are similar in character but are of a lower order and incapable of developing into passions or sentiments; they differ in their mode of expression owing to the more refined nature of the human body and complexity of its movements. The desires, the associated instincts, the primitive emotions and passions are common to all human beings whether primitive savages or cultured races. They are best observed in children, savages, and feeble-minded adults in whom the highest control is either undeveloped or imperfectly developed. Whereas the individual experience of every other animal is almost entirely lost when it dies, man, by virtue of his acquirement of speech and the creative use of the hand in perpetuating his thoughts, feelings, and ideals, has slowly built up a great social heritage. The brain of the individual is the receptor of this social mind which printed language (especially) and other creations of man's hand have placed at the disposal of all mankind.

The Social Mind.—What would happen to the child if it were deprived of this social inheritance? It is said that one of the Pharaohs made the experiment of causing a child to be brought

up without its hearing any spoken language, in order to see what language it would speak. Hearing no language it spoke no language. Again, in 1840 a wild man was found in the forests in Germany; he spoke no language, but when brought to a town he learnt German.

Let us imagine for the sake then of explaining the important part played by this social heritage on the individual mind, what would happen if man were suddenly deprived of this heritage, which as Huxley says, has "placed him as upon a mountain top, far above the level of his humble fellows and transported his grosser nature by reflecting here and there a ray from the infinite source of truth." Supposing another flood came, and instead of Noah and his family having been preserved with the animals, only two infants (male and female) survived by some such agency as the mythical she-wolf that suckled Romulus and Remus, the founders of ancient Rome: and let us imagine that they grew up and became the progenitors of a new race. Deprived of a social heritage, they would have had to start building it up anew, but probably this would have taken countless ages, for there is no proof that the innate potential brain power of these two children of modern civilised man to create a social heritage would be immeasurably superior or even much superior to the reindeer men who lived in Europe and left their handwork in caves ages ago. According to Ray Lankester, these men had as largely developed brains as modern men. The man who made those drawings of deer with his rude instruments was a great artist, and the man who first discovered how to forge metal into an instrument for the use of the hand instead of a chipped flint was potentially as great a genius as Galileo or Newton.

The life of two such human beings without a social environment would at first depend almost entirely upon the fixed, stable, and preorganised characters of the species and sex, which would determine by an untaught aptitude the instinctive actions and behaviour necessary for the preservation of the individual and the species, with primitive emotional states of feeling and their special characteristic manifestations. Hence might be displayed fear and anger, joy and sorrow, wonder and surprise, play and self-display, curiosity, taste, and disgust.

In common with all human beings, including savages, our imagined pair would exhibit not only the primitive emotions,

but sentiments and passions in their elemental form, such as love and hatred, pride and contempt, suspicion, vengeance, grief, and despair, displayed by attitude, gesture, and facial expression, accompanied by the utterance of inarticulate vocal sounds, by crying and laughing, and signs of pain and pleasure. Such expressions of the feelings constitute a universal language understood by all human beings, because common to all human beings.

At the proper season, an attraction of the two sexes necessary for the preservation of the species would occur, for this sexual attraction which we term love possesses a universal language. In the normal conditions of life it is both a physiological and psychological process; it is the fountain head of the emotions and passions, stronger even than the fear of death. Love, though mute, speaks more eloquently by signs than any spoken language.

Next, the maternal instinct. What is stronger and appeals more forcibly to our highest ideals than the tender emotion of the mother for her child and the devoted sacrifices she will make for its preservation? Yet do we not find this common to all the higher animals? Indeed, we can see that the moral sense, consisting in the highest altruistic feelings and sentiments, has its roots in these two physiological instincts; for when pure and undefiled there is nothing more noble and ennobling than love and parentage. We must therefore regard the sentiments as having an evolutional biological basis founded on the preservation of the individual and the species.

The inborn raw material of character is a complex dependent upon species, sex, racial and family ancestors; it is therefore apparent that the inborn physiological characters of the species and sex are fixed and stable; they are the stem of the tree of life, on which has been grafted the characters of race and family progenitors, these being of later evolution, and more capable of variation and mutation.

The future of the race, born of these two hypothetical children, would depend upon whether they were well-born—and by well-born I do not necessarily mean of wealthy or aristocratic parents, but of parents possessed of healthy minds in healthy bodies, coming from good stocks of broad-chested sires and deep-bosomed mothers; endowed with courage, honesty, and common-sense, which is the inborn aptitude of

profiting by experience to do the right thing at the right moment. With such a heritage these two human beings, with the instincts for the preservation of the individual and the species, would possess as inborn qualities tendencies which would be productive of a virile stock endowed with superior energy, sagacity, and racial temperament, thus enabling their descendants to have a great advantage over primitive races possessed of a language and a limited social heritage. There might be an inborn tendency to artistic feeling and expression, derived from progenitors, which under favourable conditions would find expression. There might be an inborn tendency to the instinct of curiosity which would lead them to observe and reason on natural phenomena, and thereby learn to obtain fire and to make rude weapons. If their parents were right-handed, as in all probability they were, they would use the right hand in preference; that is to say, the left half of the brain would be the active partner, and predominate in voluntary movements of the hand as an instrument of the mind.

It would be safe to assume that prior to the acquisition of articulate speech and language this new race of beings would at first only be able to communicate with one another by gesture language; then some creative mind would employ articulate sounds to supplement the primitive gesture language as a means of communicating ideas, and correspondingly would arise the dawn of intellectual development and abstract thought and reasoning, because thought in all the higher mental processes cannot be carried on without the aid of language. Then, as language by graphic signs and articulate speech progressed together, simultaneously supporting each other in the development of the higher mental faculties that differentiate the brute from the savage and the savage from the civilised human being, so the social heritage—the Universal Mind—would expand and increase. Man, instead of thinking by associating concrete images, would now carry on the processes of thought and memory by means of words heard and seen (symbols), in the form of spoken, written, and printed language.

How great a part language has played in the development of the mind can be gathered by a little consideration of the fact that individual human experience would be almost entirely lost by the cessation of every individual life, without language. Moreover, completely developed languages, when studied from

the point of view of their evolution, show that they are stamped with the print of unconscious labour that has been fashioning them in the long 'procession of ages. Reflection upon new words coined in our own time proves that the evolution of language exhibits an abstract and brief chronicle of the history and progress of the race, and it constitutes the Social Mind, embodying the record of past experience which each later individual of the race can utilise through his senses and his brain. We know that the offspring from a savage tribe in Africa, brought up among cultured people, can, by imitation, through his senses utilise this social heritage; he fails, however, individually and collectively, to initiate new ideas and to *add* to the social inheritance of mankind. The millions of negroes in America have added little or nothing to the sum of human knowledge since their emancipation from slavery.

The Brain a Transformer and Accumulator of Neural Energy from Cosmic Energy.—You may ask: Will not the brain be affected in its growth by deprivation of the stimulus of the social heritage? There are certain facts which point to its not being affected in its growth and structural development. First of all we must look upon the whole nervous system, and particularly the brain which forms the greater part of its bulk, as possessing the function of transforming cosmic energy into neural energy and storing it up as nerve potential. This function would not suffer in the least by the deprivation of the social heritage built up by language. Moreover, the fact that the wild man found in the forest in Germany was able to learn German shows that the latent capacity was there in spite of the fact that he had never since childhood heard spoken language. When I speak of the transformation of cosmic into neural energy I mean that a nerve current is a specific molecular vibration travelling along the nerve at the rate of about 30 yards a second; it is not therefore an electrical current although it produces an electrical disturbance in the tissue involved. The effect on the mind produced by an external stimulus we say is due to the nature of the stimulus; that is true, but it is also due to the specific function of the neural systems of peripheral receptor, transmitter, and central perceptor in the brain. For the same stimulus will give rise to different sensations according to the different special sense organs stimulated. Thus if an interrupted electrical current be applied to the tongue so as to stimulate the gustatory nerve,

taste is experienced; if the eye or optic nerve, a bright light; and the auditory nerve excited gives rise to the sensation of sound; and the skin, a sensation of painful vibration. Each neural system then has a specific energy of its own to transform this electrical energy into specific neural energy and to store up memories of the same in the brain.

The Temperament—A Complex of Characters derived from Species, Sex, Race, and Progenitors.—It is obvious that the fixed characters of species and sex form an important basis of the inborn potentialities of the mind of the child; they are dependent upon preorganised nervous mechanisms; in addition to these which are similar in all human beings, we have other potentialities due to race. I need not tell you that just as there are inborn structural characters of the body, including the brain peculiar to different races, so there are temperamental characteristics, and these inborn racial temperamental qualities play an important part in the formation of the raw material of character, which is a complex derived from species, sex, race, and progenitors. We are all familiar with the quick perceptive emotional temperament of the Celts, and both history and biography teach us the success that has attended the blending of the Irish, Celtic, and Anglo-Saxon temperaments in the production of great generals and statesmen.

As Pathologist to the London County Asylums I have been for a long time engaged in studying the effects of family inheritance in relation to disorders and diseases of the organ of mind, and with this part of the subject I will next deal.

Ancestral Inheritance in relation to the Inborn Potentialities of the Child's Brain.—I pointed out to you in my last lecture that the convolutional pattern of the brain—the organ of mind—is no haphazard affair, but is dependent upon the inheritance of similar folds and fissures from progenitors; just as we know that in every face are the features of ancestors, so in every character may be the character of ancestors. Galton's statistical inquiry into the inheritance of good and bad tempers showed that one set of influences tends to mix good and bad tempers in a family at haphazard; another tends to assimilate them, or that they should all be good or all be bad; a third set tends to divide families into contracted portions. This pedigree (fig. 5) shows in the third generation a sorting out or segregation of good and bad tempers according as the children resembled the father and mother

respectively. No child is born insane, but it may be born with an insane or neuropathic tendency; certainly it may be born mentally deficient owing to failure of development or arrest of growth of the grey matter of the brain. Such mental defectives are low-grade imbeciles and idiots, in whom in my last lecture I demonstrated a correlation of deficiency of mind and the material
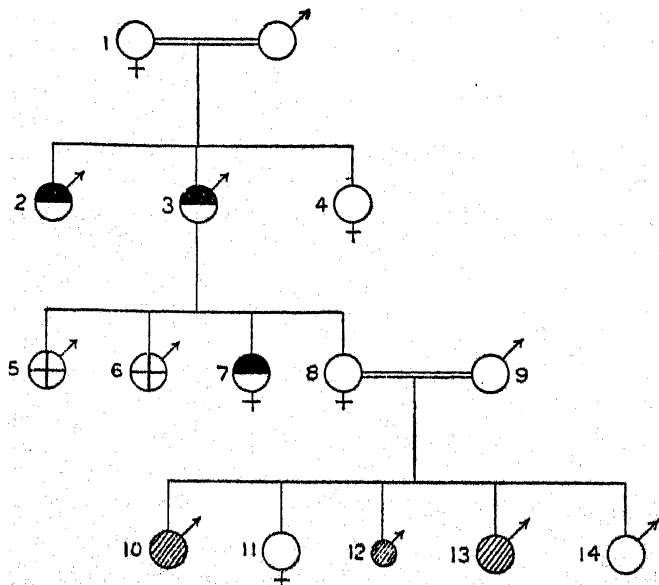
FIG. 5.—The above pedigree shows the transmission of insanity, immorality, and violent temper.

No. 1, the grandmother, was immoral. Of her children, No. 2, an engine-driver, was "a man of violent temper who smashed things on a wholesale scale at home. He died with the delusion that he was going to heaven on the footplate of an engine." No. 3 was also a man with a violent temper, dangerous to himself and others, who eventually died from general paralysis. The daughter, No. 4, was criminally immoral; she had an illegitimate child, but no children by her marriage. The children of No. 3 are as follows: Nos. 5 and 6, both men with violent tempers, drunken and immoral; No. 7, a daughter, criminally immoral, who eventually was detained in Bethlem for a period. No. 8 is a woman with a very violent temper, smashes things, and has attacked her husband with a poker, etc.; has tried to commit suicide by poison and once by hanging; gushes to every man, but repels her husband. The husband asks, "Is she mad, or bad, or both?" The husband is a healthy, robust man, who comes from a good healthy stock. The children were five in number; two survive (Nos. 11 and 14), and these fortunately resemble the father; they are healthy, robust and energetic. The first-born, No. 10, was a boy resembling his mother; he was nervous, reserved, lacked mental energy, and was prone to somnambulism and night-terrors, which existed in his mother's family; he died under an operation at the age of 12. No. 12 was the image of his father, but died from measles when 10 months old. No. 13 was nervous and resembled his mother; at 19 months he died from whooping-cough.

basis of mind—the grey matter of the brain. But the higher-grade imbecile, the epileptic, and the insane adolescent do not usually show sufficient obvious defects of structure (even by the aid of the microscope) to satisfactorily account for the mental disorder, but this may well be because methods have not yet

been devised to exhibit the bio-physical and bio-chemical con-
ditions underlying normal physiological processes in the organ
of mind; and until we have some conception of this we cannot
explain such abnormal temperamental and disordered mental
conditions due to functional derangement of the complex
mechanism of mind.

We know, however, that "like tends to beget like," and
everybody recognises the potential value to the individual of
coming from a mentally sound and good stock.

The inborn mental potentiality of the child may be sound,
partially sound, or unstable or totally unsound. A careful
inquiry into the family histories of the progenitors and the
collateral members of the ancestral stocks will in the great
majority of cases show that a child born sound in mind and body
is begotten by parents sound in mind and body themselves,
whose stocks are free from any neuropathic or physical taint.
Such a child with a good inheritance is very unlikely to suffer in
later life with feeble-mindedness, epilepsy, insanity, or functional
nervous disease. Occasionally, however, from some inexplicable
cause parents of sound stocks may beget an idiot or imbecile
child, or a child who in later life becomes insane or epileptic.
But every effect owns a cause; although we may not have
discovered it, and it is unscientific to speak of it as a sport. It
may occur as a result of a latent morbid tendency in the germ
plasm of the two stocks, as we know frequently happens in
consanguineous marriages, when both stocks are *apparently*
healthy, yet one or more of the offspring are mentally or physic-
ally unsound. A partially sound or unstable inborn mental
constitution is usually inherited, and careful inquiry generally
shows one of the parents or some other member of the parental
stocks to have been mentally unsound or unstable. The child
may give evidence of mental defect by being dull and backward
in learning, or it may exhibit fits of uncontrollable temper with-
out cause, or other signs of nervous irritability such as convulsive
attacks which may be precursors of true epilepsy. If the child
escapes any distinct morbid manifestation during childhood,
there is a danger of its showing vicious tendencies later, or
developing insanity or epilepsy at the period of adolescence
when the sexual instinct is aroused and new desires and passions
stimulate the brain to a new activity. It seems that a mental
breakdown is also liable to occur in such individuals from

repression of the sexual passions and emotions producing mental pain and stress causing exhaustion of neural energy. The more evidence of degeneracy there is in the progenitors and their stocks, the greater will be the number of children born suffering with feeble-mindedness, epilepsy, criminality, or insanity. If both parents are feeble-minded, or one feeble-minded and the other epileptic, the chances are that all the offspring will be feeble-minded or epileptic. No good can come from a stock in which there is mental deficiency; it is otherwise in the case of mental instability, for that very instability which leads to a mutation from the "honourable ordinary" may lead to the genesis of constructive imagination and a temper which, disregarding moral traditions and social usages, is often found associated with genius. History and biography proclaim that the genius of imagination of the poet, of the prophet, of the artist, of the philosopher, and the lust for action of the world's great leaders of men have been so frequently associated directly or indirectly with epilepsy, insanity, or a neuropathic tendency that Dryden's lines have become a recognised truism :

> "Great wit to madness sure is near allied,
> And thin partitions do their walls divide."

Still, if a nation (in order to progress) must have an admixture of mental instability in the form of genius and insanity, a streak of it is sufficient; for that nation will be the most virile which can breed from the greatest number of the "honourable ordinaries" endowed with the attributes of civic worth, courage, honesty, and common sense. Moreover, it is a great mistake to suppose that a stock that does not show pathological mental instability in the form of epilepsy or madness cannot therefore produce genius. One of the striking instances of hereditary genius is the Bach family. In his work on hereditary genius Galton did not refer to his own remarkable family, but I will throw on the screen the abridged pedigree of the Darwin-Galton-Wedgwood family, and it is of interest here to remark that Erasmus Darwin anticipated many of the theories of evolution and heredity subsequently elaborated and demonstrated by his illustrious grandsons Charles Darwin and Francis Galton. Genius often springs up in a stock we know not how or why, and with meteor-like flash it disappears. How far the epoch makes a man of genius, or the man of genius makes the epoch, it is difficult to say.

Dr. Maudsley has remarked that many a Napoleon has died an inglorious death upon the scaffold. Genius belongs to no social order or class, nor can we explain in the majority of cases whence it comes. The part that chance plays by a happy and harmonious combination of germs in the production of genius is shown by the fact that the most outstanding figure of the Renaissance period—Leonardo da Vinci (1452-1516)—sculptor, painter, architect, engineer, musician, philosopher, and universal genius, was the illegitimate son of a Florentine lawyer by a peasant woman. There was nothing in the history of the Da Vinci family to suggest constructive imagination; several generations of lawyers of no remarkable note was the only family history pointing to intellectual ability. Moreover, the father of Leonardo had a large family born to him in wedlock; he was married to four women, the last two gave birth to nine sons and two daughters. He had but one illegitimate child by the peasant woman, who subsequently married and had a family, none of whom attained any fame. The wonderful child, as remarkable for its beauty and strength as in its early manifestations of supreme mental endowments, was fortunately for posterity cherished by its father, who spared no opportunity which nurture and education could provide to develop this marvellous product of Nature. Would Leonardo have been what he was, had he not been born in the Renaissance period and had his wonderful talents developed by education? I could cite numbers of other illustrious men whose forbears had given no evidence of especial genius or talent, and who attained an everlasting place on the scroll of fame. Isaac Newton was the son of a small farmer proprietor of Cleethorpes; Michael Faraday the son of a blacksmith; Dalton, the son of a weaver; Turner the painter the son of a barber whose mother became insane, and from whom he probably inherited his eccentricity and imaginative genius. It is a probable fact that great men owe their genius in a great number of instances to their mother in whom it is latent. Abraham Lincoln himself said, "All I have and all I hope for I owe to my angel mother," and Goëthe poetically described his dual inheritance of body and mind in the following lines:

Vom Vater hab ich die Statur,
Des Ernstes Lebens führen,
Vom Mütterchen die Frohnatur,
Und Lust zu fabulieren,

which freely translated means he resembled his father in stature
and energy and his mother in his poetic imagination; yet his
son had none of his father's genius and is spoken of as the son
of the maid-servant.   The greatest and best of all the Roman
Emperors, Marcus Aurelius, says, "To the gods I am indebted
for having good grandparents, good parents, a good sister, good
teachers, good associates, good kinsmen and friends; nearly
everything good." Yet this man who practised the noble
precepts he taught begot the infamous Commodus, one of the
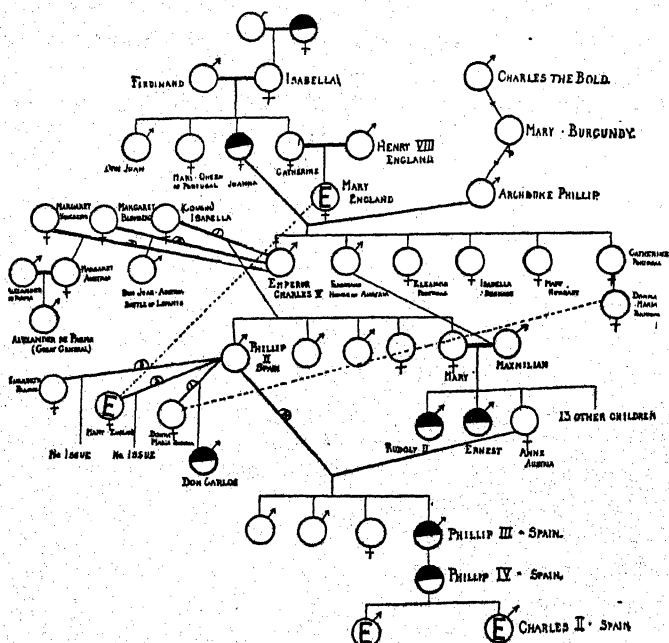
FIG. 6.

worst of the Roman Emperors. That Commodus was the son
of Marcus Aurelius is shown by their physical resemblance,
and not the son of a gladiator, as some have asserted, by the
licentious Faustina the Empress. As it is stated that in spite
of careful bringing up he early evinced depraved tastes, it is
probable that he inherited his temperament from his mother, as
he certainly did his bodily form from his father.

Perhaps one of the most striking facts of heredity in history
is the Spanish Succession, of which I will show an illustrative
pedigree on the screen (fig. 6). It shows an hereditary neuropathic

taint following a family for 350 years, and as Ireland in his work *A Blot on the Brain* says : "Sometimes passing over a generation and appearing in various forms and intensities as epilepsy, hypochondria, melancholia, mania, and imbecility till at length it extinguished the direct royal line of Spain." The tendency in the blood was, as you see, reinforced by close intermarriages with families of the same stock, and it is worthy of notice that the house of Austria, with which the Spanish line was so often connected by marriage, had few members insane, and in the end threw off the hereditary curse. "Such vigour as was in the first Spanish kings appeared in their illegitimate descendants, whereas those born in wedlock inherited the disease in spite of the known ancestral taint. A match with Spain was much coveted by the royal families of Europe; as an example we may recall the silly eagerness shown by James I. of England to marry his son Charles with the Infanta Maria. Whoever attends closely to history must know that there is a great deal in birth, but not birth fixed by laws and traced by heralds. A man who is well-made, strong, mentally gifted, and able to do much work and stand much strain must be well born, and a race sodden with epilepsy and insanity and scrofula, whatever its fictitious rank, is necessarily low born and in reality not worth preserving." I have already given you many facts which certainly show that the raw material of character which may be good, bad, or indifferent is inherited ; just as some children are born weak and others strong, some energetic and others inherently lazy. It is an undoubted fact that the foundations of moral characters are inborn, but the influence of education, example, environment, and nutrition is more potent for good or evil than is the case in morphological characters.

Finally, remember the words of Sir Thomas Browne : "Bless not thyself that thou wert born in Athens but among thy multiplied acknowledgments ; lift up one hand to heaven that thou wert born of honest parents, that modesty, veracity, and humility lay in *the same egg*, and came into the world with thee."

In the next number the third lecture will be given, which will deal with "The Influence of Nutrition and the Influence of Education in Mental Development."

THE INTERPRETATION OF FACT IN THE STUDY OF HEREDITY

By CHARLES WALKER, D.Sc.

Heredity in Relation to Eugenics, by Charles Benedict Davenport. (London : Williams & Norgate, 1912.)—*Problems of Life and Reproduction,* by Marcus Hartog. (London : John Murray, 1913.)—*Heredity,* by J. Arthur Thomson. 2nd edition. (London : John Murray, 1912.)—" The Logic of Darwinism," by Archer Wilde. (SCIENCE PROGRESS, April, 1913.)

THERE is, I should imagine, no branch of knowledge in which the intelligent reader is more likely to be misled than that which we know as " heredity." In no other subject are there greater divergences of opinion upon fundamental points among recognised exponents, nor have differences of opinion in any case been expressed with greater fanaticism and disregard or misrepresentation of the arguments and facts advanced by opponents. I do not mean to imply that all exponents of views upon heredity are guilty, but that such offences are very common.

The study of heredity involves so many branches of knowledge that it is not surprising that students in one branch often fail to understand what those in another mean, owing to the very different character and bearing of the facts dealt with. The violent controversy between the Mendelians and Biometricians is a case in point. To put it broadly, the Mendelians are dealing with the individual, while the Biometricians are dealing with the race. The Mendelians record facts connected with the transmission of particular and chosen characters which are easily observed, from individual to individual; they show how these particular characters behave in the offspring when individuals differing with regard to them are crossed. The Biometricians, on the other hand, deal with the behaviour of chosen characters in a large number of individuals in successive generations. They show to what extent, on the average, the characters of the parents are inherited by the offspring and how the average standard of a character may vary in a race. I do not see the slightest reason to question the facts put forward by either

party, nor do I see that the facts contradict each other in any way. The mode of transmission of characters from individual to individual is quite a different matter from that of recording the average standard of any given character in successive generations of a large number of individuals.

Where the real difficulty to the outsider interested in heredity comes in is that the Mendelians treat all characters as unit characters which do not blend at all in the offspring. A father with a certain definite character has offspring by a mother who has the opposite (the allelomorph) of this character, including in opposite the presence or absence of a character. The immediate offspring will show one or the other of this pair of characters; in the next generation individuals will appear in which one or the other character will be produced to the exclusion of the opposite, and in these the characters extracted from the cross will behave more or less as pure characters and breed true. This is Mendelian or alternative inheritance. Prof. Davenport in his very valuable book practically ignores any other kind of inheritance, the result being that the uninformed reader must believe that all characters are inherited in this alternative manner. This is strange, as he wrote in 1906: "Very frequently if not always the character that has once been crossed has been affected by its opposite with which it was mated and whose place it has taken in the hybrid. It may be extracted therefrom to use in a new combination, but it will be found altered. This we have seen to be true for almost every character sufficiently studied. . . . Everywhere unit characters are changed by hybridism."[1] There is, of course, not the slightest doubt that many characters present in the parent appear in succeeding generations of offspring in an alternative manner, but is this true of all characters? And if it be not, is there anything which suggests which characters are transmitted in this way? Prof. J. A. Thomson gives but little help in this direction. In an admirable account of the Mendelian theory and experiments, he appears to agree with its most bigoted supporters. He gives also an admirable account of the theories and observations which are supposed by some to contradict the Mendelians, and he appears to agree with those who uphold them.

Now it is quite obvious that the bulk of the characters in any individual are not inherited in an alternative manner. Whether

[1] *Inheritance in Poultry*, p. 80.

they originated in the remote past from characters that were Mendelian is beside the question ; they certainly are not so now. In following the behaviour of what are really small, more or less individual differences, the Mendelian school have apparently so lost sight of the bulk of the characters in the organisms they have studied, that these comparatively slight differences are treated by them as though they were the only characters that exist. A very little consideration will show what a mistake this is. Take the whole of the characters of man. I will not trouble to deal even briefly with those which he possesses in common with other animals lower in the scale than mammals, though they are numerous enough to fill volumes. Among the characters possessed by man in common with all other mammals but not by other vertebrates are the special modification which provides for the feeding of the young after birth ; hairs upon the skin ; sweat and sebaceous glands ; a peculiar formation of the skull, skeleton generally, and brain ; a particular form of red blood corpuscle ; and the separation of the body cavity into two large compartments by the diaphragm which provides an addition to the breathing mechanism not found in other animals. I must pass on to the nearest relations of man, the existing higher apes. When we consider the characters common to man and the chimpanzee or gorilla, we find that the resemblances extend to the bulk of even minute details. Compared with the points of resemblance the points of difference are small and very few. The differences between the different races of men are smaller and fewer. To me, therefore, it appears perfectly clear that the overwhelming bulk of the characters inherited by each individual is derived from very remote and prehuman ancestors. The differences which constitute the characters studied by the Mendelians are almost as nothing when considered in relation to the characters which are common to all the members of the race. But these characters common to all individuals obviously cannot be transmitted alternatively. They are always present. It is therefore evident that the characters that are inherited in the Mendelian manner are really slight additions to or subtractions from characters already present. If we choose even the largest of such differences, albinism for instance, it is clear that this is comparatively a small difference. Pigment is not entirely absent from the organism, it is absent only from certain parts and in most cases is not quite absent even from them.

To realise what is happening, it is necessary to appreciate a certain property of living matter, a property which is absolutely universal throughout the animal and vegetable kingdom from amœba to man, from algæ and the like to the most highly differentiated plants. This is the property of variation. No two organisms or parts of organisms are ever exactly alike. Living organisms consist of single cells or of groups of cells living together. No two cells are ever exactly alike. When I realise that every biologist believes in evolution of some kind through some process of selection—and they all appear to realise that variations in the offspring are necessary to evolution—I marvel at the fact that so many theories exist to account for the production of these variations during the later stages of evolution. The variations must have been present from the very first stage, otherwise evolution would obviously have been impossible. A loss of the property of varying by the cells forming any organism would of necessity have meant that evolution and the appearance of new, and the increase or diminution and disappearance of existing, characters would have ceased. But actual observation shows that in no type of cell has variation ceased. Examine the cells forming the most highly differentiated tissues of the most highly differentiated organism and you will never find two cells exactly alike. This being the case, it must be perfectly obvious that the organisms built up from these cells can never be exactly alike. Offspring must always vary from their parents and offspring of the same parents from each other. Sometimes the differences are considerable, sometimes small. Obviously when minute organisms with which the observer is not very familiar are examined, these small differences will escape his notice. Familiarity is a great factor. To the white man all negroes appear alike, but when he has lived among them for some years he sees as much difference between them as between his fellow white men. In the case of microscopic animals and plants, small differences are even more likely to escape notice, but a careful examination by a skilled observer shows that they are always there.[1] Naturally if the environment of an organism remain unchanged for a long period of time, any variations which tend to interfere with adaptation will be

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[1] I have dealt with many cases in which variation has been claimed as absent in *Hereditary Characters* (Arnold, London, 1910).

eliminated. Thus we may find some cases in which the characters of organisms have not changed materially during geological epochs of time. Any considerable variation would have been disadvantageous and so must have been eliminated. Such cases are, however, as would be expected, comparatively rare and occur chiefly among stationary or slowly moving organisms. For the origin of this property of varying we must therefore look back to the origin of life itself, and it seems a work of supererogation to invent theories as to the causes of variations during the later stages of evolution and to treat them as though they had not been there all along.

But there is one point about the variability of living organisms which I do not think has received much attention, and that is that it must obviously be the object of selection just as much as any other character. Selection must increase the variability among the individuals of a race just as it must affect the length of a tail or the shape of a head. I shall have more to say of this later.

Prof. Thomson gives a number of theories as to the causes of variation during the advanced stages of evolution, but he assumes that in many cases variability does not already exist. In explanation of this he says : " The cell which in the embryo begins the germ-cell lineage may be identical with the fertilised ovum, and the complete heritage may be continued intact through successive cell divisions until the next generation is started and the process begins anew. The completeness of hereditary resemblances depends, in Bateson's phrase, on 'that qualitative symmetry characteristic of all non-differentiating cell divisions.'" To me this appears to be a most unwarrantable assumption. I have examined hundreds of thousands of germ-cells destined to produce ova or sperms and I have never seen two exactly alike even from the same individual ; no one among the hundreds who have made similar observations has ever done so either. Profs. Thomson and Bateson must realise this themselves after due consideration. Furthermore, the fertilised ovum cannot possibly be identical with each of the germ cells which goes to form it. " That qualitative symmetry characteristic of all non-differentiating cell divisions " means no more in relation to Prof. Thomson's " completeness of hereditary resemblance (*i.e.* the absence of variation)" than that cells tend to produce cells more like

themselves and like each other, than like any other kind of cell. It can easily be demonstrated that there is no such thing as absence of variation in any living organisms; therefore, why trouble to evolve hypotheses which are quite unnecessary?

I turn to Prof. Hartog and find that he attributes the origin of variation to the inheritance of acquired characters. But I find also that he has realised that the inheritance of mutilations cannot occur, for "any tendency to transmit such deficiencies would in course of time result in a generation of formless imperfections that must needs be eliminated by natural selection." It is therefore evident that he believes that, if the tendency to inherit particular acquirements made through the action of the environment be injurious, the tendency will disappear. But a very large proportion of the effects of every environment is injurious to the organism. Certainly we find that the organism has, as a rule, the power of reacting to these injurious factors and surviving in spite of them; but they must always do some harm to the individual, as in the case of the children described by Galton,[1] who invariably showed an arrest of growth during even slight illnesses. We have ample material in the innate variability of living matter without assuming the transmission of the effect of the environment from parent to offspring; the advantages of germ cells which do not transmit such acquirements are obviously so great that they must have come under the action of selection and any tendency to transmit acquirements been eliminated. Prof. Hartog frequently expresses his disapproval of unnecessary assumptions, theories, and hypotheses. I entirely agree with him, and as the fact that cells never produce other cells exactly like themselves or like each other seems ample to account for every diverse organism that exists or has existed, I think his theory "falls under the ever trenchant blade of Occam's razor."

Of the whole stock of characters present in an individual then, the great bulk have been derived from remote ancestors. This stock is constantly being varied by what are comparatively small additions and subtractions. Some of these are variations of the individual organisms: its private property, so to speak. They may be transmitted with increases or diminutions to the offspring. Thus it becomes evident that a number of these minor characters are inherited from near ancestors. Besides

[1] *Inquiries into Human Faculty.*

the number of great characters common to all the individuals of the race, each individual therefore shows a number of differences in these characters which are common to a section of the race but not to the whole race; a smaller number of smaller differences which are common to a smaller number of individuals; and so on to those differences which are peculiar to himself alone.

As these considerations lead me to believe that but comparatively few characters are transmitted from parent to offspring in the Mendelian manner, so I am convinced that Galton's law of Ancestral Inheritance can only be applied, even in its broadest and most "averaging" sense, to precisely the same group of characters. The overwhelming bulk of our characters come equally *through*, not *from*, both parents. Half of them certainly do not come from each. On the other hand, it does not seem improbable that, on the average in a large number of individuals, small differences may be inherited approximately half from each parent, a quarter from each grandparent and so on. It cannot quite work out at this rate, however, for each individual in the ancestry makes some addition to or subtraction from what he or she inherited from the parents. The individual contributes his own variations. The "half" contributed by each parent is made up of two "quarters" contributed by each grandparent, *plus* the variations of the parents. Without this, evolution would have been impossible.

I have elsewhere put forward the view that the characters that are transmitted in the Mendelian or alternative manner are those which have comparatively recently arisen as variations in individuals.[1] Those that have become so established as to be common to all the individuals of a race do not behave as Mendelian characters when crossed. To make my meaning clear it is here necessary to deal with some features of the Mendelian experiments. One of the most important of these is, that the overwhelming majority of them have been made with domesticated races. Here I must refer to that very able exposition, "The Logic of Darwinism," by Mr. Archer Wilde. I imagine that almost every one who gives the matter serious consideration must agree with him that it is quite unreasonable

[1] *Essentials of Cytology* (Constable, London, 1907); *Hereditary Characters*, 1910.

to hold that there is really any fundamental difference between what are commonly called " natural " and " artificial " selection. What we know as artificial selection is merely the experimental proof of the effect of selection upon variations; it does not matter in the least whether the selection be applied by man or by other factors in the environment of the organism. The only difference is that the one is under the control, conscious or unconscious, of an experimenter, whilst the other is not. But he entirely missed the point I wish to emphasise here, and that is, that domesticated races possess a character in common or rather an exaggeration of a character which is not present in wild races. This is a tendency to produce comparatively large variations. Take even the most inbred stocks which are said to breed quite true and to impress their peculiar characters upon the offspring when crossed with another breed. Look at the pedigrees. The same individuals appear constantly as ancestors in the pedigrees of each descendant. This means that only those individuals have been used for breeding purposes who exhibited the desired variations; what is more important, that there were but few such individuals. Then, if in such a pedigree we look at characters which were not the objects of selection, as colour in racehorses, we find such variations common as are rarely or never found in wild animals. Domesticated races are, in fact, far more variable than are wild races. Why? Man is generally unable to detect small differences. " He has always selected animals or plants which vary from the mean of the race more than did their fellows. Whatever else he has selected then, he has always selected variability, which is just as much a character as anything else." [1] Those characters which in the domesticated races behave in the Mendelian manner may therefore reasonably be regarded as recent variations in individuals which have been rapidly exaggerated in the offspring by the mode of selection. Man, in his process of selection, has substituted his desires for many other factors in the environment and has allowed characters in which he was not interested to run riot in a manner that would certainly have entailed the destruction of the organism if it had not been protected by him. I would suggest that these characters which are apparently recent and which are trans- mitted alternatively should be called " individual " or "personal "

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[1] *Hereditary Characters*, p. 71.

characters, whilst those which are common to all the individuals of a race should be called "racial."

Do we know anything of the behaviour of racial characters when crossed? There are a great many illustrations from which I will select only a few. The cross between negro and Caucasian is a good example, and I take it the more willingly because Prof. Davenport, who as I have already pointed out apparently believes that all characters are transmitted alternatively, has used it. I am enabled to go further than this and use his statement of the case because of the very frank and fair manner in which he has dealt with the facts. He shows that the individuals forming consecutive generations may vary from as light as Caucasians to 46 per cent. of black in the skin. He goes on to say: "Just as perfect white skin colour can be extracted from the hybrid, so may other Caucasian physical and mental qualities be extracted and a typical Caucasian arise out of the mixture. However, this result will occur only in the third or later hybrid generation, and the event will not be very common." I suppose that we may presume that fresh white blood is being brought in at each generation and that even when several individuals who appear to be pure white have been produced, negro characters will be liable to appear in their offspring. The final production of a pure white race could therefore be more easily explained by a process of swamping than by alternative inheritance.

A better example, because it affords a direct comparison of the behaviour after crossing between similar characters, one of which is racial and the other individual, is afforded by the breeding experiments of Messrs. Prout and Bacot.[1] They found that the moth *Acidalia virgularia* in the neighbourhood of London was dark. The same moth found at Hyères in the South of France was white. They crossed individuals from the two races and bred ten generations which provided between five and six thousand specimens. There was no segregation into dark and white groups, but such delicate intergrading between the two parent forms that grouping was impracticable. In the case of local variants of other Lepidoptera, *e.g. Tryphœna comes* and its dark aberration, *Xanthorhoë ferrugata* and its black

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[1] "On the Cross-breeding of the Moth *Acidalia virgularia*," *Proc. Roy. Soc. B,* vol. lxxxi. 1909.

aberration,[1] the same authors obtained Mendelian results. They came to the conclusion that, in order to obtain Mendelian segregation, variations occurring in a race occupying the same geographical area must be crossed; but that if characters in geographically separated races are crossed, they blend. My belief is that this happens simply because the variations in the same locality are individual characters of recent origin, whilst differences between two geographically separated races, which are common to all the individuals of each race, are racial characters and are comparatively ancient.

Crosses between individuals belonging to different species and even to different genera of fish, among the Salmonidæ particularly, are common, and practically perfect blending of the characters is almost invariable.

The alternative transmission of personal or individual variations must be of enormous advantage in the process of evolution. As even every cell is different from every other cell, the number of variations round the mean of any character in the multicellular organism must be incalculable. It is also obvious that most of these variations must be useless and some actually injurious. The rapid elimination of useless variations is of great importance, and this rapidity is provided for by the alternative inheritance of recent variations. Only 25 per cent. of the second generation from the introduction of the variation can possess gametes which all carry the character. Of the rest, 25 per cent. will not possess the character at all and in 50 per cent. it will be present in only half the gametes. If the variation be advantageous, it will thus be more easily preserved; if it be useless or injurious, it will be more readily and rapidly eliminated.

We have in certain constituents of the cell—the chromosomes—and the mode in which they are alternatively distributed to the gametes upon fertilisation, an exact parallel to the distribution of the characters in Mendelian inheritance. I have elsewhere suggested the probability of the intimate connection between these phenomena.[2]

Sex is claimed as a Mendelian character, and with some modifications I feel that this claim is justified. Leaving aside the highly technical points in relation to chromosomes as deter-

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[1] *Entomologist's Record*, xv. and xvi.; *Trans. Entomol. Soc. London*, 1906, and *Proc.* 1907.

[2] *Hereditary Characters.*

minants of sex, described by both Profs. Davenport and Thomson, I think that the conclusion may be arrived at on more general lines. Such differences as constitute sex, fundamentally the difference between the production of cells that actively fertilise and those that are passively fertilised, must, like other characters, have arisen from variations that were transmitted in an alternative manner. In the case of variations generally which are of sufficient advantage to the race to be preserved by selection, the alternative inheritance disappears in time and the character becomes racial. But the advantages of the differentiation of individuals into two sexes is dependent upon the alternative occurrence of particular characters, so selection would necessarily have eliminated the tendency to blend to a great extent. That it has not done so beyond the necessary point is evident from the potentiality of producing the secondary characters of the opposite sex under certain conditions, a potentiality which varies in different individuals just as do all other characters. Thus we see that, as Prof. Davenport says, opposite characters when crossed always leave their marks upon each other when extracted; and also we see that the variation towards blending is always appearing, which fact Prof. Davenport has missed.

I must confess that I am unable to follow the argument of Prof. Thomson, who says that " the difference between an ovum producer and a sperm producer is fundamentally a difference in the balance of chemical changes, *i.e.* in the ratio of anabolic and katabolic processes." Why should not the difference in the " ratio of anabolic and katabolic processes " be the result, not the cause, of sexual differences ?

A comparatively recent and serious cause of contention has arisen out of de Vries' mutation hypothesis. In de Vries' own words, quoted by Prof. Thomson, this may be briefly described as follows : " The current belief assumes that species are slowly changed into new types. In contradiction to this conception the theory of mutation assumes that the new species and varieties are produced from existing forms by sudden leaps. The parent type itself remains unchanged throughout this process and may repeatedly give rise to new forms." Prof. Thomson has such a high opinion of this hypothesis that he constantly treats it as though it were generally accepted by biologists all over the world. It certainly accords well with the tendency he

frequently shows in his book towards a belief in some kind of supernatural directive power which regulates evolution, and on these lines is a most desirable asset to his arguments; but it is not the case that the hypothesis has been accepted by the majority of biologists, indeed many repudiate it altogether. Prof. Thomson is certainly more reasonable in one respect than Prof. Bateson, the apostle in this country of the mutation hypothesis. The latter and his school assume that "all organised nature is arranged in disconnected series of groups, differing from each other by differences which are specific."[1] I think that those biologists who have been largely occupied in the study of species and varieties are unanimously of opinion that so-called species very frequently, if not generally, merge into each other by almost insensible gradations. When these links are not found, their absence may reasonably be accounted for by the fact that enormous numbers of forms have disappeared in the past, without leaving any traces. Prof. Thomson realises that "species are often connected by intermediate links," but suggests that these links "may have been formed *after* the species from which they are theoretically supposed to give rise." To me this explanation appears inconceivable. The intermediate links are admittedly there. Therefore the organisms are obviously capable of producing these links between the two extremes. If they are produced gradually in response to slight changes in the environment, they will not throw the individual out of harmony with it, which any sudden large change must very frequently, if not always, do. Prof. Thomson lays great stress upon the criticism that the theory that evolution has been due to the selection of small variations "places such a heavy burden on the shoulders of natural selection that the idea of a leaping instead of a creeping Proteus has always been welcome." But to me the burden appears to remain the same, whether the intermediate links were produced in the process of species making or afterwards, for they have been produced in either case.

Whilst then the gradual small change in characters appears to offer so many advantages, the utility of sudden and large changes seems so highly problematical and this hypothesis seems so much in the nature of an intellectual "mutation" on insufficient grounds that I am not inclined to accept it.

[1] *Materials for the Study of Variation*, p. 17 (London, 1894).

Prof. Thomson gives a full and excellent account of the facts that led to the formulation of the mutation hypothesis, and here we find the explanation of its origin. I cannot find an instance of an established "mutation" except in domesticated races. De Vries' original case of a "mutating form" was the evening primrose. It was introduced into Europe from America probably during the eighteenth century, so there is no doubt as to its having been subjected to selection by man. All the other instances are similar, and when large variations in wild species are taken and bred from by man precisely the same criticism applies. No one denies that large variations do sometimes occur in races which have not been selected by man, though de Vries was not able to find any among the hundreds of wild plants he investigated. These large variations must throw the organism in which they occur so much out of adaptation to its environment that they must as a rule end in elimination, though it is conceivable that there might be some sudden change in the environment occasionally which would favour the preservation of a large variation in a particular direction should it occur. Changes in the environment are, however, almost invariably very gradual. But as I have already pointed out, man has always selected variability in the animals and plants he has domesticated. He has done more than this. He has selected the character of producing large variations, as large variations have been most easily selected by him, and as he has substituted himself for many other factors in the environment, he has removed that check upon the constant production of large variations which must usually be present under natural conditions. It is thus not surprising that de Vries found large variations in the first domesticated plant with which he experimented. But the selection of large variations by man will not be constant. When he has reached a certain standard he will in certain cases do no more than try to keep up this standard, and he will then reject large variations to some extent. Thus a particular organism will exhibit a tendency to produce large or small variations according to whether it has been recently selected for one or the other character. This may very possibly account for the origin of de Vries' hypothesis that "mutations" appear in considerable numbers in a given race at intervals but that between these "mutating" periods the race remains stationary.

The application of all these facts and theories about heredity

is of the greatest importance in relation to eugenics. I think that almost every one who has studied the matter at all thoroughly will agree in the main with Prof. Davenport's general conclusions. His opinion that all characters are inherited in an alternative manner does not matter so very much, whether he be right or, as I think, wrong; for the overwhelming proportion of the characters which would be selected by the eugenic methods would be recent variations—individual or personal characters, in fact—which are, according to the evidence available, inherited alternatively. I cannot, however, see eye to eye with him with regard to the crossing of black and white races or indeed any races, for the process of swamping undesirable racial characters would be a very lengthy and uncertain one; as I have already said, it does not appear that racial characters can be segregated by breeding. I cannot agree with him either that mental traits, such as imbecility and criminalistic tendencies, have come down directly through an unbroken succession of generations of individuals from our animal ancestors. The very factors in the environment which have produced an intellect incomparably superior to that of our ape-like progenitors, and a high standard of morality in the majority of individuals, must have continually eliminated variations in other directions. It seems to me more reasonable to account for these characters through the constant occurrence of variations in all directions. The latter view is surely also a much more hopeful one. There is some danger in Prof. Davenport's suggestion that individuals who, according to the results of the Mendelian experiments and observations, are capable of producing offspring with undesirable characters only when mated with others who are similarly capable, should be allowed to marry individuals that possess a clean pedigree. This means preserving the potentiality of producing the undesirable characters indefinitely. In his conclusions he appears also to have forgotten his own statement, that crossed characters always bear traces of their opposite. In spite of being apparently at times too much influenced by sentimental reasons in his suggestions, there is no doubt that if the measures Prof. Davenport advocates in his valuable book were adopted, an enormous benefit to mankind would result. His reasons are stated clearly, and though apparently his softer feelings prevent him in all cases from arriving at the complete logical conclusions which must result

from them, there is never any appeal to the metaphysical, nor does he allow sentiment to gloss over facts.

In the case of Prof. Thomson's book these matters are dealt with in a very different way. He appears to me to belittle facts and to enlarge sentimentality; he shows frequently that he places reliance in what, as far as I can make out, is a metaphysical directive power in evolution; though he has not formulated this definitely, as Bergson does, he has very decided leanings in that direction. A not inconsiderable number of biologists, most unfortunately, are inclined to somewhat similar opinions. Prof. Thomson lays great stress upon the danger of adopting legislative measures of limiting the breeding of the unfit, because many variations are "unknown quantities"; because "the unpromising bud may burst into a fair flower"; because evil traits may work themselves out; because many bad traits may be due to modifications produced in the individual by the environment (he quotes the Jukes as a possible example of the modificational effect of "social ostracism"); and because "preoccupation with the biological outlook—the breeder's point of view—will undoubtedly lead to fallacy upon fallacy, the 'materialisms' to which we have already referred."

If we take facts as they are, there can be no doubt that there is a constant interchange between the various grades of individuals in the civilised state. Variations towards mental and physical inferiority tend to cause a fall, and *vice versa*. The mortality in the lowest class is higher than in any other, and thus provides a process of elimination acting most forcibly upon the most undesirable part of the population. But modern sentimental legislation is altering all this. The mortality per thousand has fallen greatly all over the country, in the town population particularly. Dr. Chalmers recently gave an analysis of the mortality in the population of Glasgow. This shows that the mortality has fallen 19·4 per cent. during the past ten years, but that the greater part of this fall has been in families living in one or two rooms. The mortality of that part of the population consisting of families living in four rooms or more has remained practically unchanged. This gives one seriously to think, for it means that a most necessary form of selection is ceasing and nothing is taking its place.

It is quite certain that any form of selection may occasionally destroy desirable individuals, but this cannot be the usual

course of events. Besides, it does not seem to me worth while to preserve and breed from thousands of undesirables in order to avoid the possible loss of one desirable individual. Prof. Davenport's book shows that the production of the efficient by inefficient parents is very rare, whilst efficient parents commonly produce efficient children.

The question as to what proportion of undesirable traits may be modificational is a very important one, and one upon which it is very easy to fall into serious errors. It involves the question of the inheritance of acquired characters to some extent. The question to deal with is—which of the characters of the adult organism are acquired and which inborn ? We speak of them as those due to "nurture" and "nature" respectively; as being in fact divided into two distinct and easily separated groups. As Dr. Archdall Reid has pointed out, they are not to be thus easily distinguished. Every multicellular organism begins its existence as a single cell, the fertilised ovum; it is quite evident that the characters of the adult organism cannot be present as such in a single cell. What then represent the characters of the adult organism in the ovum ? The capacity to develop along certain lines within certain comparatively narrow limits under certain conditions. We may regard the ovum as a portion of very complex matter of such a nature and so shaped that additions can only be made to it in certain very definite directions and in certain very definite ways, with the result that it is capable of growing only into a particular form with particular characters. It is then these capacities for develop- ment along particular lines, these potentialities, which are inborn. The resulting development of these capacities must obviously be modified from the very first by the environment. The amount of possible modification by the environment varies enormously in different organisms. In the butterfly it is extra- ordinarily small; in man it is extraordinarily great. This great dependence of man upon modifications by the environment has led many people to attach too great importance to it and not enough to the inborn capacities. Take any class of school-boys. No two boys will show the same capacity for obtaining know- ledge and skill in any given subject; the boy who is above the average capacity in one may be below it in another, though most will be able to reach an average standard in all. Now it is quite evident that in such cases the difference in the environment is

not sufficient to account for the difference in facility with which the different individuals acquire knowledge and skill; indeed it would be easy to find innumerable examples where individuals with greater facilities had not done as well as individuals with less. The difference lies in the capacity of making acquirements in particular directions. It certainly may happen that the environment of an individual with a small capacity may result in his acquirements in a particular line being as great as those of an individual in a different environment who possesses a greater capacity, but the difference in the environments must be greater than the difference in the capacities to produce this result; which in many cases is unattainable under any circumstances.

Take the case of the Jukes quoted by Prof. Thomson. The "criminal taint" which he regards as being among the suggestions "quaint in their unpracticality" was in no ways due to the effect of "social ostracism," to the environment, in fact, for several members of the family were taken away in babyhood and brought up under circumstances most favourable to the development of any moral and other desirable mental capacities they might happen to possess. Unfortunately for Prof. Thomson's views, they all turned out as criminally inclined as their ancestors. Their performances appear to have been limited mainly by their opportunities.

We know quite well that mental capacities, that is, capacities for making particular mental acquirements, are subject to selection just as much as capacities for making physical acquirements. Breeds of sporting dogs are examples of this point. Therefore I do not see any valid reason for saying that the biological point of view is likely to lead to fallacies. Certainly it is less liable to lead us astray than a combination of sentimentality and metaphysical speculation.

With regard to the transmission of acquired characters the real question is, therefore, whether these inborn differences in capacities for making acquirements can be reproduced in the germ cells by the action of the environment upon the organisms producing the germ cells; whether in fact the effects of the environment upon the parent can be metamorphosed into a capacity for acquiring characters in the offspring. To me it appears rather like saying that the effect produces the cause. However, as there are apparently many who do believe that acquirements are transmuted into capacities in successive

generations, a consideration of the nature of the evidence is necessary. Prof. Thomson gives a full account of the evidence on both sides which occupies eighty-five pages of his book. He is able to arrive only at the following conclusion, however, which he obviously considers to be of the utmost importance, as he prints it in italics:

" *If there is little or no scientific warrant for our being other than extremely sceptical at present as to the inheritance of acquired characters—or better, the transmission of modifications—this scepticism lends greater importance than ever on the one hand to a good 'nature,' to secure which is the business of careful mating; and, on the other hand, to a good 'nurture,' to secure which for our children is one of our most obvious and binding duties; the hopefulness of the task resting especially upon the fact that, unlike the beasts that perish, man has a lasting external heritage, capable of endless modifications for the better, a heritage of ideas and ideals, embodied in prose and verse, in statue and painting, in cathedral and university, in tradition and convention, and above all in society itself.*"

This does not seem to help very much. Prof. Hartog's evidence is all one-sided. Beyond what I have already said as to the improbability of the transmission of acquirements, we find that, in fact, a very favourable environment when applied to all the individuals of a race tends to result in the disappearance of characters. Characters are preserved only when necessary or beneficial to the individual. But necessary and beneficial characters, or rather the potentiality of producing them, must generally be of such a nature as to enable their possessors to resist or overcome unfavourable factors in the environment. But unfavourable factors in the environment must always be injurious to the individual, and if the inheritance of the response to the environment be accepted it involves the belief in the evolution of a potentiality, which must be present in every individual, of selecting which kind of acquirement is to be inherited and which is not—just as big a result in itself as all the rest of evolution without it. Without something of this kind an unfavourable environment must necessarily cause a race to grow weaker, while a very favourable environment would cause it to grow stronger. We know that this is not what happens. On the other hand, that the capacities for development along certain lines should be produced by the selection of favourable variations occurring in individuals seems easy to understand.

Perhaps the most important point as regards eugenics is how far the Mendelian phenomena apply to the human race. Any means which are to act in a selective manner in improving or preventing the degeneration of the race must be applied to characters appearing in individuals. Particular characters in individuals, as individuals, must be dealt with. It seems probable that most of these will prove to be comparatively ecent variations and so will be transmitted alternatively.

It is in fact the selection of variations occurring in individuals which offers the only chance of improving the characters, mental and physical, of a race. Nothing in the way of forcing acquirements upon individuals with inferior capacities can raise the standard of capacity in the race any more than teaching bull-dogs to point would produce a capacity of learning to point. Selection of variations in the capacity for acquiring the necessary characters involved in pointing, if extended over many generations, would no doubt produce a race of bull-dogs that were comparatively easy to train to point, but it would hardly be a practical proposition, as we already have a breed of dogs which has been subjected to selection with regard to these capacities for hundreds of generations. In the same way it does not seem to be a practical proposition to attempt to breed men with desirable and without undesirable qualities from the failures by selecting the favourable variations they may produce. They would reproduce thousands of unfavourable variations to one favourable one, and that one would vary from a lower mean than the average; and worse than all, the undesirable offspring cannot be drowned as puppies are by the breeder, but must be kept alive to produce more undesirables.

Such characters as lunacy and idiocy, deaf-mutism and criminal tendencies, were, until recently, subjected to such stringent selection that they must have been eliminated very soon after the unfavourable variations appeared. So far Prof. Davenport's views are, I think, unassailable. But when it comes to crossing racial characters, mental or physical, the problem is a more serious one and involves far greater dangers; as, if my views are correct, even a slight blend of undesirable racial characters may be almost impossible to eliminate.

THE METHOD OF DARK-GROUND ILLUMINATION IN BOTANICAL RESEARCH

By S. REGINALD PRICE, B.A.

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To the microscopist the method of dark-ground illumination, and its recent extended applications, are so well known as to need no general description, but to many of those who use the microscope as an instrument of research the method is more or less strange. Hence a short description of the general principles may not be out of place, as an introduction to a brief review of botanical work which has been done of late by its use.

It is a well-known fact that small particles when illuminated strongly from the side appear to the observer as though they were self-luminous—diffraction images are produced and observed by the eye. By means of these diffraction images, particles which are too small for observation with the unaided eye may be made visible, much in the same way as the stars, although point sources of light, are visible by their diffraction images.

Every one must have observed in an early morning walk in the woods, how fine spiders' webs, illuminated by lateral shafts of sunlight through the trees, appear as incandescent silver lines, even when so far from the eye as to be quite invisible under ordinary conditions.

Prof. Buller[1] has also shown that spores of certain fungi, measuring only 10 μ or even less, can be rendered apparent to the unaided eye by means of an intense beam of light projected through a spore cloud, in a direction perpendicular to the observer's line of vision. It is obvious that both the spider's web at a considerable distance from the eye, and the fungus spore in any case, are outside the possibility of unaided vision,

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[1] Buller, Prof. A. H. R., *Researches on Fungi*, vii. p. 94. (Longmans & Co., 1909.)

and are only rendered visible by the scattering of light which they bring about.

This general method of illumination, which is aptly called dark-ground illumination, has been applied to observation with the microscope. For use with low powers of the microscope only, the method has long been known, but it is since the beginning of the present century that the great development of the method for high-power work has taken place.

In 1903, by the employment of the ultramicroscope, Siedentopf and Zsigmondy showed the possibility of demonstrating the presence of particles which were below the limits of microscopic vision. For a short general discussion of the principles and methods of ultramicroscopy reference may be made to the article by H. Thirkill in this journal for 1909.[1]

As there is a growing confusion with regard to terminology, a few words on the subject may not be out of place. The term "ultramicroscope" is best confined to the form of apparatus with unilateral illumination as originally devised by Siedentopf, although on the continent there is a great tendency to extend the term. Sub-stage condensers especially designed to give dark-ground illumination should be called dark-ground illuminators, although in many cases it is possible by their means to observe particles which are below the limits of observation with the microscope with direct illumination. The apparatus of Siedentopf and Zsigmondy is thus a special means of producing dark-ground illumination applicable for ultramicroscopic observation ; but dark-ground illumination does not necessarily imply ultramicroscopic vision.

So also the newer illuminators, the Cardioid condenser of Zeiss[2] and the Ultracondenser of Leitz[3] are best regarded as dark-ground illuminators, although their light-concentrating power is greater than that of the ultramicroscope.

Attention will now be turned to the special subject under discussion, and an indication will first be given of how the method is best applied in the observation of suitable plant structures.

For microscopic observation, botanical objects have generally

[1] Thirkill, H., "Ultramicroscopy and Ultramicroscopic Particles," SCIENCE PROGRESS, 1909, p. 55.

[2] v. Zeiss pamphlets : "Mikro 306," "Cardioid Ultramicroscope."

[3] v. Leitz pamphlet.

to be mounted between an object slide and a cover slip, so that the ultramicroscope of Siedentopf is quite unsuitable for ordinary work; moreover, the method gives apparently no better results for this class of work, and is considerably more difficult to use, than various types of dark-ground illuminators. Most of the sub-stage immersion condensers give good results for such work as the study of small transparent structures, or for the observation of the intimate arrangement of the living cell. Gaidukov[1] also used, with considerable advantage in the case of thick objects, Siedentopf's[2] method of stopping out the central portion of the front lens of the objective; but observation is rather difficult with this apparatus.

Dry objectives give on the whole the best results, but the apochromatic series is greatly superior to ordinary objectives. If homogeneous immersion is used, a suitable stop must be introduced, when very good results are obtained. A. E. Conrady has recently shown[3] that the maximum resolving power with dark-ground illumination is obtained when the condenser has not less than three times the N.A. of the objective.

The centring of the sub-stage condenser is very important. As a source of light a good Nernst lamp is sufficient for some work on ciliation and the study of bacteria, but for the colloid structure of the cell a small arc lamp is much more suitable and shows particles which are missed with a weaker light. As a condenser a spherical flask of water is very useful, and prevents a large heating effect on the stage of the microscope. The object slides—of selected thickness—and cover glasses should be of good quality, specially cleaned, kept in alcohol, and rapidly dried just before use.

Work of a botanical nature which has been done by the application of this method falls generally into two main categories, which will be considered separately. The method has greatly facilitated the observation of small, transparent structures such as cilia, and of minute bacteria in the living state, and as a development of its application to the study of colloids it has been applied to the optical analysis of the living plant cell and the protoplast.

[1] Gaidukov, v. infra.

[2] Siedentopf, v. Zeiss pamphlet No. 228.

[3] Conrady, A. E., Jour. Quekett Micro. Club, xi. 1912, pp. 475-80 ; v. abstract, Jour. Roy. Micro. Soc., April 1913, p. 210.

I. Study of Minute Organisms and Ciliation

In 1904 Rählmann[1] showed that the method of Siedentopf and Zsigmondy could be used with advantage for the study of bacteria in the unstained condition. Even when of comparatively large dimensions these are difficult to observe in direct illumination, but by the dark-ground method diffraction images of even the very minute ones appear as bright patches of light against a dark background.

Cotton and Mouton[2] showed that observation of bacteria was also possible with their special total reflection apparatus, and it has since been shown that the sub-stage dark-ground illuminator is in general very suitable for observations upon living bacteria.

It is obvious from the theory of the method that true images of the organisms are never obtained, but that generally a very good idea of the actual form is given, since the diffraction images are produced by objects whose dimensions are within the limits of microscopic vision.

A considerable number of observations have been made on the flagella of living bacteria. As the observations are mostly of interest to the specialist they will not be discussed further here. For a list of some of the more important papers which have appeared among a large literature, reference may be made to the work of Dr. Gaidukov.[3]

The method also provided a means for study of the moving cilia of motile algæ, of zoospores, and so on. These extremely fine and transparent structures when illuminated by this method appear as bright moving lines against a dark background. As is well known, it is much easier to see a bright line on a dark ground than a dark line of the same width on a bright ground, so that if this were the only effect the visibility of these cilia would be greatly increased. As they are by no means black lines when viewed in direct illumination, the contrast obtained by the two methods of illumination is even more pronounced. V. Úlelah[4] has recently published a series of researches on the movements of cilia of various organisms, the observations being performed by the aid of a Zeiss paraboloid. The following list

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[1] Rählmann, E., *Munch. Med. Wochenschr.* Nr. 2, 7S. 1904.
[2] Cotton and Mouton, *Les Ultramicroscopes*, etc. Masson, Paris, 1906.
[3] *V. infra.*
[4] Úlelah, V., *Biol. Centralblatt.*, 1911.

of some of the organisms which he studied will give an idea of the general utility of the method for this class of work :

Flagellata :
> Monas, Bodo, Euglena.

Bacteria.

Chlorophyceæ :
> Chlamydomonas, Pandorina, zoospores of Ulothrix, Coleochæte.

Phæophyceæ :
> Scytosiphon.

Bryophyta :
> Spermatozoids of Marchantia.

The actual results obtained are hardly of general interest, but from the point of view of this discussion the interest attaches rather to the method employed.

II. STUDY OF THE PLANT CELL AND THE PROTOPLAST

The great utility of the method in studying the structure of colloids had been demonstrated by Zsigmondy, and as it was generally becoming realised that protoplasm was colloidal in nature, a study of the plant cell by this method was likely to throw further light on the actual state involved.

Observations in this direction were first made by Dr. N. Gaidukov, the appearances of certain objects being described in the *Berichte der deutschen botanischen Gesellschaft* for 1906.[1] Most of the published work on the subject is due to Gaidukov, and a full account of his researches will be found in his work, *Dunkelfeldbeleuchtung und Ultramikroskopie in der Biologie und in der Medizin.*[2]

In the practical application of the method the observer is confronted at the outset with numerous experimental difficulties, chiefly perhaps in the task of finding suitable material for investigation. For observation with a sub-stage condenser the material must be mounted in water in the usual way, and for good results must be only one cell in thickness, otherwise the

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[1] Gaidukov, N., *Berichte d. d. bot. Ges.*, 1906; *Unters. mit Hilfe des Ultramikroskopes*, p. 107; *Weitere Unters.*, etc., p. 155; *Über ult. Eigenschaften der Protoplasten*, p. 192; *Ult. Unters. der Stärkekörner*, etc., p. 581.

[2] Gaidukov, N., *Dunkelfeldbeleuchtung und Ultramikroskopie in der Biologie und in der Medizin.* (Gustav Fischer, 1910, 8 marks.)

greater portion of the light is scattered by the lower cell layer. This at once limits the field of choice; sections with torn cell walls and escaping contents are generally useless, the scattering effect of these preventing any good observation of the contents of unbroken cells. Unicellular organisms, filamentous Algæ, leaves of some water-plants, leaves of some Bryophytes, fungal hyphæ, and plant hairs give most of the categories from which selection can be made. There are still other desiderata for good observations to be possible. The diameter of the cell or the filament must not be very small, as if this is the case the diffraction effects produced by the walls greatly interfere with observation of the cell contents. The walls should be free from markings and generally optically homogeneous, the slightest heterogeneity again preventing satisfactory study of the cell contents. If possible also chromatophores should not be too conspicuous as they also tend to scatter light, though not very strongly in most cases, but their images mask those of some of the smaller particles.

The scarcity of good material is undoubtedly the greatest barrier to the comprehensive use of the method. Some of the best objects so far examined are : *Spirogyra*, *Mougeotia*, Desmids, staminal hairs of *Tradescantia*, Myxomycetes,[1] the leaf-edge cells of *Elodea canadensis*, root hairs, hairs of certain flowering plants,[2] and the hyphæ of *Saprolegnia* and other fungi.

Only one or two filaments of the Algæ, a single leaf of *Elodea* as clean as possible, and so on, should be used to get the maximum light effect. There is no need to use specially prepared "ultra water" for this kind of work, ordinary distilled water being free enough from particles, and in any case it is almost impossible to prevent such from escaping from broken cells into the mounting liquid.

In many cases the appearance of a living cell when first viewed by this method is undoubtedly surprising, especially if no previous study of colloids by the method has been made. Perhaps it may be said that a little study of the cell in this way serves to emphasise more strongly than ever the fact that the single cell is a system of great activity. This is the case for some cells only, as will be seen below, and obviously we cannot

[1] Gaidukov, *l.c.*

[2] Price, S. R., "Observations with Dark-ground Illumination on Plant Cells," *Proc. Camb. Phil. Soc.*, vol. xvi. p. 481.

postulate a similar organisation and structure for cells of all types.

Spirogyra is undoubtedly one of the best and most easily obtained objects, and this part of the subject can hardly be introduced better than by a short description of the general appearances presented by the cells under this type of illumination. A species of rather large diameter with a fairly loose spiral chloroplast is most suitable, but any species of not too small diameter will suffice.

As is well known, the protoplast forms a layer lining the wall of the cell ; in this layer is the chloroplast, while the nucleus is suspended in the central vacuole by cytoplasmic threads. Under dark-ground illumination the protoplasmic layer is seen to contain large numbers of small particles, manifested of course as bright points of light, and in the living cell exhibiting a constant oscillating motion, generally about a small orbit. As is well known, the protoplast in direct illumination appears as practically homogeneous. These particles (which are probably to be classed as sub-microns [1]) can be brought into focus above, that is outside the chloroplast, so that without doubt they are actually in the protoplasm. So also these particles can be seen in the cytoplasmic threads which suspend the nucleus, where they also show this oscillatory movement.[2] More careful study, and the examination of plasmolysed cells,[3] reveal the presence of smaller particles in the protoplasm, which are undoubtedly completely ultramicroscopic.

On focussing below the upper part of the chloroplast, that is to say in the vacuole, particles of much larger size can usually be observed also in oscillation. These particles can often be seen on careful examination of the cell in transmitted light, and they are obviously of quite another order of magnitude. Gaidukov thinks that they are particles of some colloid nature in the cell sap. Such particles seem to occur quite frequently in the sap vacuoles of plant cells, and on account of this they may be referred to as " sap particles " or " sap inclusions."

The chloroplast shows little detailed structure, giving rather

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[1] The terms are generally thus applied : *Microns* are small particles visible with direct illumination in the microscope. *Sub-microns* are ultramicroscopic, but may be made visible by methods of dark-ground illumination. *Amicrons* are below the limits of observation.

[2] Price, *loc. cit.*

[3] Price, from unpublished work.

a dull reflection image, while the pyrenoids appear as bright spots. The cell wall itself is optically homogeneous.

The oscillating movement of the particles both in the protoplasm and cell sap, already referred to, is undoubtedly of the nature of a Brownian movement. Since the great impetus given to the study of colloids by Siedentopf and Zsigmondy's work, this phenomenon has been brought into fresh prominence. Discovered by a botanist, Dr. Brown, in 1827 (after whom it is called), it was shown to extend to particles of extremely minute and ultimately of ultramicroscopic size, though here the movement is very much more rapid. It has been shown that the rapidity of motion varies inversely with the size of the particles, and, as a result chiefly of Perrin's beautiful researches, it has been shown almost without doubt that the movement is a direct expression of the actual molecular movement in the surrounding fluid. Zsigmondy observed that the minute particles present in liquid colloid solutions—of the nature of " sols "—showed such a Brownian movement in a very striking manner.

Spirogyra is quite good for the study of this Brownian movement, for the larger sap particles can be seen to oscillate much more slowly than the minute particles of the protoplasm, illustrating the variation of the rate of movement with variation in size of the particles.

Very similar appearances are given by other cells examined. *Mougeotia*, for example, shows a similar structure, with well-marked Brownian movement, but on account of the character of the chloroplast is not quite so suitable for observation.

The staminal hairs of *Tradescantia*, used by Gaidukov, have a cell wall which is optically heterogeneous, and this interferes with the clear observation of the cell contents. Cells in four different states of vitality were examined.

1. Young cells without sap vacuoles.—Particles with strong Brownian movement were present in the protoplasm.

2. Older cells with vacuoles and streaming protoplasm.—In spite of which the Brownian movement was clearly seen.

3. Dying cells.—A moderately active movement could be seen.

4. Dead cells.—The protoplast had coagulated and the constituent particles were motionless.

In the young cells the Brownian movement is more difficult to see, on account of the closer aggregation of the particles in the complex.

Myxomycetes in the " amœba condition " were also examined, and generally showed the protoplasm filled with moving particles.

In *Vaucheria, Cladophora, Œdogonium,* and *Stigeoclonium,* the chloroplasts generally prevent the clear observation of the cytoplasm, so that these are not good objects for study.

The leaf edge of *Elodea canadensis*[1] makes quite an instructive object. The leaf edge is only one cell thick, and the cell walls are very clear. The protoplast usually lines the cell wall, while the chloroplasts of these edge cells are comparatively few in number and relatively inconspicuous under this illumination. " Sap particles " are nearly always present in the cell sap. The protoplasm is seen to contain very numerous small particles, which exhibit the usual Brownian movement. After a time, circulation of the protoplasm usually occurs, and the particles can be clearly seen, as they are carried on by the stream, still executing their Brownian oscillations. The sap particles are usually unaffected by this circulation.

Gaidukov[2] states that towards the cell wall and the vacuole the hydrosol is covered by a layer of gel—" hydrogelschict "— which is produced by the contact of the hydrosol with the electrolytes of the cell sap. These electrolytes coagulate the hydrosol and protect the inner portion of the complex from further reaction with the solution—the reversible portion from forming a colloid solution with the water, and the irreversible portion from coagulation. There is, of course, considerable reason for identifying this layer with the plasmahaut; but there is here room for a great deal of work.

In other cases of cells examined the protoplasm presents another appearance. No discrete particles can be made out in the protoplasm and no motion can be detected. In some cases the protoplasm has a somewhat mottled appearance, recalling perhaps the network-like structure as postulated by Bütschli and other observers.

On the death of a cell which shows a structure with moving particles, a complete cessation of the movement in the protoplasm is brought about. This is also naturally the case when fixing agents are allowed to act on the living cell. The proto-, plasm then appears as a mass of overlapping diffraction images—

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[1] S. R. Price, *l.c.*
[2] Gaidukov, *Berichte, l.c.* p. 587; *Dunkelfeld.,* etc., p. 62.

an appearance, of course, indicating a heterogeneous structure for the fixed plasma.

The living material of the plant cell in many cases thus exhibits a structure which we have been led to attribute to that type of colloid solution, the hydrosol. This was perhaps the most important fact established by Gaidukov's researches. As has been mentioned above, with the gradual development of the study of the physics and chemistry of colloids it became increasingly evident that the protoplasm was to be regarded as a complex of this type. Thus the activity of the cell depends in a certain measure on the activity of the colloid hydrosol, and the death of the cell and coagulation of the colloid complex are probably closely inter-related; in fact, we may say that the coagulation of the hydrosol causes the cessation of living processes in the protoplasm, and the irreversible change hydrosol —hydrogel, is synonymous with death. This, at least, appears to be Gaidukov's view.

There are, however, those cases of cells which do not appear to show the hydrosol structure, to be considered; for here also, in most cases, the protoplasm must certainly be regarded as in an actively living state. It may be said that most cells which permitted of favourable observation did show Brownian movement, and Gaidukov considers that the cases referred to may possibly be explained as follows: the particles in a young cell are much more difficult to make out, and the Brownian movement is more difficult to observe, chiefly, it would seem, through the close proximity of the particles of the disperse phase in the continuous phase. The same reasoning, he thinks, may apply to these other cells, the particles being too close and too small to manifest their motion by this method. Whatever the explanation may be, however, there is no doubt that the protoplasm is not to be regarded as a single type of complex, but a series of different colloids with differing properties in different cases—"the protoplasm is very polymorphic."[1]

A short summary of the main conclusions reached by Gaidukov may be useful, although involving some repetition of what has already been described.[2]

[1] Gaidukov, *l.c.* p. 61.

[2] Gaidukov, *v.* Bechold, *Die Koll. in Biol. und Med.*, p. 256. (Steinkopff, Dresden, 1912.)

1. The small particles with Brownian movement generally seen in favourable cases showed the protoplasmic colloid to be of the nature of a hydrosol.

2. These particles can unite with one another, forming aggregates; or break up, thus decreasing or increasing in number. (This may be related to variations in the general vitality or nutritive condition of the cell.[1])

3. In other cases, cells which were undoubtedly living, and generally speaking well nourished, failed to show any such movement, but the motion may have been masked by the smallness and close proximity of the particles.

4. The spontaneous change from the sol state to the gel or vice versa was not observed in the living cell. On the death of a cell, however, complete coagulation of the sol takes place, with cessation of the Brownian movement, while the gel thus formed gives an appearance of crowded diffraction images under dark-ground illumination.

5. The colloid complex of the protoplasm consists of a reversible and an irreversible portion.[2] This is deduced from the behaviour of broken living and dead cells in water. Some particles produce a colloid solution with the water—the reversible portion—while others aggregate and remain together—the irreversible portion.

6. Since the protoplasm contains an irreversible colloid, the taking up of an electrolyte by the cell should result in its coagulation. Some evidence is brought forward to show this, but the matter requires further investigation.

It may perhaps be said that the method has not realised to the full, the expectations of those who hoped it would clear up definitely certain vexed questions of cell structure. The idea of the method generally suggests the possibility of its application to the cytological study of the nucleus and the behaviour of the chromosomes in the living nucleus. In this direction but little help has been derived from the method up to the present, and only in a few cases has nuclear structure been observed. The difficulty of choosing suitable material is even greater than ever, and generally only resting nuclei have been observed. Where this has been done, the nucleus seems to show little except the

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[1] v. Bechold, l.c. p. 256.

[2] See any work on colloids, e.g. Introduction to Physics and Chemistry of Colloids, Emil Hatschek. (T. & A. Churchill, 1913, 2s. 6d.)

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ordinary colloid structure.[1] It may be that further careful use of the method will add to our knowledge of the behaviour of the nucleus in the living state, but on account of its limitations the method can never become a general one for the study of nuclear cytology.

These limitations are also an obstacle in the way of progress by the method, in the extended study of the intimate physics and constitution of the plant cell. As has been indicated, the protoplasm is by no means constant in characters in the cases which have been examined, so that for a logical study of cell physiology in relation to the plant the component cells in question must be examined. There is no doubt, however, that the method has given us a further insight into the actual structure of the living cell, and considering its comparatively recent development these results are sufficient to establish it as an important method of research. Certain attributes of cell structure must be of more or less general application, and along these lines the results should be of great use.

No attempt has been made in the present brief account to discuss the problems which arise from considerations of the results obtained. It has been rather desired to give in outline the methods of practical application of the principle to botanical work, and to state without any full discussion the main results which have so far been achieved. In the study of colloids the method is now an indispensable one, and undoubtedly it must become so in researches into the behaviour of the colloid protoplast.

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[1] _v._ Gaidukov; also from unpublished work of the Author.

SCIENTIFIC SPELLING

I —By SIR HARRY JOHNSTON, G.C.M.G., K.C.B., D.Sc.

THE Editor of this review has asked me, who have just published a work on Phonetic Spelling through the Cambridge University Press, to write on the subject of 'Scientific Spelling' in the pages of this quarterly.

In some ways I prefer the Editor's suggested title to that which covers my book, for any change of a radical nature which we may attempt to make in the orthography of English or any other well-established tongue should be scientific as well as what may be called phonetic; that is to say, that as nearly as possible we should interpret the utterances of the human voice with scientific exactitude, classifying the sounds—vowel and consonant—in relation to the parts of the mouth and throat which utter them.

Phonetic or scientific spelling must be logical. All sounds which we describe as single because it is exceedingly difficult, if not impossible, to split them up into component utterances, must be represented by distinct single letters, and compound sounds be expressed by the letter symbols of their component parts, only a very few exceptions being made in cases where the compound sounds are so nearly fused that division becomes an act of preciosity, or where the construction is so common and so frequently uttered that it should be given one simple and easily formed symbol. A case in point is the sound of *o* in 'bone' and 'mow.' This in most reasonable phonetic systems is represented by the Greek letter *ω*, whether or not this was the value of the omega. In reality it is a fusion of the separate vowel sounds of *ŏ* and *u*. Similarly, in the scientific alphabet I propose, and in the majority of those already adopted by scientific men abroad, the letter *c* stands for the English *ch* in 'church' or the Italian *c* in 'cielo' or 'cera,' and *j* likewise has its English value, instead of being used as the consonantal *i* (*y*). Logically, it would be more correct to express *c* by *tsh* (if one used the orthography of the India Office or Royal Geographical

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Society), and *j* by *dzh*. Personally, I object to adopting what may be called the India Office alphabet as the final scientific orthography for the rendering of all tongues all over the world ; for the reason that it is not strictly logical, and does not take into account the need for expressing a variety of sounds and combinations of sounds which occur not only in English but in many other important languages. Take, for example, the matter of aspirated letters. In English, and very much so in Arabic and the languages of India and of East Africa, we have aspirated consonants—*th, ph, dh, sh, kh, ch*, and *zh*, which require the *h* to express the aspiration that follows. This need precludes the use of *th* and *dh* to express the English *th* in 'this' and 'think,' and *zh* for the French *j* or the *z* in 'azure,' or *ph* in 'physic' (which last we pronounce literally as *p h* in 'Clapham' and 'haphazard'). The Arab name of the Muhammadan university at Cairo—Al-Azhar—is pronounced 'Az-har,' and not as if it were written in French, 'Ajar.' A large proportion of the *kh*'s that we meet with in Indian words are not pronounced like the *ch* in the Scotch 'loch,' but like the aspirated *k* in 'block*h*ead.'

Consequently, we need in our scientific alphabet single symbols for the German and Scottish *ch* (or the *kh* so constantly used in transcribing Arabic, etc.), for the modern Greek χ, for the quite different *ch* in the English, Spanish, and Indian languages, for the *gh* represented by the Arabic ﻍ, for *th* in 'theory' and *th* in 'that,' for the *sh* and *zh*. We require to discriminate between the ordinary *s* represented by *s* in 'sea' and *ss* in 'fussy,' and the alveolar Arabic ṣ (ص), between the German *ch* in 'machen' and that in 'ich' and 'dicht.' (This last, represented in the standard alphabet of Lepsius by χ̇, is practically the pronunciation of the Polish ś, and the sound is alleged to occur in certain forms of East African speech. It is a transition between the English *sh* and the German *ch*—*s* and χ.) Then, again, we must provide a symbol for the Arabic *d* (ض), *t* (ط), and *z* (ظ), most of which are alveolar, almost palatal pronunciations of the ordinary *d, t*, and *z*. The nasal consonant—expressed clumsily by *ng* in most modern European tongues, and still more clumsily by the apposition of two gutturals in Greek—must have a symbol all to itself, and this is most conveniently supplied by the *ñ*. It is true that *ñ* is associated in Spanish with the palatalised *n*, but this is really nothing but *ny*, two separate sounds combined. We are, however, used to

the tilde (~) in Portuguese and in a good many conventional alphabets as a sign of nasalisation. To employ the *ng* for this suggests the carrying on of the *g* sound. This no doubt was the original pronunciation of *ng* in English as well as in the Teutonic languages of the Continent, but in modern German and Dutch, for example, *ng* has become identified exclusively with *ñ*; and if one wishes (say, in transcribing words in the Malay Archipelago) to give it the value of the English *ng* in 'linger,' 'finger,' one has to write it *ngg*. If it is required to express the value of *ñk* in 'think' or 'blinker,' it must be written *ngk*. *Ng* in English writing is a most puzzling combination to the foreigner. When it terminates a word it is pronounced like *ñ*, as also when it occurs in the middle of words like 'singing,' 'clinging.' Where it is derived anciently from the French it is pronounced like *nj*, as 'ranging,' 'manger,' 'danger.' And it is given its logical pronunciation as *ñg* in 'finger,' 'anger,' 'Rangoon,' etc.

The value of the modern Greek gamma (γ), of the Arabic *ghain*, and of the velar *r* in modern German and French pronunciation is best represented by the Greek γ; though in the case of the velar *r*, which exists—unacknowledged—in the modern pronunciation of French, German, Danish, and Northumbrian English, this ugly variation, if it is to be encouraged and recognised at all, is most conveniently expressed by *ŕ*.

In the scientific alphabet I propose, the four distinct clicks of Hottentot and Zulu (and four out of the numerous Bushman clicks) are represented by clearly differing modifications of the letter *c*, as these prove to be easy to write and constitute a compromise between the inconvenient types of Lepsius and the inadmissible rendering of these clicks in the South African alphabet by the letters *c*, *q*, *x*, and *qc*. The other and more obscure clicks in Bushman can be distinguished by the symbols proposed by Bleek and other writers on the Bushman language. It is impossible to follow official South Africa in the allocation of *c*, *q*, *x*, and *qc* for the four click sounds in Zulu and Hottentot (one of which is sometimes employed in Sesuto), because *c* is already required for *tsh*, *q* is the natural equivalent of the Arabic ﻕ (which also occurs in Hebrew, Phœnician, and most of the Semitic tongues, besides in certain Hamitic, Asiatic, Oceanic, and African languages), and *x* must be taken from the Greek (as *x* or *χ*) to represent the guttural of widespread use heard

in the Scottish *ch* and inadequately represented hitherto by *kh*.

When we add the Polish *l* (*ł*) and the strong Arabic *h* (*ḥ*), *ş* and *ż* for the palatalised *s* and *z* (English *sh* and *zh*), *d* for the *th* in 'this,' and *ŧ* for the *th* in 'think' to the already familiar *m, b, w, v, p, f, s, z, d, t, n, l, r, y, ñ, k, g, q,* and *h*, we have all the consonantal symbols which can—in reason—be possibly required for writing and printing all the known languages of the world.

As regards vowel sounds, we have first of all to recognise the curious fact that some which would appear to be primordial and simple vowel sounds (amongst those first uttered in human speech) have, in the alphabets of the Mediterranean which laid the foundation of our own Greek, Latin, Cyrillic, German, and Irish letters, received no single, individual equivalent in a sign without a special accent or a diacritic mark. Such primordial vowels of world-wide use are *o* as in 'store,' or as represented by the diphthong *aw* or *au* in English; *ö* like the English *u* in 'hurt,' *ea* in 'heard,' or *ir* in 'bird' (the German *ö*, the French *œu*, the Scandinavian *ø*); *u* as in 'hut'; *a* as in 'hat'; the Welsh *ŷ* and the Slavic *y* or ы. The Greek *u* (upsilon), heard in modern French and in Dutch and met with in many modern forms of civilised and savage speech, secured for itself the ordinary *u* symbol in Greek, leaving its original sound to be represented by two letters—*ou*; but in Latin the Greek *u* (*ü*) came to be represented by *y*, and this value of *y* is still continued under some conditions in Germany, and much more so in Scandinavia. In Western Europe the Latin symbol *y* faded into a light *i* sound as a vowel, or became the equivalent of the consonantal *i* which in other directions was taking the form of *j*. It has been frequently suggested by German phonologists that we should represent the French *u* or the German *ü* by the Latin *y* and recur to *j* for the consonantal *i*. But on the whole, for reasons which I have given at length in my book on Phonetic Spelling, I think it is wiser to continue the use of *j* for the palatal combination *dż*, and retain *y* for expressing the consonantal *i*, a sound between vowel and consonant which links guttural and palatal consonants together. *Y* is as necessary as a separate symbol (instead of using the short *i*) as *w* is to represent a consonantal *u*, for *w*, though nearly equivalent to the short *u*, is also a semi-consonant and is closely connected in speech development with *b, v,* and *p*; and, strange to say, with

g and *γ*. In common with others writing on phonetics, I adopt in slightly modified form the Greek omega as an equivalent for the diphthongal sound of *o* in 'bone.' I adopt the italic *a* as the equivalent of the sound of *u* in 'but,' or of the short *a* met with in Arabic and so many Indian tongues, also in parts of West Africa. This vowel (*a*) is of course extremely common in modern English and represents the perversion of the short *u* which began in Elizabethan times. This perversion had its analogue on the Continent, where we find, earlier than the period mentioned, the diphthonging of an original Teutonic *u* into *au* ('hus' into 'haus'). At the same time in England, and very slightly in Holland and Flanders, the short *u* was pronounced like *a*, which is really an extremely abbreviated pronunciation of the diphthong *au*. We see this in the relations of 'out' and 'utter,' 'bout' and 'but' (the surname Butterfield is really derived from one of the many Flemish names in Eastern England, and was originally Bouterfeld, or the 'outer field,' as contrasted with Binnenfeld or Binfield). In transcribing English, as well as various Oriental tongues, it is highly necessary to distinguish between the short *a* sound and the long; the short being represented by the unaccented *alif* or *fatha* in so many Arabic, Indian, or Persian words, as contrasted with the long *alif*. This short *a* is sufficiently near to the English sound of *u* in 'but' as to be represented by the same symbol, *a*, while the long sound is best indicated uniformly by the original type—*a*. If we make this distinction—that is to say, use our existing italic *a* (made erect for Roman type and supplied with an enlarged form as a capital), and reserve *a* in its Roman form with an equivalent italic for the sound of *a* in 'father,' 'hard,' etc. (the Continental *a*)—we shall make phonetic transcription much simpler. Similarly, a convenient symbol for the sound of *a* in 'hat'—a very prominent sound in English and in North African Arabic—is *æ*. This was probably its equivalent, more or less, in Anglo-Saxon pronunciation. The *a* in 'hat' is really a very short pronunciation of the diphthong *ea* or *eö*. *Ea* in Anglo-Saxon was probably pronounced exactly as we pronounce it in 'pear' (or like *a* in 'stare'). In modern English this, however, is perhaps most logically rendered by *eö*, and the fused letters *æ* are best reserved for the short sound in 'hat' or 'mad.'

One cannot consider the question of the phonetic writing of

English without dealing with that of French. I would propose for the peculiar French sounds represented by the diphthong *eu* and the nasalised *e* and *i* in many words, the symbol ɛ, which when nasalised has only to be surmounted by a ͂; thus 'peu' would be spelt *pɛ*, and 'pin' would be written *pɛ̃*, 'bien,' *biɛ̃*, and 'rien,' *rɛ̃*. For the French unaccented *e* as heard in 'le,' 'de,' 'menu,' I would supply a new symbol, a reversed *e* (ə). For the Welsh accented *y* as heard in words like *tŷ* = 'house,' and similarly for the Slavic *y* and ы, I would propose a new symbol (ψ) which by its form suggests something like a union of *u* and *i*. For this peculiar, almost guttural, vowel, which is derived from the Central Asian languages and extends in its modern range almost from China to Poland (its reappearance in modern Welsh is probably an accidental coincidence), is like a mingling of *ü* and *i* (as in 'hit'); pronounced, however, in a very guttural fashion. The vocalised *r* and *z* met with in so many Slavic tongues, and in some of the Indian languages descended from Sanskrit (similar sounds occur occasionally in dialectal English), are best represented by *r̆* and *z̆*. The little mark on the top of this *r* and *z* is my equivalent for the simple vocalisation of a consonant and is nothing but a miniature form of the reversed *e* which I propose for the French unaccented *e*. Very often in writing Bantu languages or in writing English, it is not necessary to insert this little symbol above the consonant which is to be vocalised, for common sense in reading the words suggests this vocalisation. But it will be necessary to use this symbol *above* the line in transcribing many French words exactly as they are pronounced in ordinary speech. For instance, while we must write 'le' and 'sera,' *lə* and *səra*, we must often transcribe 'lettre,' *lettrə*.

In my proposed alphabet I discriminate between the *e* in 'met' and the *e* in 'fête' by the placing of a stress mark over the strongly pronounced *e*, and similarly between *i* in 'hit' and *i* in 'ravine.' Likewise between the *u* in 'put' and the *u* in 'rule,' between *o* in 'store' or 'gone,' and the *o* in 'not' and 'gong.' Some have suggested that instead of writing a stress mark, which, when carelessly made, may be confused with the nasal sign, or perhaps with an accent, it is better to double the vowel which is to be broadly pronounced. But as the result of much practice, I consider that both in printing and in writing it is more convenient to indicate the strongly pronounced vowel

by a stress mark, since the double vowel must be reserved often for a double or repeated pronunciation, which it is inconvenient to indicate by a diæresis.

It would be seen therefore that amongst the vowel symbols I propose there are not many that are completely new to our types. *Ö* is familiar to us through German, but as a matter of fact I think it is most conveniently represented in printing, if not in writing, by ꝺ. The two forms, however, might be allowed to co-exist, both of them equivalent to the sound of *u* in 'hurt.' ε, which I have proposed for the French *eu*, is familiar to many of us through the systems published by the International Phonetic Association.

Ɜ (ꝫ) is best represented in the majuscule (not often required) by Ɜ. The minuscule—already described—is ꝫ (a reversed *e*).

Ɑ is familiar to us in its italic form of *a*. This must be enlarged for the majuscule, and made erect for Roman print. *U* for the French *u* is made familiar to us by German, and ѵ for the peculiar Slavic and Welsh sound already described is so far outside the transcription of other European languages that its consideration need not detain us here, especially as it is not required in transcribing English phonetically and need scarcely be used in Welsh, except perhaps in place of the accented *y*. The ordinary equivalent of the Welsh unaccented *y* is *a*, *ĭ*, or ꝫ.

In addition to these consonants and vowels there is a long list of what I call half-letters; that is to say, signs, accents, tone and stress marks, aspirates, gasps, clicks, nasalisation, etc. ' is the ordinary apostrophe or an indication of an elided vowel, the equivalent of the Greek ' and of the Hebrew א; ; = the hiatus or gasp, the Arabic *hamza*, or the French *h* in 'haut,' 'Sahara.' ' = the light aspirate, the English *h* or the Greek '. It is not a symbol that need be much employed in phonetic writing, as its place is best taken by the ordinary letter *h*. ʕ = the Arabic ع (*ăin*), a faucal or velar contraction of the voice very marked in the Semitic languages and imparting to the vowel that follows an almost snarling sound. It is, however, only a 'half-consonant,' and is best placed above the vowel that it influences, instead of—as it were—breaking up a word by appearing in the form of a consonant. ~ = nasalisation. Thus *ñ* is sounded like the English *ng* in 'singing,' and not like the *n* in 'vanguard.' Nasalised vowels—*õ*, *ã*, *ɛ̃*, *ũ*—are sounded as if

written in French, *on, oin, in, un*. Also *õ, ã, ẽ, ĩ* would be pro-
nounced like the Portuguese *õ, ã, ẽ, ĩ*; or *om, am, em, im*. The
difference between the French and English pronunciations of
'long' are that the French should be written *lõ* and the English
loñ. ' would indicate palatalisation, a faint *y* sound, frequently
met with in the Slavic tongues of Europe and the Hamitic
languages of East Africa. I have already alluded to the ⁹ as the
symbol for the indeterminate vowel sounds, *ĭ, l, š, ž, ṁ, ĭ*, etc.,
in so many Aryan tongues, in Chinese, in Bantu, Sanskrit,
Slavic, etc. It is often heard in English words in the unaccented
vowels, and in the terminal *le*. But I propose to leave it out of
English use as an unnecessary complication, either to write the
consonant without any vowel as *fībl* for 'feeble,' or to represent
it by the vowel it most nearly resembles, *e, a,* or *ǝ*.

Almost the only accent required in transcribing English,
French, and most European languages would be the acute
accent— ´, which indicates the ordinary pitch of an accented
syllable, the rising tone of voice. The assumption in writing
all tongues will be that the customary pronunciation is to accent
the penultimate syllable in all words of more than one syllable.
It is only where these rules are departed from in accentuating
the first or last syllable that this accent would be required.
The other accents, of which I supply a good many forms in my
book, are for the most part only required in transcribing certain
West and Central African languages, Chinese, Burmese, and the
languages of Indo-China. The symbols of stressed and marked
unstressed vowels are the familiar ‾ and ˘. I have already
referred to the equivalents of the clicks, and thus in this sketch
I have more or less covered the whole range of 'recognised
phonetics. It might, however, be convenient for the reader to
set out succinctly the full range of the phonetic alphabet I
propose, with its equivalents as nearly as possible in old-
fashioned English spelling.

Half-consonants :—

   ' = apostrophe for an elided letter or indication of initial
        utterance of vowel, like Arabic '.

   ı = the hiatus or gasp between two letters, and French
        'aspirated' *h*.

   ‘ = the light aspirate (Greek ʽ).

   ʕ = the Arabic ʕ.

   ‾ = nasalisation.

ı = palatalisation.

ꙍ = indeterminate vowel.

ˊ = the acute, ˋ the grave, and ˄ the 'intense' accents.

ˉ = stress on a vowel, and ˘ unstress or special tenuity of sound.

C̦ = the dental click in Zulu, etc.; ᗡ, the alveolar; C̦, the palatal; and C̦ the lateral.

Consonants :—

    m, b, w, v, p, f, s, z, j, d, t, n, l, r, y, k, g, and *h* as in English; ʂ like English *sh*; ᶎ like French *j* or *z* in 'azure'; *c* like English *ch*; *đ* for the *th* in 'that' and *ƀ* for the *th* in 'theory'; ȿ, ż, ᶌ, ᶑ like peculiar Arabic sibilants and dentals (ض, ط, ظ, ص); χ or *x* for the Scottish and German *ch*; χ̣ for the Polish *ś* and the German *ch* in 'ich'; *r* for the velar *r* (the Northumbrian 'burr'); *ř* for the vocalised *ř* (the 'Midland' *r*); γ for the Arabic *ghain* (غ) often expressed in English *gh*; *q* for the Arabic ق; ñ for the *ng* in 'singing'; *ł* for the Polish *l*; and *ħ* for the strong Arabic *h* (ح).

Vowels :—

    ō, o; ө, ɸ, or *ö*; ꙍ, ꙍ; ω; *a, a*; æ; *e, ē*; *i, ī*; ѱ; *ü*; *u, ū*.

So much for the system of scientific spelling which—borrowing from many sources and adding a few original suggestions of my own—I have published in my book. I believe that this will be found in every way the most convenient alphabet for transcribing all African, Asiatic, and Amerindian languages which are now being put into print. It will, perhaps, have been already noticed by one or two critics that my alphabet looks a good deal simpler than that which is in use by certain German philologists for transcribing African languages — philologists who attempt to discriminate between three or four different ways of pronouncing the letters *t, d, z, n, r, l,* etc., in Bantu or Sudanese Africa. I have given, perhaps, equal time and attention to the consideration of this problem, and I have decided that to mar one's print and tire one's readers' eyes with an infinitude of diacritical marks above or below a consonant is a useless preciosity. It must be taken for granted that Africans, as well as Asiatics and Europeans, do not always clearly enunciate their words; also, that there is great individual variation (within a certain degree of range) in the pronunciation of consonants,

We have the same ·in our own country. Look, for example, at the widely different pronunciations of the letter r throughout Great Britain and Ireland. The r in the speech of cultivated people, especially in London and Oxford, at the great English centres of education, and in Southern England generally, is completely elided in many words, and its elision has been carried to such an extent in past decades that, in transcribing the fashionable utterances of the 'sixties and 'seventies, it was often represented by a w. We still meet with people in what is called conventionally 'good society,' who say 'vewy' or 'vey' instead of 'very,' and 'bwait' instead of 'bright.' In the Midlands the r is pronounced wherever written, but often with a peculiar cerebral or palatal growl, unmistakable to those who have heard it, easy to imitate, and equivalent to the vocalised $\dot{r}$ of Indian and Slavic speech. The r of Northumbria is burred or pronounced with the velar palate, like French r in *grasseyé*. The r of Scotland and Ireland is more or less strongly trilled. Then again, the t, which varies so much in Bantu Africa, varies a good deal in Great Britain (in dialect), being sometimes pronounced like d, sometimes as an actual hiatus, and even as an r. Well: similarly, in Bantu and Sudanese Africa it is occasionally difficult for a listener to determine whether the speaker is uttering an r, a d, or a t. The t is sometimes strongly aspirated. But I hold that as long as one writes it t when it is most like a t, d when it is most like a d, and r when it most like an r, it will be quite sufficiently discriminated, and I take the same line in regard to other consonants ; a reasonable line, in view of the mutability of human speech and the unreasonableness of expecting any student of a foreign language to be able to speak that language so as to give his hearers the impression that it is his native tongue. Of course there are cases where a man or woman has lived a long time in a foreign country and caught up, like a child, the exact local pronunciation of the local speech. But it is well-nigh impossible to teach any one such perfection of imitation by book study ; and the multiplication of symbols to indicate every conceivable grade of utterance will only embarrass students and deter them from studies which appear too difficult. The discrimination, as it is, between the dental and the alveolar s and z, d and t, between the ordinary and the Polish or Welsh l's, the χ and the $\dot{\chi}$, the r and the $\dot{r}$ and $\ddot{r}$, has been carried quite far enough. We wish to aim at an alphabet

sufficiently copious to reproduce human speech—standardised human speech—by a series of easily written and printed symbols of unchanging application ; but it is not necessary to carry our accuracy to a ridiculous extreme by supplying tedious equivalents for every slurred or hesitating utterance. It is this preciosity which has done so much to prejudice busy people against phonetic spelling, or which is driving them into the opposite camp of the Indian Government or Royal Geographical method, one which makes no pretence at being either logical or exact.

Now comes in the question whether or not we should change the official spelling of our own tongue—English—and adopt some such scientific orthography as that set forth in this article. The reasons against doing so do not seem to me very adequate. They are usually three in number.

(1) That the phonetic spelling of English must first of all depend on what is to be regarded as the standard pronunciation. If we render it phonetically and logically as it is spoken by educated people in London and Oxford, such a pronunciation is at once out of keeping with that which is in vogue even amongst educated people in Scotland, Ireland, or America, to say nothing of the wide difference between the pronunciation of academic English and dialectal English.

(2) That in spelling English phonetically we may lose count of the extremely interesting historical etymology of words.

(3) That the revolution would be so great, so tiresome, so productive of printers' strikes, that it would altogether outweigh the gain in simplicity and the saving of trouble to children and foreigners.

As regards the first objection, I admit that a standard pronunciation must be determined by some committee or educational body whose decision would secure acceptance, at any rate amongst the majority in the United Kingdom, in the United States, and in the great dominions under the British Crown. But once having fixed this standard pronunciation, the whole mass of English-speaking peoples of the world would in course of time adhere to it more or less, especially as it became adopted in their schools. Supposing, however, that the United States, out of national pride, refused to accept the standard of this British committee and set up a standard of its own. American pronunciation, nevertheless, at the present day does not differ more from the pronunciation of the conventional, correct English

of London than the latter differs from Scottish, Midland, or Irish English. Even if some English words were differently written to agree with local pronunciation in the United States, their meaning would be rapidly grasped by any one who read them phonetically. The probability is, however (in view of the importance of the subject and of the language) that, especially if the United States was well represented on this commission (together with the Dominions) there would be universal acceptance of the standard.

The argument as to the loss of historical etymology, etc., is mostly rubbish. The spelling of English in the early 18th century is appreciably different from the spelling of English at the commencement of the 20th century, and that again differs from the conventional spelling in the time of Shakespeare, or in the reign of Henry VII. Still more marked is the divergence in orthography between the period of Chaucer and the present day, the fact being that the spelling of English has insensibly, but continuously, altered as century succeeded century. There is far more hope of its stability if a standard of pronunciation was fixed and the spelling was made to conform logically with that standard.

I admit the trouble that will be caused by the change, but in my book I have attempted to explain how in many ways that might be avoided or lessened.

On the other hand, the gain would be great. The logical spelling of English is the one obstacle which stands in the way of our tongue becoming a universal world-speech and knocking the stuffing out of inventions like Esperanto, inventions which seem almost as horrible to me as would be some artificially manufactured human being, something more wonderful and self-acting than the manikins put before us by Maskelyne and Devant. Much time and many tears would be saved the childhood of the coming and of future generations by a simplification of spelling. I have shown in my book that the new spelling is practically as easy to write as the old; it is far easier to print, and still more easy to read. To convince the reader on all these points, I would venture to refer him to my book on Phonetic Spelling.

II.—By Sir RONALD ROSS, K.C B , F R.S., D.Sc.

THE subject of spelling reform does not directly concern science, but is of sóme indirect importance to it, as to óther forms of intellectual effort, on accóunt of mischief caused by óur present irrational 'orthography'—which distracts óur children, impedes the léarning of English by foreigners, wastes abóut ónetenth of the móney spent on printing and writing, and assists the disintegrátion of óur pronunciátion. Unavailing efforts at reform háve been made dúring sóme centuries. Years ago Pitman and Ellis poured óut large sums on the cause, and scores of reformers háve invented scores of systems which théy advocated as substitutes for the óne in use—áll quite frúitlessly. More récently, however, the creátion of the science of phonetics and the teaching of it in sóme schools and universities, the establishment of the International Phonetic Associátion, and of Mr. Carnegie's spelling reform committees in Britain and the States, and especially the official adoption of sóme smáll changes by Mr. Roosevelt in America, háve suggested hopes of better fortune in the fúture. Still more récently, books tóuching the subject háve been published by two distinguished men. Sir Harry Johnston, whóse article is printed abóve, has álso gi̇̈ven us an interesting book on Phonetic Spelling (University Press, Cámbridge), in which he suggests a good scheme of international spelling applicable to áll languages, including the African tóngues which he has studied so well; and the Poet Laureate has written a witty and pregnant tract on the Present State of English Pronunciátion (Clarendon Press, Oxford), in which he cálls attention to sóme of the vulgar degradátions of óur speech and suggests anóther phonetic scheme (applicable to English alone).

My own excuse for adding a note is that I wish to make yet anóther suggestion, which, I belíeve, has never been made before in spite of the immense amóunt of matter written on the theme—and I think that dúring mány years' attention to this cúrious side-bránch of húman endéavour, I háve stúdied every important propósal which has been advocated. I shoúld say first that the failure of these propósals has been due, in my opinion, to twó causes. The first is that the Anglo-Saxon mi̇̈nd, whatever its merits may be, is extremely illogical—so that its illogical spelling is really an accurate expression of itself. This

quálity springs from mental indolence, which is unwilling to face new thoúghts, and leads to mental subservience, which for ever finds rest in dogmas. Our spelling has thérefore becóme a dogma, which, like óther dogmas of óurs, the national intellect dóes not possess enoúgh energy to bréak throúgh, however exigent and obvious may be the reasons for dóing so. The second cause for the failure of spelling reform is that such a large number of álmost équally good new schemes may be suggested that thére is gréat difficulty in selecting the best óne —much more so in obtaining unanimity of choice; and it is absurd to suppose that the public will make ány change until this point is decided. Thus the óld spelling easily hólds its gróund in spite of áll attacks.

I classify áll the préviously suggested schemes as follows :

(1) *The Deletory Scheme,* which merely consists in dropping useless letters, as in such spellings as *ar, hav, wil, hed, peple, beuty,* etc.; withóut making ány óther change.

(2) *The Emendatory Scheme,* which consists in substituting good for bad letters, withóut attempting ány complete revolútion—as in such spellings as *haz, woz, duz, luv, whot,* etc. This is generally propósed in addition to the prévious scheme.

(3) *Old-Letter Homographies,* which aim at rendering each sóund in óne way, withóut the introduction of new letters. This cláss is divided into twó groúps, (*a*) *digraphic* schemes, in which móst of the longer vówels are uniformly expressed by dïgraphs, as in *bait, beet, biet, boet, boot,* etc., whether the dïgraphs are based on English or continental values of vówels; and (*b*) *diacritical* schemes, which use marked or accented letters for sóme of the vówels, such letters being supposed to be alréady available for printing.

(4) *New-Letter Homographies,* which effect the same purpose by using, in the place of dïgraphs or marked letters, new letters in addition to those contained in óur present alphabet. These schemes may be either méant for English use ónly, such as Dr. Bridges' system ; or may be international, such as Sir Harry Johnston's one.

The twó first schemes coúld be employed at ónce—álmost withóut discussion, because the reasonableness of the proposed changes in détail is unquestionable. Théy woúld produce a very gréat ameliorátion of óur spelling ; woúld entail no extra cost for new letters, and woúld indeed save a vást sum of móney

every year in the nátion's printing bill. Théy áre not employed ónly because of public inertia and because of the opposition of a few peóple whó imagine that théy may change the spirit of the language. With regard to the third cláss of schemes, adoption is much more difficult owing to the necessity of selection. Literally a score of good schemes may be devised under this héading, each possessing sómething to commend it. The system of the Simplified Speling Soesiety belongs to the digraphic groúp, but, like óther systems of this groúp, has the defect of using mány letters and of failing to indicate the syllabic stress—which is just as important as the length of the vówels, and which can be easily gīven by well-arranged diacritical systems. The latter groúp álso saves móney in printing, but requires the insertion of marks in writing and typing. The new-letter systems áre, of course, ideálly the best, but áre usually so expensive and troúblesome to print that théy cannot be used at ónce. Théy álso require selection; and, moreóver, such excellent diacritical systems may be devised that the necessity for costly new letters is not álways appárent. Few of the proposed schemes (apart from strictly phonetic ónes) ever attempt to indicate the syllabic stresses.

The scheme nów suggested by me belongs to nóne of these clásses. In its simplest form it consists merely in the introduction of a diacritic to mark the syllabic stress on certain vówels, *withóut making ány actual change at áll in the accepted spelling*. The rúle under which this is dóne serves, not ónly to indicate the accent in mány wórds, but álso to gīve the quálity of the vówels in óthers, or, at least, to show whére irregularity occurs. The scheme dóes not of course perfect óur spelling, but it impróves it gréatly withóut áltering it. If ánything, it adds elegance to it, especially in verse; and can be employed at ónce in printing with little additional cost. The scheme can be extended by the employment, if we please, of more than óne diacritic, and will thus serve as an introduction to more ambitious schemes. Combined with the first twó schemes mentioned abóve, it will gīve us whát is álmost an homography in place of our present jumble.

Let us begin, however, with the simplest form, and suppose that ónly óne diacritic is allówed. The best mark—the easiest óne to write and the móst elegant in print—is the acute accent.[1]

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[1] Except on *i*, where it may be replaced by the diéresis.

I propose then that we shoúld first lay down a general, but sómewhat arbitrary, rúle regarding English vówels, and then *mark ónly those vówels which do not conform to it.*

The vówel symbols *a, e, i, o, u* may háve in English when stressed no less than five different groúps of values, which I classify as follows:

(1) *Long idiomatic* values, as in *mate, mete, mite, mote, mute.*

(2) *Short idiomatic* values, as in *bat, bet, bit, bot, but.*

(3) *Orthoëpic* values, as in *fár, fáther; gréat, véin, béar, fête; prïest, field, polïce; bóught, bróad, bórn; fúll, púsh; rúde, trúth.*

(4) *Degraded* values, which áre númerous and irregular. The commonest occur, especially áfter *w* and *qu*, when orthoëpic *a* degenerates into sóme value of *o*, as in *wás, whát, yácht, wánt, wánder, wár, áll, áwl, cáught;* when *o* degenerates into sóme value of *u*, as in *móther, óne, flóod, dóst, wórd, whó, to, wóman, tómb, góod, fóod;* and when *er, ir, ur* take nearly the same value, as in *her, fir, fur.*

(5) *Silent* values, as in *heàd, madè, recèive, peòple, gùard.*

Now let us assume the following general Rúle:

Stressed vówels shoúld háve long idiomatic values when fínal, before óther vówels, and in the lást sóunded syllable of wórds ending in *e* mute and théir derivatives[1]: ótherwise théy shoúld háve short idiomatic values.

If this Rúle is obéyed, the accent is not marked: if it is infringed, the accent is marked on the offending letter.

Thus the accent shoúld be marked on áll orthoëpic and degraded values; on short idiomatic values before vówels, or in the penultimate of wórds ending in *e* mute and théir derivatives; and on long idiomatic values placed ótherwise—that is, in the exceptions to the Rúle.

This serves to indicate bóth stress and length of vówel in a vást number of wórds, such as *náture, natural, nátion, national, fúture, futúrity, study, stúdious; dúnce, flánge, revénge, askánce, scónce; mïld, mïnd, góld, móst,* etc., especially if subsidiary rúles áre adopted regarding the effect of suffixes (which I háve no space to deal with here).

It álso fixes the pronunciátion of móst of the númerous irregular vówel-dïgraphs which at present cause such confúsion

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[1] It may be better to lay dówn that vówels shall be long before a single consonant followed by *any* vówel. This will serve to indicate the accentuátion on a gréater number of words.

in óur spelling—for, if such dïgraphs áre stressed, the accent shoúld be marked upon théir first vówel if this is short or irregular, but not if it is long. We thus háve *ail, áisle, aye* (ever), *áye, say, sáid, grease, gréat, breathe, bréath, read, réad* (p.p.), *ear, éarth, tear, téar* (verb), *steer, stéad, stéak, receive, belïeve, ceil, yïeld, pïerce, véin, théir, obéy, peóple, léopard, jéopardy, pie, pïece, denied, nïece ; know, nów, bow, bów* (obéisance), *bóugh, roe, row, rów* (noise) *soul, soúght, thou, boat, bóard, bróad, though, throúgh, youth, yóung, coúld, roúte, floód, doór, beaúty, adieú* (whére a whole polygraph is irregular the accent shoúld be marked on the lást letter concerned).

It álso indicates the presence of orthoëpic or degraded values. The accepted spelling generally expresses the long idiomatic values either by dïgraphs or by *e* mute, at least in monosyllables and théir derivatives, except in a few wórds such as *mïld, mïnd, pïnt, sïgn, móst, óld, wónt.* Except in these, thérefore, the accent in monosyllables will denote orthoëpic or degraded values. Before *s, n,* and often *l,* marked *á* generally indicates the long orthoëpic value (at least in Standard English), as in *páss, cást, ánswer, dánce, cálm, hálf, enchántment*; ótherwise it indicates degraded values, because thére is no digraph or *e* mute to suggest long idiomatic values. The short *o* is álso often lengthened, especially before *s,* as in *loss, lost, off*; but as this pronunciátion is very variable, I do not mark it.

Before single *r,* followed by a consonant, or fïnal, *a* and *o* generally háve orthoëpic values, and *e, i, u* degenerate to the *ur* sound. I mark thérefore ónly the exceptions as commonly pronóunced, such as *stárry, glóry, stóry.* *Ore,* and *oar* and their rhymes are so variously pronounced that they also need not be marked.

We may álso excuse the mark whére the quálity of the vówels is fixed; that is, on *au* and *aw*; *oi* and *oy*; *oo,* long and short; *i* before *gh*; *a, e, o, u* before fïnal *-tion* and *-sion*; and in the twelve common constructive wórds *tó, you, your, whó, whóm, whóse, óur, théy, théir, áre, wére, háve*—especially the first, as in this article. The object of such omissions is to save tróuble in writing and the excessive use of the marks in print; and if áll the omissions just suggested áre allówed thére woúld be mány fewer accents than háve been employed here, whére, of course, théy áre required for an exemplar. A still gréater simplificátion woúld consist in using the marks ónly for the

idiomatic vówels, whére needed, and letting the óthers look áfter themselves. The degraded vówels of course disappear if we use the emendatory scheme as well.

Accents may álso be neglected on capitals; and the mark of diéresis may be used on *i*, instéad of the acute accent, which dóes not look well on that letter.

It may be thoúght that much confúsion will still be caused by the employment of the same mark for so mány values; but the confúsion is not so gréat as might be expected, because the different groúps of values tend to occur in different clásses of wórds. Thus the marks on idiomatic values áre required principally in polysyllables, and those on the óther groúps, chíefly in monosyllables.

Of course we can be much more exact if we áre allówed more than óne diacritic. A good plan is to use the diéresis, whére required, for the long idiomatic values (sómewhat as in German), and the acute accent for the óther values ónly; and this gíves much gréater accuracy withóut much change. The grave accent may álso be used for irregular *unstressed* values and for sílent letters. But the difficulty is that the employment of mány díverse marks makes the printing unsightly—as will be observed on comparing a page of French with óne of Spanish, with its álmost exclúsive and elegant use of the acute accent.

But I cannot discuss áll the détails here. My main point is to suggest that English spelling coúld be gréatly impróved by the introduction of óne or éven more diacritics, withóut making the alterátions which offend so mány peóple. At áll events, the marks woúld serve to cáll attention to existing defects, and thérefore to encoúrage efforts to remedy them.

REVIEWS

No Struggle for Existence: No Natural Selection. A critical examination of the fundamental principles of the Darwinian theory. By GEORGE PAULIN. [Pp. xx + 261.] (Edinburgh: T. & T. Clark, 1908. Price 5s.)

WE can infer from the mere title of this book that the author has not only undertaken a critical examination of the Darwinian theory, but has established its inaccuracy; and from the paper wrapper of the book we learn that he "*proves* that Nature has made special provision for eliminating all excess of reproduction so as to avert a Darwinian struggle, and that individual qualities or variations play no part in her elimination. His second chapter is devoted to a *demonstration* that Nature does not make use of individual variations to originate new forms. The second book, dealing with the Law of Population, *shows* that neither Malthusian nor Darwinian principles affect, in any wise, the movements of population." The italics are ours, and the italicised words "prove" only the author's self-confidence. On looking through the twenty pages of preface we find nothing but repetitions of the same statements. He states that he has been a lifelong evolutionist, but that he has now altered his previous convictions; that he believes in a moral basis to the universe, and is therefore convinced that "Darwin's conception of the cruelty of Nature to her sentient offspring is wholly mistaken." Darwin's theory, he says, is "an extraordinary concatenation of weird concepts of sins against logic and common sense, of criminal violations of Nature's known laws, and of audacious and indefensible assertions. My investigation proved it to be so—a rotten tenement tottering in its every joint, a ship tumbling helplessly on the brine, leaking at every plank." He says that he wishes to "counteract, in short, that gross and degrading materialism which Darwin has gone far to make the recognised stamp of present-day scientific thought." But even after twenty pages of preface, and twenty more pages of the first chapter of the book, we still fail to ascertain the nature of this remarkable "proof." We then learn that there is no struggle for existence amongst animals, because of the destruction of their young offspring by the ravenous males! When the population becomes crowded, the females cannot hide their young sufficiently easily from their unnatural mates; when, however, the population becomes thinner, they succeed in doing so. Thus the numbers of animals are maintained by Nature always at about the same level. Thus also there is no struggle for existence, and consequently no natural selection on the principles enunciated by Darwin. The author does not, apparently and fortunately, extend this explanation to the cases where a human population remains fixed; but here he introduces another hypothesis, to the effect that the birth-rate declines when the food supply does so. The evidence which he adduces for both these arguments is of the slenderest nature; but worse than that, he seems to have failed to understand Darwin's meaning. He takes Darwin's metaphorical expression "struggle for existence" in a literal sense, and seems to imagine that animals do nothing but fight each other for their food. Cases such as those of innumerable insects, of which the population remains limited though

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they have unlimited food and though they cannot possibly destroy their offspring, do not concern him ; and he reaches his proofs and demonstrations with the security of those who start with preconceived ideas. If the man of science should be defined as one who is engaged on the laborious task of fitting theories to many facts, his opposite, the dogmatist, may be defined as one who is engaged upon the easy one of fitting facts to many theories. Surely his theory of the destruction of the young by the males is, if anything, more revolting than the most horrible struggle for existence suggested by Darwin ; and the attempt to fix a charge of immorality upon scientific theorems with which we do not agree is itself of doubtful morality.

O. A. CRAGGS.

A Beginner's Star-Book. By KELVIN McKREADY. [Pp. 148 ; 70 illustrations, including charts, etc.] (London : Knickerbocker Press, 1912.)

THIS book is written for the use of beginners whose instrumental equipment ranges from an opera-glass to a 3-in. telescope. It contains a series of night-charts of the sky at intervals throughout the year, which, together, practically serve as a planisphere. For any given date there are two charts, depicting the sky as seen by an observer looking north and south respectively, each accompanied by a concise general description of the constellations and stars in it. Opposite each chart is a corresponding key-map, with notes of the objects of more especial interest to observers with a field-glass, a 2-in. or a 3-in. telescope. For more detailed information cross-references are given to a compact but very useful Observer's Catalogue. It is hoped that by this method the beginner will be able more easily to identify the various objects which he sees than when he has only the usual form of printed map, covered with lines and symbols, with which to compare the sky before him.

Subsequent chapters describe simply and briefly the chief points of interest for the observer in the sun, moon, and brighter planets ; and tables are given of the position in the sky of Venus, Mars, Jupiter, and Saturn month by month until the year 1930. Practical hints are also given as to the choice of a field-glass or telescope.

The paper and printing are both good, while the many beautiful reproductions of recent astronomical photographs cannot be too highly praised. Those of the moon may be specially mentioned. It is to be hoped that this interesting and practical book will achieve the author's purpose in stimulating the interest of the beginner sufficiently for him to pursue the study of the subject further, and to seek fuller information elsewhere.

H. S. J.

Qualitative Determination of Organic Compounds. By J. W. SHEPHERD, B.Sc., A.R.C.S. [Pp. xvi + 347.] (London : W. B. Clive, 1913. Price 6s. 6d.)

THE volume is one of the numerous examination text-books issued by the University Tutorial Press, and is intended for the advanced science student. It is divided into two parts, dealing respectively with the tests for the various groups of organic compounds and the various types of organic reactions. The scheme of identification (Chapter XX.) is the result of many years' experience in this class of work, and, with the scale of melting and boiling points, will probably be found to be the most practically useful.

It seems a pity, however, as the qualitative tests for organic compounds are given so fully, that a short résumé of the methods of quantitative determination was

not included in place of the second part of the work. The subject is so closely allied to the separation of mixtures. There are one or two books dealing with quantitative determination, but there is room for a complete work on the elementary methods of organic analysis. In its present form, however, it should be of service to the examination student. H. S. S.

The Control of Water as applied to Irrigation, Power, and Town Water Supply Purposes. By PHILIP A. MORLEY PARKER. [Pp. vi + 1055, with full diagrammatic illustrations.] (London : George Routledge & Sons, Ltd., 1913. Price 21s. net.)

ALTHOUGH the title of this book is almost alarming in its comprehensiveness, it is only fair to say that in this closely printed volume of more than one thousand pages, a fairly successful attempt is made to produce a manual covering all the ground which is generally necessary for engineers in practical work ; and the author certainly displays both judgment and industry in the collection and arrangement of his material.

The book presupposes the usual training that any educated engineer receives at the present time in the subject of hydraulics at a technical school or college ; and there is a good deal to be said for the view of the writer, that results from well-conducted observations are more accurate than the assumptions made in most modern mathematical treatments of hydraulics : indeed the author might have gone farther and said that there is no really accurate and scientific basis of practical hydraulics since there is practically no such thing as steady motion in a large number of the most important cases with which the hydraulic engineer has to deal.

The subjects of critical velocities, capillary elevation, and velocity of percolation dealt with in the second chapter are well treated. In the third chapter the gauging of streams and rivers shows that the author himself has practical acquaintance with the subject, although he does not deal with one or two of the best modern meters.

Pressure tubes are clearly treated : and the modern methods of chemical gauging are more fully dealt with than anywhere else, although it is doubtful if such methods would be allowed in many waters.

The theory of Venturi meter and results with it are also well treated. It is of course impossible in the space available to comment at length on the various chapters which deal with the questions of Gauging by Weirs, Discharge of Orifices, Dams and Reservoirs, Pipes, Open Channels, Filtration and Purification of Water, Problems connected with Town Water Supply, Irrigation, Movable Dams, Hydraulic Machinery other than Turbines, Turbines and Centrifugal Pumps : concluding with the chapter on Concrete, Ironwork, and Allied Hydraulic Construction ; but it may be said that the treatise is worthy to take its place as a standard one among the literature of water supply.

Wireless Telegraphy. By C. L. FORTESCUE, M.A. [Pp. vi + 143] (Cambridge : at the University Press, 1913. Price 1s.)

THIS little book is written for readers possessing general scientific knowledge who may be anxious to know something about both the accomplishments of wireless telegraphy and the means by which results have been obtained.

The first four chapters are devoted to explanations of the electrical phenomena concerned, and the last seven to a general survey of the applications of wireless telegraphy.

The fifth and sixth chapters are devoted respectively to the transmitting and receiving instruments employed. It may be said at once that the matter is dealt with throughout in an elementary and instructive manner, and entirely fulfils the object of the writer. One excellent feature is the clear way in which the processes of wireless telegraphy are made more simple by analogy with hydraulics, though in a future edition the picture of the hydraulic model of a condenser should be re-drawn with a little more care in order to make clear which are the pipe arrangements and which are the cylinders.

Continuous Beams in Reinforced Concrete. By BURNARD GEEN, A.M.I.C.E., M.S.E., M.C.I. [Pp. 210, illustrated.] (London : Chapman & Hall, Ltd., 1913. Price 9s. net.).

THE subject-matter of this volume is rather more limited in its scope than the title would lead one to expect, consisting as it does chiefly in a series of diagrams and tables dealing with the theoretical Bending Moments, Shears and Reactions in continuous beams of reinforced concrete, and their supports, though the results are in general equally applicable to any other form of continuous girder.

The aim of the author is to place in the hands of the designer of such structures as warehouses and other buildings in which a great many of such reinforced concrete beams are employed a set of tables from which he can deduce by a simple operation the Bending Moments, Shearing Forces and Reactions for any system and any intensity of dead and live loads, thus avoiding the laborious calculations entailed on the application of the Theorem of Three Moments to each individual case. This end is accomplished fairly comprehensively by reducing to standard spans and intensities of loads.

All results are calculated from a consideration of the General Theorem of Three Moments, which is enunciated and proved in a clear manner in Chapter II.

There is a wealth of diagrams covering almost every possible case of loading over 2, 3, and 5 spans, and on a scale sufficiently large to be of use ; but it would be of advantage if a few words of explanation were appended to some of them, as it is now necessary to count the number of spans in diagrams 1 to 39 in order to ascertain which case is being treated.

There are short chapters dealing with the utility of haunches in coping with the excessive negative Bending Moments at supports, the effects of support subsidence on the stresses in the beams, etc., and interesting paragraphs on the insufficiency of the usual formula $\frac{wL^2}{12}$ recommended by the Institution of British Architects for the Bending Moments at centres of spans and supports in the case of rigid beams on rigid supports, and on the extent to which the columns may be assumed to withstand bending. Examples of the method of application of the tables are given, from which it appears that the necessary calculations are very simply made ; and no doubt this work will find its place in the drawing offices of those who are engaged in the design of this increasingly important class of structure.

Man and His Forerunners. By PROF. H. VON BUTTEL-REEPEN ; authorised translation by A. G. THACKER. [Pp. x + 96, 8vo, with a frontispiece, 70 figures in the text, and 3 tables.] (London : Longmans, Green & Co., 1913.)

THE last few years have witnessed a tremendous growth of interest in the earliest remains of mankind. This no doubt has been due partly to the normal

growth of scientific knowledge, which is ever adding new significance to old material, and transmuting the dry technicalities of anatomy and geology into a more or less intelligible story of Man in the making, or Nature's attempts at man-making, that naturally appeals to all mankind. But fresh fuel, often of a highly inflammable kind, has been repeatedly added to this flame of popular interest within recent years as, one after another, surprising fragments of ancient types of man and his handiwork have come to light.

Naturally enough, with this rapid growth of knowledge and constant conflict on the part of the pundits as to the meaning of each new fact that is brought to light, there is a constant demand on the part of the intelligent public for information concerning the progress made and for some light on the significance of the new knowledge of our earliest human forbears and their relations. A host of small books of a more or less expository nature have been issued to meet this demand within the last few years. There have been new editions of such standard treatises as those of Ranke and Haeckel, and smaller new books dealing specifically with this problem of man's origin, such as those written by Leche, Branca and this work of v. Buttel-Reepen's ("Aus dem Werdegang der Mens-chenheit") on the Continent, and the books by Sollas, Keith, Duckworth, McCabe and others in this country.

The English version of v. Buttel-Reepen's work has been brought right up to date by giving a full summary of Dr. Smith Woodward's and Mr. Charles Dawson's account of the Piltdown skull, perhaps the most surprising type of very early man yet discovered.

Every one who has read anything whatever of the recent literature relating to early man must be aware that at the present time there are very considerable discrepancies between the views of different scholars as to the relative values and precise significance of the various remains of fossil men.

Since characteristically human remains such as the Heidelberg and Piltdown specimens must be referred back to the commencement of the Pleistocene period, it seems quite certain that man must have lived in the Pliocene period. So much, I think, will be granted by most scientific men who have given any thought to this problem ; but what most of these authorities are not yet convinced of is whether such traces of man and his works, the existence of which they do not doubt, have actually been found, as Rutot, Verworn, Ray Lankester, and Keith, among others, believe, each in his own way.

In the little book before us, which is written in a delightfully clear and simple style, the writers (there is no indication whether Prof. v. Buttel-Reepen is wholly responsible or Mr. Thacker shares also in this result) display the utmost catholicity in their acceptance, partially or wholly, of the views of those whom other writers regard, collectively or individually, as extremists. They accept Verworn's evidence of Upper *Miocene* man ; go the whole way with Rutot ; and set forth Klaatsch's extraordinary speculations concerning the kinship of different human races with the various species of anthropoid apes as quite serious contribu-tions to the discussion, although they add at the end that " it would be well to take the theory *cum grano salis*."

The whole book, in fact, may be regarded as a pleasantly written, wholly uncritical, and very credulous summary of recent literature dealing with early types of mankind ; and the reader who enjoys this delightfully unfettered romance should remember that he ought also, as a corrective, to refer to the original sources of information which appear in the bibliography at the end of the volume.

The book bears the obvious impress of its origin. There is hardly any

reference to the important Gibraltar skull, and the translator makes certain passages unintelligible to any except the expert by his ignorance of anatomical terms. The worst instance of this is the use of the expression "third lobe of the brain" (p. 49) for the third frontal gyrus.

G. ELLIOT SMITH.

Modern Electrical Theory. By NORMAN ROBERT CAMPBELL. [Pp. xii + 400.] Second edition. (Cambridge University Press. Price 9*s*. net.)

A CAREFUL comparison of this second edition with the first edition (1907) fully confirms the author's statement in the preface that this is really a new book; even in the places where the work of the last six years has not added to or much affected our knowledge, the book has been rewritten and recast. A mention of some of the remarkable experiments and revolutionary theory of the last six years which are discussed will make it clear how completely a recent book on electrical theory must necessarily differ from one six years old; reference need only be made to Planck and Einstein's theory of light quanta, Nernst's work on specific heats, the experiments of Barkla, Bragg, and Lane and his collaborators on X-rays, and the principle of relativity. This work is all too recent to have found its way into the text-books, and the papers and pamphlets on it are enormous in number, scattered, and not always particularly clearly written. Whether they are to stand or fall, these modern theories of light and electro-dynamics in general are far too important for any physicist to be able to ignore them, and a book where he can get a general yet correct presentation of them, and find them compared with the older theories, is badly needed, although it may be, probably will be, out of date in another five years. We can congratulate the author both on his courage in attempting such a book, and on the successful result; for, on the whole, the book gives a presentation of just the nature required by the working physicist, neither too "popular" nor too mathematical. If he shows a disposition to try to bully the reader into an acceptance of every view which has won his own belief, it must be remembered that a certain amount of personal opinion and partisanship is probably necessary to give unity to the book, and to make it the connected presentation it is rather than a mere collection of independent theories and observations.

The book is now divided into three parts—the electron theory, radiation, and electricity and matter. In the first part, besides a good account of the Faraday-Maxwell theory and the electromagnetic theory of dispersion, there is an account of many important matters not treated at all in the standard English books; especially needed is the chapter on the electronic theory of magnetisation, giving an account of the work of Langevin and Weiss. Elsewhere, in the treatment of conduction, we think the author might point out the difficulty of supposing electrons to be gas-kinetically reflected from atoms and molecules, considering that experiment points rather to their being absorbed and subsequently liberated by the molecules, a very different mechanism which, we think, may possibly form the basis of a more complete theory.

Two chapters in the second part of the book contain an interesting and able discussion of the relative merits of the wave theory and Einstein's corpuscular theory of light, and of the nature of X-rays, in which it is made clear that while modern experiment seems to have conclusively established that X-rays are essentially similar to light, the nature of both light and X-rays is very doubtful. It may be mentioned that Lane's and Bragg's X-ray photographs of 1912 receive

adequate reference. The electrical mechanism by which light is emitted from the atom or molecule is, however, not so adequately treated. While Stark's theory that positively charged atoms emit the line spectra can be reconciled with Wien's observations on canal rays, there is no good confirmation of it, and in a paper not mentioned by the author Baerwald (*Annalen der Physik*, 34, p. 883, 1911) from modified experiments on the Doppler effect in canal rays comes to the conclusion that the carriers of the series cannot be positively charged, but are in all probability neutral atoms which emit light at the moment of neutralisation by an electron, in accordance with the theory developed by Lenard in his work on phosphorescence and elsewhere, and adopted by Wien for canal rays. There also seems little doubt that line spectra are to be attributed to atoms, band spectra to molecules, which hypothesis will account for the emission sometimes of lines, sometimes of bands by the same element according to conditions, a fact which the author describes as unexplained.

In the third part of the book a chapter is devoted to the structure of the atom, in which, we think, an unnecessary amount of attention is given to Stark's theory, which has not proved particularly valuable, and which for those interested is easily accessible elsewhere (in Stark's *Atomdynamik*): there is no mention of Nicholson's work. The last chapter is on the principle of relativity. The author begins by giving the Einstein transformations, and does not state the physical reasons which led up to them, or the physical assumptions underlying them, until he has deduced their chief results; this seems rather unsatisfactory for those approaching the subject for the first time. Again, we do not think he gives quite a fair account of the obstacles in the way of acceptation of the principle in its present form, at any rate ; no mention is made of the difficulties presented by the dynamics of rigid body rotation. But the most important applications to electro-dynamics are fully and clearly presented : we only trust that Dr. Campbell's evident contempt for the yet unconverted will not offend intending converts.

The book is full of matter of extraordinary interest, the treatment is always vigorous, and such small faults as we have found are quite insufficient to warrant us treating it as anything but a very successful attempt to deal with the difficult task of giving an account of electrical theory as it stood at the beginning of this year. The specialist may find small omissions in his particular branch, but he will not find any very serious fault ; in general he will find the book stimulating, informative, and an excellent preliminary when he wishes to read up any other branch. To the student and scientist engaged in other departments of science who have not time for much reference to original papers, the book will be invaluable.

E. N. DA C. A.

Mathematical Physics. Vol. I. Electricity and Magnetism. By C. W. C. BAR-LOW. [Pp. vi + 312.] (University Tutorial Press.)

As the book does not, as far as we can see, pretend to be more than a cram-book for examinations, it is not necessary to point out that it is not always particularly clear on the fundamental conceptions which underlie the mathematical theory of electricity. It has many examples, with answers, and will, we think, answer its purpose.

E. N. DA C. A.

BOOKS RECEIVED

(Publishers are requested to notify prices)

The Petrology of the Sedimentary Rocks. A Description of the Sediments and their Metamorphic Derivatives. By F. H. Hatch, Ph.D., Mem. Inst. Civil Engineers, Vice-President of the Inst. of Mining and Metallurgy, and Past President of the Geol. Soc. of South Africa, and R. H. Rastall, M.A., Demonstrator of Geology in the University of Cambridge. With an Appendix on the Systematic Examination of Loose Detrital Sediments by T. Crook, A.R.Sc. (Dublin). London: George Allen & Co., Ltd., 44 and 45, Rathbone Place, 1913. (Pp. xii, 425.) Crown 8vo. 7s. 6d. net.

G. W. Bacon & Co.'s New Contour Globe. Fifteen inch diameter, with compass. Three heights of land and four depths of sea are shown in different colours. Total weight only 4½ lbs. Price 25s. net.—Also Bacon's Wall Maps. United States. 4 by 5 ft. Scale, 1 : 3,200,000. Drawn on a secant conical projection with errorless parallels, 34° and 44° North latitude. Price, on cloth, rollers and varnished, or on cloth, cut to fold, 16s.—Also a New Contour Map of England mounted to fold. Price 7s. 6d.—Also a New Contour Map of Wales in Welsh, edited by Prof. Timothy Lewis, M.A. Price 7s. 6d.—Also Excelsior Map of Mediterranean Lands. Also New Contour Map of the Near and Middle East (the Land of the Five Seas). Size, 40 by 30 inches. Price, to hang on the wall, cut to fold and eyeletted, or on rollers and varnished, with or without names, 7s. 6d. Bacon & Co., 127, Strand, London.

Panama, the Creation, Destruction, and Resurrection. By Philippe Bunau-Varilla. London : Constable & Co., Ltd., 1913. (Pp. xx, 565.) 12s. 6d. net.

Text-Book of Zoology. By H. G. Wells, B.Sc., F.Z.S., F.C.P., and A. M. Davies, D.Sc. Seventh impression (sixth edition). Revised by J. T. Cunningham, M.A., Oxon. London : W. B. Clive, University Tutorial Press, Ltd., High Street, New Oxford Street, W.C., 1913. (Pp. vii, 487.) 6s. 6d. net.

Beiträge zur Rassenkunde, Heft 12. Die "Natürlichen" Grundstämme der Menscheit, von Maurus Horst. Hildburghausen, 1913: Thüringische Verlags-Anstalt. (Pp. 35.) Price 75 Pfg.

The British Journal of Tuberculosis. Edited by T. N. Kelynack, M.D. London : Bailliere, Tindall & Co., 8, Henrietta Street, Covent Garden. Publishers in the United States : G. E. Stechert & Co., 151-155, West 25th Street, New York. (Pp. xxx, 216.) 1s. 6d. net.

Irritability. A Physiological Analysis of the General Effect of Stimuli in Living Substance. By Max Verworn, M.D . Ph.D., Professor at Bonn Physiological Institute. With Diagrams and Illustrations. New Haven : Yale University Press. London: Henry Frowde. Oxford : University Press, 1913. (Pp. xii, 264.) 15s. net.

Guide to Photo-Micrography. Primarily prepared for Users of Apparatus made by E. Leitz. (Pp. 38.)

The Microscope, and Some Hints on How to Use it. By E. Leitz. (Pp. 42.)

Organic Chemistry for Advanced Students. Vol. II. By Julius B. Cohen, Ph.D., B.Sc., F.R.S., Professor of Organic Chemistry in the University of Leeds, and Associate of Owens College, Manchester. London : Edward Arnold, 41 and 43, Maddox Street, Bond Street, W., 1913. (Pp. vii, 427.) 16s. net.

Evolution by Co-operation, a Study in Bio-Economics. By Hermann Reinheimer. Author of "Nutrition and Evolution" and "Survival and Reproduction." London : Kegan Paul, Trench, Trubner & Co., Ltd., Broadway House, 68-74, Carter Lane, E.C., 1913. (Pp. xiii, 199.)

A Systematic Course of Practical Science. For Secondary and other Schools. Book I. Introductory Physical Measurements. (Pp. vi, 126.) 1s. 6d. net. Book II. Experimental Heat. (Pp. vi, 162.) 2s. 6d. net. By Arthur W. Mason, B.Sc., B.A. (Lond.), Senior Science Master, Municipal High School, Tynemouth. Rivingtons, 14, King Street, Covent Garden, London, 1912.

Life, Light, and Cleanliness. A Health Primer for Schools. Published under the Direction of the Director of Public Instruction, Punjab. Lahore : Rai Sahib M. Gulab Singh & Sons, 1912. (Pp. 128.) Price 8 annas.

Australian Institute of Tropical Medicine. Report for the year 1911. By Anton Breinl, M.D., Director of the Institute, in Collaboration with Frank H. Taylor, F.E.S., and T. Harvey Johnston, M.A., D.Sc., F.L.S., Lecturer in Biology, University, Brisbane. Printed by W. A. Pepperday & Co., 119a, Pitt Street, Sydney. Published by Angus & Robertson, Ltd., publishers to the University of Sydney'; the Oxford University Press, Amen Corner, London, E.C., and 29 West 32nd Street, New York. (Pp. iii, 96.) With eleven plates.

LITERARY NOTE.

Messrs. Constable will publish almost immediately the "Life and Letters of Alexander Agassiz," edited by his son.

NOTES

IT will be of interest to examine how the literary and scientific Nobel Prizes have been distributed among the nations since the inauguration of the prizes in 1901. The prizes were rendered possible by the will of Alfred Nobel, who left a vast sum of money, the interest of which provides the necessary funds. The Peace Prize is given in Stockholm, and we do not consider it here because it refers to a species of human effort which is outside our immediate province. The literary and scientific prizes are allotted and distributed by Sweden. Workers are not allowed to ask for prizes; but every year the Nobel Committee issues an invitation to leading men asking for nominations. These are then collected and carefully considered during a whole year by the committees, on the report of assessors who, we understand, make the most exhaustive study of the literature connected with the nominations. Four prizes are given every year by Sweden, each one consisting of a medal, an illuminated album, and a cheque for between seven and eight thousand pounds. Sometimes, however, one prize is divided between two recipients. The presentation is usually made by His Majesty the King of Sweden himself (on December 10) in a very distinguished ceremony; and the recipients are required to give lectures on their work, which are published annually by the Nobel Committee. The four prizes distributed by Sweden are for Literature, Physics, Chemistry, and Medicine. It is obvious that the exceptional and international nature of the prizes attaches very great honour to them; while the pecuniary addition constitutes the first attempt ever made by mankind to give some suitable recompense to their benefactors in great branches of work which often receive no other reward. On the whole, therefore, the title of Nobel Laureate, which is assumed by the recipients, is perhaps the greatest of honours. The conditions of the awards are such that there can be no possibility of the interplay

of personal influence or of prize-hunting; and probably as much impartiality and care is bestowed upon the allotments as is possible in this world.

During the twelve years from 1901–12 inclusive, fifty-six prizes have been allotted to citizens of fourteen different countries. So far as we can ascertain the nationalities are correctly placed in the following table. In this we have entered the numbers of recipients of each country which have received each class of prize; and have compared the total prizes received by each country with the population of that country—the comparison being expressed in a common rate per 100,000,000 of people. The populations are taken from the Census figures in 1910 or 1911, given in the Britannica Year Book for 1913— except in those countries where there has been no census, and where the population is "estimated." The countries are arranged in the order of their success in obtaining prizes.

COMPARATIVE TABLE OF THE SCIENTIFIC AND LITERARY NOBEL PRIZES AWARDED DURING TWELVE YEARS, 1901 TO 1912

Country.	Popula-tion in millions.	Prizes awarded for					Rate per 100 millions of popula-tion.
		Physics.	Chemistry.	Medicine.	Literature.	Total.	
1. Sweden	5·6	1	1	1	1	4	71·9
2. Holland	5·9	3	0	0	0	3	50·5
3. Norway	2·4	0	0	0	1	1	41·8
4. Denmark	2·7	0	0	1	0	1	36·4
5. France	39·6	4	4	3	2	13	32·8
6. Germany	64·9	4	6	4	4	18	27·7
7. Switzerland	3·7	0	0	1	0	1	26·7
8. Belgium	7·4	0	0	0	1	1	13·5
9. Britain	45·4	2	2	1	1	6	13·3
10. Spain	19·6	0	0	1	1	2	10·2
11. Italy	34·7	1	0	1	1	3	8·6
12. Poland (Russian)	12·5	0	0	0	1	1	8·0
13. United States	92·0	1	0	0	0	1	1·1
14. Russia	120·6	0	0	1	0	1	0·8

It is obvious from statistical considerations that the Rate Column cannot be considered very exact for the smaller countries, especially when they have received only one prize; and there may be some subconscious desire to give a prize to nations, especially the smaller ones, which have not yet received one. There has also been some outcry in Sweden upon this subject. In these cases, a single prize will obviously affect very greatly

the position of one of these nations on the list; but for the larger nations the numbers are more decisive. It will be observed that Holland, France, and Germany have been by far the most successful among these; that Belgium, Britain, Spain, and Italy come in a second class; and that the United States and Russia are in the third class.

Neither Britain nor the United States can be congratulated on the result. The table probably gives a good rough measure of intellectual development in the respective nations, and one which would be likely to be confirmed in other lines such as mathematics, zoology, and botany, art, music, and even invention during the present century. The failure of Britain and the United States is probably due to their attitude towards intellectual effort, to their preoccupation with politics and game-playing, and possibly to the unreality of their education. It is probably due, however, still more to the poor payment made for scientific work in comparison with other lines of effort or of no-effort. How little interest is taken in this country in the higher intellectual work may be gauged from the very small references to the Nobel Prizes which appear in the British press, compared with the endless talk about such matters as the so-called Olympic Games. But the country of Shakespeare and Newton can scarcely be second to any in fertility of genius-production, and there are probably secondary factors at work to-day which are suppressing that invaluable asset.

The University of Bristol

In the July Number we inserted a brief note on the affairs of the University of Bristol, mentioning some of the criticisms which had previously been published upon the management of this institution. Since then we have been asked to make a thorough examination of the questions at issue. We have consequently studied all the documents on the subject which have already been published, including papers on both sides of the controversy.

We have no bias at all in the matter; and it is one which concerns science only in regard to the general influence of university management upon scientific work and teaching. To us, as to all, it is unpleasant to have to criticise any public institution; but it must be confessed that the study of the

documents which we have made is very convincing as to the soundness of the allegations against the conduct of this University.

On the other hand, the explanations which have been put forward do not appear to be at all satisfactory; and we are strongly of opinion that the matter is one which certainly calls for public inquiry, either by the authority constitutionally appointed for that purpose, namely the Visitor, or by the Board of Education. The case has aroused and is arousing very serious criticism; it touches the whole question of academical life and prosperity in this country; and, if it is not one for intervention, we cannot understand how there can often be any case which will call for such. The careful scrutiny of the facts which we have made justify us in stating our opinion; and we add no more at present, only because we still hope that a public inquiry will be made.

Mr. Balfour at the National Physical Laboratory

On June 26 the Right Hon. A. J. Balfour, M.P., opened the new buildings of the National Physical Laboratory, Sir Archibald Geikie, P.R.S., being in the chair. The scheme for additional laboratories and offices, planned in 1909, was estimated to cost more than £35,000, towards which the Treasury has promised £15,000 provided that there is no further application to the Government. Dr. Glazebrook remarked that the buildings had been erected in no small degree by faith—faith in the importance of the work and faith in the liberality of friends. Lord Rayleigh emphasised the fact that funds were still needed for the equipment of the laboratory, and wished that pure science might have figured a little more there. He trusted that in future funds would be devoted to pure science as well as to the immediate advantage of industry. Mr. Balfour fully admitted the great importance of science to-day. "Everybody, I think," he said *inter alia*, "would be ready to admit that one of the great conditions of human progress is our growing command over nature; that this growing command over nature is the sphere of our activities in which it is most plainly and obviously certain that immense advance has been made in the last one hundred and fifty years—an advance which, instead of diminishing in its rate of progress, seems to me to be increasing. You may argue as to whether we have improved in this or in that respect; you may debate whether great social or political

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influences are or are not for the general advantage of society; but the one thing you cannot argue about is the command which science has given us—which science is teaching to those who are engaged in the technical work of industry. Nobody can dispute that that, at all events, has covered an immense range of progress, and that we are still moving rapidly in the right direction. . . . Lord Rayleigh incidentally dropped a criticism— I hardly like to call it a criticism—to express faint regret that in the history of this institution a larger fraction of the labour had been devoted to matter immediately connected with industry than to the abstract or purely scientific investigations, on the successes of which ultimately, and as years go on, the future of industry depends. Now I think all of us must share that regret. I have not sufficient acquaintance with the work of the institution to know how much of the time and labour of the staff have been devoted to pure research, but believing as I do—it is, indeed, one of my foremost articles of social faith—that it is to the labours of the man of science, working for purely scientific ends and without any thought of the application of his discoveries to the practical needs of mankind, that mankind will be most indebted as time goes on; holding, as I say, that faith, I should desire that as much advance should be made in pure science in these buildings as money and space allow."

The Seventeenth International Congress of Medicine (PHILIP HAMILL, M.A., M.D., D.Sc., M.R.C.P.)

At the seventeenth International Congress held in London this year remarkable progress in the knowledge and treatment of disease was recorded. The communications dealing with the notable advances which have recently been made in the more purely scientific domain of medicine are of especial interest and significance in their bearing upon the future of practical medicine. It may be useful, therefore, briefly to review some of the more important discoveries which were considered and discussed at the Congress.

Chemiotherapy.—The address delivered by Prof. Ehrlich summarised in masterly fashion the advances which have been made in this subject. Specific chemiotherapy is a recent development of medicine, and rests upon a foundation of extensive researches on parasitology.

It has been found that if an animal be infected by a parasite the injection into the circulation of certain substances which can be prepared synthetically will bring about the death of the parasite whilst leaving the host unharmed—*i.e.* the drug is "parasitotropic" rather than "organotropic." But the mode of action of such a drug is more complicated than can be accounted for on the assumption that it acts merely as a differential poison. If a particular parasite be exposed to the action of the drug *in vitro*, it may escape death; and if it be a motile organism, such as

a spirochæte, its activity may remain undiminished. If, however, the parasite, after treatment with the drug, be injected into a living animal, it is immediately killed by the blood of the host. The same result is obtained if the drug be injected into the circulation of an infected animal. To this method of treatment Ehrlich has applied the term *Therapia sterilans magna*, and by such means it is possible to sterilise the host as far as a particular parasite in question is concerned. The problem of chemiotherapy therefore resolves itself into the discovery of a substance which can be administered in a dose large enough to secure death of the parasite as a result of the combined action of the drug and the tissues of the host, without producing toxic effects upon the host. Amongst the substances which appear to be particularly effective in this respect are certain organic compounds of arsenic, notably those in which the arsenic is linked to a benzene nucleus bearing an amino group. Up till quite recently, atoxyl

$$NH_2 \langle \rangle AsO \begin{matrix} \nearrow OH \\ \searrow ONa \end{matrix}$$
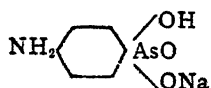
was much used, and was of considerable service ; but unfortunately it is somewhat too markedly "organotropic," and several cases of optic atrophy resulting in total blindness have been recorded as a result of its use. After extensive researches, in which 605 synthetic organic compounds containing arsenic were tested, excellent results were obtained with the 606th compound, dihydroxy-diamino-arsenobenzene,

$$\begin{matrix} & As & As & \\ NH_2 & & & NH_2 \\ & OH & OH & \end{matrix}$$
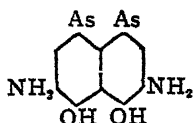
now universally known as "Salvarsan" or "606." More recently a derivative of salvarsan, neo-salvarsan, has come into use, and although it is rather more unstable than salvarsan, it can be administered with greater ease.

Several interesting phenomena have been observed during researches on this subject ; from the practical standpoint one of the most important is the acquisition by the parasite of tolerance to the drug. If small doses, insufficient to sterilise the host, are given, the parasites may become increasingly difficult to destroy by subsequent injections ; hence it is important, from a therapeutical point of view, to give the largest doses which can be tolerated in order to ensure immediate sterilisation. For this reason it is clearly desirable to use a drug having as low an "organotropic" tendency as possible. Such an acquisition of tolerance is shown by many parasites. The tolerance so acquired is specific for the drug employed.

The great practical value of the new therapy has been most clearly demonstrated in connection with syphilis and certain tropical diseases such as yaws (frambœsia), caused by a spirochæte allied to that of syphilis. The success which has attended the new treatment of these diseases is remarkable. In the case of soldiers treated for syphilis at the military hospital in Rochester Row, the recovery has been such as to result in the annual saving to the army of a number of days of sickness which is equivalent to the services of a battalion for nearly three months. Even more remarkable results have been obtained in the case of yaws, which can be cured with a single dose of salvarsan. As a result of this treatmen

ı hospital which contained on an average 300 patients suffering from this disease was no longer required.

Up to the present the most brilliant successes have resulted from the treatment of diseases due to animal parasites; but evidence is not wanting that similar successes will soon be forthcoming in the case of diseases of bacterial origin. In this connection organic compounds of copper and other metals are being investigated, and there is ground for hope that valuable remedies for tuberculous infections may before long be found.

Dietetics.—Recently the significance of hitherto unsuspected constituents of food has come to be recognised, and it is now realised that dietary factors which until lately have not received consideration are of cardinal importance in the maintenance of normal metabolism. It is now clear that in addition to what are known as the proximate principles—proteins, carbohydrates, fats, and salts—there are in a mixed diet minute amounts of certain substances which seem to be essential for the normal nutrition of the body. If, for any reason, these substances are absent or deficient, various disorders of metabolism, resulting in the production of characteristic symptoms, make their appearance. Beri-beri appears to be a disorder of this nature. It has been found associated with a diet of rice from which the pericarp has been removed by milling (polished rice). In the rice grain the essential substances above mentioned, for which the name "trophones" has been suggested, are located mainly in the pericarp. Beri-beri can be prevented by using rice from which the pericarp has not been removed, or by including in the diet foods which are rich in trophones. Polyneuritis, simulating many of the symptoms of beri-beri, has been produced in animals as a result of feeding them on a diet poor in trophones. Young animals fed on diets consisting of purified proteins, fats, and carbohydrates, even with the addition of salts and phosphatides, soon cease growing; but the addition of minute amounts of fresh foods or tissue extracts is sufficient to ensure normal growth.

The nature of these essential substances (trophones) is not yet precisely known. There appear to be several substances concerned, of which the vitamine of Kunk is probably one. They do not seem to exist free, but are probably portions of more complicated molecules. Many of them are cyclic compounds, purin and pyrimidin bases, which the animal seems incapable of synthesising, and which, as sources of energy, are negligible. The trophones are unstable bodies, and are injuriously affected by prolonged storage, by cooling, and by a variety of other agencies.

The nature of the salts in the food also appears to be of importance. There is evidence to show that the ash of mixed foods is much more valuable than an artificial mixture of salts corresponding in every chemical detail with the ash. Possibly a minute trace of fluorine and manganese may be essential to proper nutrition.

Cardiac Pathology and Therapeutics.—In almost every branch of medical science the application of exact methods of observation has been followed by the discovery of important results. This is strikingly exemplified by the advances which have been made in recent years in the physiology, pathology, and therapeutics of the cardio-vascular system. In this field English workers have been prominent. When the Congress was last held in London in 1881, much mystery surrounded the mechanism of the heart's rhythm. Shortly before that time Gaskell had begun his classical researches on the heart of the tortoise, and had promulgated his theory of muscular conduction from chamber to chamber without the intervention of nervous mechanism. This was followed by

the discovery by Kent and by His of the specialised conducting bundle generally associated with the name of the latter observer. In recent years the "pacemaker" of the heart, or the point of origin of the cardiac rhythm, has been definitely localised as a result of the work of Keith, Flack, and Lewis. The pioneer work of Mackenzie on disorders of cardiac rhythm has been greatly extended by the use of Einthoven's storing galvanometer, which, in the hands of Lewis and other workers, has yielded results of great scientific interest and clinical value. The exact nature of disorders of cardiac rhythm has been determined, and complete irregularity of the pulse has been shown to be due to fibrillary contraction of the auricles. Clinically, these results are of great importance, inasmuch as they help to differentiate serious from trivial conditions, previously often confused, and furnish a rational basis for the administration of cardiac remedies.

In contrast to the advances made in the study of the disorders of rhythm and conduction is the unsatisfactory state of present knowledge in regard to the functional competence of the heart. Prognosis in cardiac failure is a matter of extreme difficulty, for as yet there is no method of ascertaining the reserve power of the heart muscle. If a method could be devised which could be applied clinically, it would be possible to substitute facts for conjecture, and thus enable prognosis to be placed upon a more reliable basis.

Radiology.—Radium is now being widely used in the treatment of malignant growths. It is, however, not yet possible definitely to appraise its value in this respect; it cannot yet be said to what extent radium treatment is likely to supplant operative interference, although it is generally accepted that radium treatment is useful as an aid to eradicating traces of growth left behind after operation. There appears, however, to be general agreement on the following points : (1) Unfiltered rays have a high power of tissue destruction ; (2) certain rays, notably the β-rays, have the power of stimulating growth, and they may therefore act harmfully by inducing increased multiplication of cancer cells ; (3) the γ-rays are the most useful therapeutically, since young actively growing cells are most susceptible to their influence ; (4) malignant growths of mesoblastic origin (sarcomata) are more amenable to treatment than carcinomata ; (5) filters of aluminium or lead or even air are useful in removing the undesirable radiations.

At present, although radium is of value in treating superficial growths, its penetrating power is limited, so that for deep-seated growths, such as cancer of the breast, it is unjustifiable to rely on this treatment to the exclusion of operative measures. There is evidence to show that the application of X-rays and radium to the field of operation tends to lower the liability to recurrence. All cancerous growths are not equally amenable to treatment ; those of the mouth and tongue are less favourably affected than those of the breast, whilst carcinoma of the uterus affords a hopeful field for radiotherapy.

Successes which have attended other applications of science to the practice of medicine, such, for instance, as inoculation against typhoid fever, were reported to the Congress, but space does not permit of their mention here. It should be noted that the advances in scientific medicine which have been communicated to the Congress could not have been made without experiments on animals. This was fully recognised by the Congress, which was unanimously of opinion that no restrictions which might in any way impede the progress of medicine should be placed upon such experiments.

The future is full of hope ; increasing interest is being taken by the people in

the advances of medicine ; all the great departments of State directly concerned with the well-being of the community are realising the importance of scientific inquiry, and it may confidently be predicted that when the Congress next meets in London many of the diseases which have vexed the present assembly will have lost their terrors for humanity.

THE GENIUS OF SCIENCE

WHEN we examine the little animals in a droplet of water, the first thing which strikes us is their movement. But is the movement merely a chance transference hither and thither into new places where perhaps the food elements have not already been exhausted; or is it a purposeful search? The latter implies, even in these minute creatures, the first element of mind— the mind of amœbæ and of infusoria. The aimless transference requires no consciousness of direction; but the first property of mind should be that its possessor can remember directions which prove on trial to be rich or deficient in nutritive elements; can store its past impressions; and can select the directions which give good results. Even among these little bodies there is often some evidence of purposeful movement—the creature stops, turns or accelerates its speed, or, when it is interrupted by some great mass of vegetable fibre or other detritus, still attempts to persist in its former course. It seems to be consciously searching its food—to be rejecting profitless directions and following profitable ones. Higher in the animal scale, we find the ants and bees travelling abroad in the most obviously conscious seekings for food; and indeed, animals in general seem to exist upon the principle given by the proverbial injunction " If at first you don't succeed, try, try, try again."

On watching their movements, however, we are impressed by the fact that the intelligence seems to be very elementary. Thus, as the American humorist remarks, the busy ant, instead of having the wisdom to walk round a stalk of grass, will take the trouble of ascending it to its top and running down the other side. Similarly the wasp, led by the scent of sugar, will enter a window, but does not possess the good sense to find its way out again at the same opening. The neglected subject of comparative psychology gives us many other instances. To attach a dog to a post, it is sufficient to tie the rope with any knot—and the animal never has the wit to undo the knot with his teeth. So also with horses, and even with monkeys and

baboons; but it is another story with the anthropoid apes, which will quickly loosen the knot. All the faculties of the lower animals are devoted merely to the search for immediate necessities, and they are satisfied when the object is attained. It does not occur to them that they might facilitate the search by a previous investigation of phenomena. Similarly when we rise in the scale of mankind, we find that most of them are merely searching for their food; they try here and there; they remember the directions in which they do or do not succeed, and are thus able to follow the most promising course—the agriculturist in a search for the best crops, the shopkeeper in the choice of goods for sale, the financier in his selection of securities, the politician in search of policies, and even the mathematician in the solution of numerical equations. That is, they seek by the method of trial and failure for a solution of the immediate problem before them—which is generally concerned with their livelihood. It does not occur to them to investigate the phenomena under consideration, to generalise, and to make one solution suffice for many. When we rise to this point we become men of science and inventors. In the great dumb ages which elapsed before men became conscious of science many of them must have observed the different shapes of stones, and have even selected certain shapes for their houses. Then some genius thought of investigating shapes in general, and the science of geometry was created, and the Pyramids and Parthenon rose from the ground. Every one was acquainted with fire, but it was not until we commenced to investigate burning in general that chemistry was born; and men were almost helpless before infectious disease until a few students began to investigate its cause.

The dog does not untie the knot because it never occurs to him to attempt to do so, though he would be easily able to do so with his teeth if it had occurred to him. All those who observed the different shapes of stones did not found geometry, not perhaps because they would have been unable to do so, but because the conception of generalisation and the wish for it never entered their mind. How many millions of dogs or of men may have performed these feats if only they had thought of them? The new idea is always the rarest idea. How many millions or billions of men and how many thousands of sages must have watched the heavenly bodies rising and setting and evidently

circulating round the world, without ever having thought for a moment that this evident movement was not real, and was only an apparent movement due to the rotation of the seemingly steadfast mass upon which they were standing; and what an extraordinary flash of genius it was which gave Copernicus that new idea. After him, none of the great astronomers until Newton ever dreamed that these heavenly bodies are chained to each other by the same law as that which attaches loose stones to the surface of the earth. Why did not the innumerable arithmeticians of old days conceive the possibility of generalising the arithmetical laws and creating algebra; and why again was it left to the supreme genius of Newton to analyse movement by its fluxion and, on the converse, to sum fluxion into movement? Why is it that so few of us think of these things? Indeed, the masses of men tend to ridicule the very flashes of genius which are of such supreme benefit to them—as witness the case of Columbus and of many others. But their obtuseness punishes themselves.

In science therefore the first requirement is that flash of intelligence, imagination, or inspiration—call it what you will—which awakens the idea; but this of itself is not sufficient. The person to whom the idea has occurred must have the sagacity to become convinced of its usefulness; and this requires a mind which can attain to a high purview of things in general. The mass of men would attach no importance whatever to any of the ideas just mentioned, even if they had thought of them. They are not interested in generalisations, which give them neither bread, nor fortune, nor such fame as they may desire; their efforts are directed to the benefit only of their self or perhaps their family or their country. Even if they possess very great natural ability, they concentrate it upon such objects, and become prosperous citizens, millionaires, generals, and politicians—men of merit perhaps, but who bestow small benefits, or even disasters, upon mankind in general. This leads us to ask, what is greatness? It is in the first place knowledge of what is really great. The able man can do things; but the great man can first select what is best to be done. The first may be great in small things, but the second is great in great things. The youth in search of the work for his lifetime will select it according to the degree of his mental ability. If this is very low he will seek only pleasure; if it is

higher, he will seek for wealth or fame or both, and chiefly for himself; if it is still higher, he will work for his country; if it is very high, he will seek to confer great benefits on mankind in general, regardless of himself. We often hear it discussed as to who were the greatest men. So far as simple personal ability is concerned, it would be difficult to choose between a Newton, a Shakespeare, and a Bonaparte. But the last worked really only for himself, with some secondary thoughts for his adopted country. When good fortune took him by the hand, he asked her only for enormous fame; he saw himself become a thunderbolt among men and the wonder of all; but since he died, what has been left of him and his work except a story and a name which are scarcely greater than the stories and names created out of Shakespeare's brain— to-day he is nothing more to us than Hamlet and Othello. But we can imagine that Shakespeare said to himself " I will hold the mirror up to men and teach them their own nature." He therefore gave us a boon incomparably greater than that given by Napoleon. In this supreme line of effort the great poet and the great man of science are one; for indeed the two muses are twin sisters. Newton did not demonstrate men to man, but he demonstrated to him the heavens and the science of numbers. Scarcely less are the travellers and soldiers who confer civilisation upon barbaric tracts ; and the inventors who confer innumerable utilities upon the whole race. In all of such, not only must there have been the flash of the original idea; but also the appreciation of its value to the world in general. Where, compared with these, are the numerous men of talent who are great only for themselves ?

But even these two supreme qualities are not alone sufficient, and the scientific man must possess the determination and the vigour to overcome many difficulties before the original idea can be materialised. That which when discovered becomes an easy commonplace is when undiscovered an almost unattainable summit. He sees that summit only at moments through the drifting clouds of doubt; he commences the ascent weighted by endless troubles and perplexities, and new difficulties confront him at each footstep. How often does he fail and turn back to the pleasant vales of ordinary life! It is a commonplace to think that Shakespeare dashed off his dramas without thought; but each one shows by the evidence

of its structure that it was created only by ceaseless labour. What must have been the toil necessary to found geometry, algebra, the calculus, the atomic theory, the theories of gravitation, electricity, and evolution?—not less than the toils which gave the New World to the Old World and the map of Africa to Europe.

But in addition to all these qualities which the man of science must possess—the genius to conceive, the sagacity to perceive, the determination to succeed, and the strength to work—he must also be fortunate enough to find an opportunity. There may have been, and probably were, many potential Newtons and Shakespeares, as well as Napoleons in the old, old history of mankind; but the opportunities, that is the powers given by previous workers, were not there. But to say this is not to depreciate the value of the personal qualities required. We often hear it said scornfully of some discoverer that if he had not lived some one else would have taken his place; but this is generally true only of small workers. There have been revolutions without Napoleons, and many opportunities without discoveries. Here again the personal qualities enable the man to seize the opportunity. In fact opportunities are common but genius is rare; and to a great extent genius makes its own opportunities.

The conjunction of circumstances leading to the production of scientific genius must therefore be very rare. It is rare, and its rarity explains the slowness of human advance. There is much evidence to show that nations produce genius of all kinds only at certain epochs—that a nation may exist for ages without new science, new art, or indeed advance in any particular. Suddenly, however, there comes a blossoming-forth. Indeed a biological law may be suspected here—that genius is like the flowers on the tree, and that the mass of mankind are but the leaves. The latter serve the ordinary purposes of the plant; the former serve the extraordinary purpose of a greater growth and a more glorious future. The first asset which a nation possesses is its capacity for producing genius— greater than the possession of a fertile soil, or of mineral wealth, or of opportunity for commerce; as great as the assets of industry and honesty in its people. The history of nations is mostly the history of their men of genius great and small; and there are nations which, possessing no men of genius, have taken no part in the history of the world for ages.

Science, however, needs not only men of supreme genius, but men of another class who are scarcely less meritorious though fortunately much more common—the class of men who are engaged upon the record and classification of observations, without attempting wide generalisations. And this branch of science requires qualities, not so rare and brilliant perhaps, but also great—the desire to do important work, the determination to attempt it, and the patience to accomplish it—and that, generally without hope of any adequate recompense. Such work often leads by chance to very important discoveries, and has now become an actual necessity for advance. We may distinguish the two classes of mind. The first is essentially the solver of problems; the second the observer of facts. To some extent every man of science must be composed of both; but in a few the former essence predominates, and in most, the latter one. Science may be almost said to require nine parts of thought to one of observation—but there must always be something of both in it.

Lombroso attempted to prove by statistics the kinship of genius and madness; but it is more probable that the latter grows from the former and not the former from the latter. Genius is the most terrible of all tyrants; it exacts endless service and it spares not either its victim, nor his fortune, nor even his children. It is in that way that the madness lies. The fire which impels also consumes. Now it burns low with despondency, now it frets at each obstacle, now it overwhelms with success; and it must be fed eternally with all the man's possessions. Even his cup of triumph is mingled with myrrh—the scepticism of friends, the puerilities of critics, the spite of ools, the jealousy of rivals, the intrigues of the schemers who profit by every new discovery at the expense of the discoverer, and the large indifference of the dull public. Is not all this written in the book of the history of science—the poison for Socrates, the flame for Bruno, the prison for Galileo and Columbus, opposition for Jenner, and poverty, obloquy, or neglect for scores of the world's greatest benefactors? Nor has it ceased to-day. The noblest of histories and religions is based upon this theme. The greatest man of science, who obtained from his study of human morality a divine medicine for many of the world's evils, suffered for his work in a manner which we hear of in every church to-day; yet those who hear it

go about to do precisely the thing which was done to him—to punish their benefactors. But then, they say that these benefactors are mad; or that their work was really done by others, or that it was useless, or injurious, or contrary to religion, or even to science! And cases of this kind have occurred recently and will continue to occur. The kink is really in the mind, not of the man of genius, but of the public.

Of course these are also some of the troubles of all good workers, not only of those of genius; but men of science are perhaps the greatest sufferers, because science brings no material reward to them. Science is not protected by copyright or patent; and their labours are therefore not counterbalanced by any hope of payment except the consciousness of their own good works. They are exploited by all and paid by no one; and few are found to face the prospect.

Hitherto the world has done nothing for the most wonderful of its products, the higher genius. It has regarded only the leaves of the tree of life—not the flowers and the fruit; and, with a strange obtuseness, has indeed often cut the flowers or pulled the fruit before it was ripe. It has left all to nature, and nature has often responded according to her wont—by barrenness. Where this has occurred—where the higher genius has died out—the whole intellectual life of the people has tended to fall to the lower and sordid level at which it stands among some nations to-day; and it is the duty and interest of mankind to work for the prevention of this calamity in the future.

SIR OLIVER LODGE'S ADDRESS[1]

I.—THE LOGIC OF SCIENCE

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THE Presidential Address at the British Association is the great manifesto which annually announces *urbi et orbi* what advances in scientific knowledge seem to its distinguished author to be worthy of the attention of the English-speaking world, and usually excites keen interest and debate. It is therefore highly flattering to a philosopher who is not callous to the progress of knowledge to be invited to take part in this debate and to have an opportunity of expressing his characteristic comment before a scientific audience. But to avoid misunderstandings, he should make clear at the outset how very restricted is the philosopher's competence in such a case. His primary attitude ought to be that of a learner who welcomes gratefully the improvements in human knowledge which the sciences have achieved. It is only secondarily that he should claim the right to comment critically on those aspects of scientific controversy which are ultimately logical, and, skirmishing ahead as an unauthorised raider, to "speculate" about those subjects which cannot yet be cultivated by the approved methods of scientific experiment

In the latter case his ingenuity may enable him to guess at analogies that may hereafter lead to a successful cultivation of the field ; in the former, he may sometimes protect the scientist against the deceptive glamour of words and help him to have the courage of his convictions and his methods, in spite of the arrogant pretensions and misleading suggestions of philosophic " logic." For the philosopher should never forget that the scientist is doing the actual work of human knowing, of which logic professes to expound the theory. But unfortunately science and logic at present conduct their operations almost completely out of each other's sight, and only so avoid a conflict which, if they met, would be fatal to one or the other. Modern

[1] Recently republished with notes (J. M. Dent & Sons).

science flourishes because it rests on a salutary ignorance of logic and a healthy contempt for the traditional philosophies; modern logic survives, together with the ancient philosophies it springs from, because it has entrenched itself in a culpable ignorance of science. It is in consequence of this specialism that philosophers have been so slow to recognise the logical value of the actual procedure of the sciences, while scientists have hardly troubled as yet to appreciate the scientific importance of the radical conversion of philosophers to empiricism and Darwinism which goes under the name of Pragmatism.

But to a pragmatist philosopher the scientific situation of the present day is full of interest and stimulus, and beautifully confirms his generalisations about the real nature of scientific method. Dogmas are no longer received on mere authority, and are everywhere quoted at a discount. Experiment has everywhere established its right to test assertions and to question prejudices. Principles are no longer conceived as self-evident and self-proving "intuitions" or immutable "necessities of thought," but are everywhere treated as convenient postulates or methodological assumptions, whose effective truth depends on confirmation by experience rather than on a man's psychological willingness to accept them at a first hearing, so that their real proof comes from their scientific services and their success in handling the "facts" of the sciences that were boldly built upon them. Hence the man of science has won great freedom for himself in his attitude towards his "principles." It has everywhere become permissible to discuss principles, to consider what formulations of what principles are most useful, and to suggest alternatives and improvements on those in use. As a matter of fact the principles of most sciences have been greatly modified, with the happiest effects. Those of biology have been revolutionised by evolutionism, those of geometry by meta-geometry, those of physics by radioactivity, those of mechanics and chemistry by the electric theory of matter, etc., and even such fundamental assumptions as the conservation of energy and the indestructibility of matter have to submit to the indignity of experimental verification.

Nowhere can one see a set of principles, even in the sciences which have not experienced such convulsions, that do not seem to be essentially open to discussion and that merely force themselves upon the mind through our sheer inability to think of

alternatives to them which are more convenient and more fertile scientifically. In arithmetic alone old-fashioned philosophers still fancy that they are confronted by this brute and uninstructive sort of "*a priori* necessity of thought"; but only because arithmetic is the oldest, and has become the least progressive, of the sciences, and no one has taken the trouble to devise a calculus which would systematically vary the initial postulates of common arithmetic.

Now what is the meaning of all this unsettling of traditions and upsetting of scientific "foundations"? According to philosophic "logic" it reveals how incurable are the defects of scientific method, how uncertain are all the principles of the sciences, how incapable they are of conducting to real proof and stable conclusions. It is held that "demonstration" is the *sine qua non* of reasoning, and that demonstration is impossible unless the principles on which it rests are certain. Now inferences from hypothetical assumptions are infected with the defects of the premisses from which they are deduced. Empirical verification also is useless, because it can never lead to a "valid" conclusion. It must always commit the "fallacy" of "affirming the consequent," because it tries to argue from the success of the consequences to the truth of the initial premisses. Once this paralysing criticism is grasped, the greater the activity of thought the greater the danger seems. The freedom to think and the licence to speculate can conduce only to anarchy and augment the chances of going wrong. The situation therefore ought to mean chaos in the scientific world, and the discrediting of science.

But this is not the way either the scientists or the public take it. We all imagine ourselves to be living in an era of unexampled scientific progress, of enormous scientific activity, of infinite scientific ingenuity and resource. Moreover, the differences of scientific opinion, the struggle for existence of ideas, appears to do no harm; the keener it is, the more rapidly and certainly the sciences progress.

Evidently, therefore, something has gone wrong with the traditional valuation of scientific method. The facts do not bear out the belief that science flourishes best when it conceives itself to be under obligation to start from certainties and to play for safety, to anchor itself to unquestionable dogmas, or when it dreads freedom of thought and of debate and resents doubt and

criticism. On the contrary it seems to grow all the faster for cutting itself loose from what has always been believed, plunging into an agitated sea of wild hypotheses and hazardous experiments, and hailing as "true" whatever belief most successfully emerges from the rough and tumble of the conflict of opinions.

What then is the solution of the paradox that the prosperity of science seems to depend on its ignoring all the rules laid down for its guidance in the traditional logic? Simply this, that the traditional logic is wrong in all its regulations, that scientific practice is right, and that logical theory should be based on scientific practice. The pragmatist is the philosopher who has grasped all this and has therefore discarded the meaningless ideals of an impracticable "logic." He has recognised instead that certainty is not the "presupposition" of scientific inquiry but its (distant) aim, and that no matter how much confirmation a scientific theory acquires, it can never become *absolutely* certain. He willingly admits the "formal fallacy" involved in "verification," but does not draw the formal logician's inference therefrom. Instead of inferring that therefore empirical evidence can never be conclusive, that experience can never "prove" anything, he infers that since science nevertheless accumulates such stores of valuable truth, it must be possible to dispense with evidence coming up to the logician's specifications and with the logical ideal of "proof." An ever-growing *probability*, sufficient for the purposes of the science, must be what "certainty" really means in the concrete, and the existence of alternative explanations and rival probabilities must be recognised in theory, as in fact.

Logic, in other words, must assimilate the great dictum of Sir J. J. Thomson that "a scientific theory is a policy and not a creed," and modify itself accordingly. If truth (like honesty) is the best policy, our keenness to attain it will be enhanced; but so will the (apparent) difficulties of ensuring that we are pursuing the *best* policy and picking it out from among the alternatives that present themselves. For we clearly run the risk that by adopting *one* policy we blind ourselves to the good that is in the others and to the facts that they could bring to light. To minimise this risk, it is evident that science must systematically cultivate open-mindedness and practise toleration. Alternative theories must always be borne in mind, even when the known facts are on the whole against them, and no working theory

should be utterly condemned. Nor can it be wrong to experiment with a variety of working theories, even though it is recognised that they are not, as they stand, compatible with each other. Only so shall we secure a willingness to try experiments in every direction and have our attention directed upon the facts that may lurk in every quarter. In short, for the narrow-minded intolerance of a logic that speaks only in terms of "necessity," "cogency," and "proof," and leaves us wrecked on the rocks of scepticism when it turns out that absolute truth and certainty are nowhere attainable by man, we must substitute a logic that will allow us to take risks and is familiar with the notions of freedom, toleration, and success, and knows how to justify its selections and preferences by their superiority in scientific *value*.

It may be thought that these general considerations are somewhat remote from the special topics of Sir Oliver Lodge's Address; but in fact they conduce directly to its proper appreciation and supply the principles which are properly applicable to the controversial issues which it raises. Not only do they render rational and intelligible that profusion of speculation of which Sir Oliver Lodge gives so lucid and fascinating a description, but they justify also such of his speculations as are still somewhat repugnant to the prejudices of those who have been brought up to believe that at every temporary halting-place of knowledge they had attained absolute and final truth.

I will not presume, however, to discuss what I take to be the primary subjects of scientific interest in Sir Oliver Lodge's Address. These appear to lie in the region of physics, and concern the scientific status of the ether and the atom. I will not venture to comment on Sir Oliver's championship of the reality of the ether, beyond remarking that he still seems to me to leave all the properties of the ether functional and the belief in it a methodological assumption, *i.e.* one of those pragmatic postulates which pave the way for the advance of science. But this is not of course to deny that our notion may not some day be found to be something more than a convenience of thought. The strange romance of the atom, which began as a bit of metaphysical dogmatism, which had a long career as a methodological assumption, and seemed just about to be reduced to a methodological fiction when it was shown to be a real fact in nature, should serve as a signal warning against the rash

presumption that what is assumed because it is convenient cannot be really true. But it should be remembered also that even as the atom was proved to exist only by being exploded and became good science only by becoming a logical contradiction and ceasing to be as indivisible as an "atom" is verbally bound to be, so the ether may be promoted out of the methodological status it bears at present only by being so transformed in the advance of physics that its best friends, like Sir Oliver Lodge, will hesitate to recognise it.

I will refrain also from contesting minor points, e.g. from cavilling at the variety of his definitions of Time, which declare in one passage that time is "essentially unchangeable," even for mathematicians, and in another that it is an "abstraction" of the element of "progressiveness," and so presumably of our own construction, together with its "uniformity," which is postulated, but assuredly could not be established experimentally. I will pass rather to those points of Sir Oliver Lodge's which are likely to be unpopular with scientists, and show that they contain nothing that is contrary to the true spirit and methods of science.

To discuss first the legitimacy of "Vitalism." We are here confronted with a dispute which has grown intricate because it was not observed that no conceptions which are capable of being scientifically tested are either scientific or unscientific per se. It is not scientific to believe in matter, any more than in spirit, as an unreasoning act of faith, nor unscientific to believe in devils, any more than in ether, as a definite hypothesis from which verifiable consequences are deducible. What is unscientific is to believe in devils without good and sufficient evidence, and to disbelieve in them merely because they are such an uncomfortable hypothesis. Even the conception of "law" may be conceived in a thoroughly anti-scientific way and used as a method of burking scientific inquiry. E.g. sociologists are prone, so soon as they have detected any uniformity in human affairs, to dub it a "law," and to think that this ends the matter, instead of investigating what combinations of forces, often very various, have produced the apparently uniform result, such as e.g. the fall of the birth-rate in all civilised societies. Or again, it is very common to hear the law of evolution talked about as if it were an adequate explanation and assured guarantee of the changes which we value as "progress." In

both cases the notion of law is used to procure a facile satisfaction and to bar the way to further inquiry.

Hence I would venture with all deference to suggest to the disputants here that the case is similar, and that both vitalism and mechanism are scientifically legitimate or the reverse, according to the spirit in which they are held. They are legitimate if, and in so far as, they are meant to further scientific inquiry; they cease to be so if, and so soon as, they are intended to block and to preclude any inquiry that promises scientific gain. Both also are capable of being used and misused. If belief in the "mechanical" nature of the world means the intention to employ to the utmost a bold working assumption which, after many crudities, blunders, and false starts, from Thales to Descartes, we have at last got to apply to a large proportion of happenings, it is a good thing and legitimate; if it means a dogmatic refusal to let any other methods of interpreting nature be tried, a wilful blindness to the differences between the different sorts of happenings, and a stupid ostracism of the inevitable question as to how the mind is to be placed in relation to the mechanical theory it has itself devised, it is a bad thing, because it allies itself with ignorance against the spirit of inquiry. Similarly, if vitalism means that vital processes are not to be investigated by "mechanical" methods, that their apparent differences are to be accepted as ultimate, that the vital is simply incalculable and "not mechanical," and eludes the methods of physics and chemistry; or again, that pseudo-explanations are to be given in terms of a "vital force" which we are forbidden to inquire into further, or even that the convenient distinction between "life" and "matter" must be taken as absolute and may not be questioned, then vitalism is essentially negative and merely obstructive, bad in method, and scientifically noxious. But if it merely pleads for permission to devise appropriate methods for dealing with the peculiar subject-matter of each science, and asserts the right of biology to pay regard to the peculiarities of "living" matter and to become as "independent" as its work requires, or that in the presence of "living" matter effects are observed which do not occur when matter is "dead," there can be no scientific objection to "vitalism."

A complication is, however, introduced by the fact that truly disputable extensions of vitalism exist. For example,

shall we hold that biology is entitled by the nature of its problems to operate with the conception of a real efficacy of mind, in spite of the fact that the (methodological) principle of the conservation of energy is usually so stated as to rule out the possibility that what is classified as "psychical" can initiate "physical" changes? If we grant to a science this licence to go on its own way without regard to the way it contradicts principles which are useful in another science, we must evidently appeal to the doctrine that conflicting hypotheses may be provisionally used. This will seem more reasonable when we recollect that originally all hypotheses were devised by us for our use. Or again, how much emphasis is it legitimate to put on the corollary that if vital phenomena are more than "mechanical," they are mechanically incalculable and "free"? Clearly if this is over-emphasised, it will conflict with the tacit scientific postulate that whatever it is desired to investigate must be assumed to be knowable. Hence it may be well to remind ourselves that what is not mechanically calculable need not be, on that account, incalculable altogether, and that actions and events may be foreseen also by an appeal to psychological principles. In both cases the more tolerant attitude towards these corollaries of vitalism will probably be to the greater advantage of science, and, if we adopt it, I can see nothing in Sir Oliver Lodge's pronouncements that would justify the rejection of his vitalism as anti-scientific.

But its vitalism is not the greatest stumbling-block of Sir Oliver Lodge's Address. His plea for "Psychical Research" is undoubtedly still more of a shock to the susceptibilities of many. Here again, however, I hold that the logic of science substantially justifies his attitude, even though those who see this may not all agree that the evidence accumulated up to date by Psychical Research is such as to generate in themselves a positive and assured belief that immortality has been proved.

An impartial logician, *i.e.* one who is aware of his personal bias and endeavours to counteract it, would I think at present feel unable to attribute such high value to the evidence in question. Not because he personally disbelieves it or fails to recognise that it is a considerable improvement on the evidence that was in existence when the Society for Psychical Research began its operations and for the first time in the world's history attempted to investigate the most momentous of all questions in

a scientific spirit and by scientific methods, but because he sees that the scientific conquest of this dim region of experience is only just beginning. The science of psychology is not yet sufficiently advanced to gauge with any confidence the limits of insanity, hallucination, error, self-deception, and fraud. Even where the good faith of the experience is not to be questioned, it is impossible to exclude a great variety of interpretations. The evidence is not yet recorded much better than that which we have for the ordinary occurrences of life, though its quality is appreciably rising. Its quantity also has increased, though it is still miserably insufficient for scientific requirements. But the most fatal defect in it is that it has not yet been really subjected to experimental control. It is still mainly observational in its nature, and so the conditions of the phenomena under investigation cannot be explored.

The result is that it has little or no logical "cogency" as against those whose bias impels them to disbelieve it, even though it has become dangerously attractive to many who merely wish to believe, and not to know. Disputes about "what Psychical Research has proved" must at present end in a drawn battle. For each disputant, by looking at what favours his own interpretation and viewing the evidence in the light of his bias, can justify his belief in his own eyes, though he usually fails to do so in those of his opponent. Neither party can, strictly, "prove" its case, and the great mass of mankind, which only wants to "believe," i.e. not to think, is indifferent, and does little to help either.

This being so, what, the logician may ask, are the conditions of proof in such a matter? It is in the answers given to this question that the mischiefs of false logic become most apparent. If we assume that no man has a right to believe in what is not fully proved, and that it is our duty to demand absolutely conclusive evidence before we lift a hand or stir a foot, and if it is good scientific method to employ every art of pettifogging prosecution and every resource of scientific ingenuity to crush every bit of evidence as it arises, it is clear that no proof will ever be forthcoming. We shall never get to the end we profess to aim at, because we shall never be allowed to take the first step towards it, and whatever facts may exist to be discovered we shall never find them, because we shall not permit ourselves to look for them. But if we lay claim to a right to experiment

and to risk beliefs, if we allow our logic to observe that absolute proof does not exist and that scientific proof is in its nature cumulative, that the objects of scientific research are always objects of scientific *interest* and *desire*, that facts which are not looked for are in general not seen, that nature everywhere insists that to find we must seek and usually contrives to hide away her most important treasures in the oddest corners, it will not seem credible that the procedure hitherto recommended and pursued deserves to be described as a search for knowledge at all. It will look rather like a clumsy and unfair attempt to burk inquiry, and it will have to be pointed out that if we wish to prove anything we must allow the evidence to accumulate and permit the theory to grow gradually more probable, until it is no longer worth a reasonable man's while to dispute its truth.

With such a reformed notion of proof the researches to which the psychical researchers addict themselves appear in a new light. They are no longer impossible, unreasonable, or anti-scientific. True, they are still *risky*, and demand the courage that braves the terrors of the unknown in a higher degree than most ; for they may fail altogether and lead to nothing, or to nothing that was desired or expected. But *this risk* is taken by every one who undertakes to extend the borders of science. They may also be difficult and protracted, and a weariness to the flesh. This again is not uncommon in scientific research. But both interscientific comity and the true interests of science demand that those who are here sinking a shaft into the unknown should not be thwarted and persecuted, but rather assisted, by all who are interested in the fullest exploration of the universe. Sir Oliver Lodge's eloquent appeal for toleration— " Allow us anyhow to make the attempt. Give us a fair field. Let those who prefer the materialistic hypothesis by all means try to develop their thesis as far as they can ; but let us try what we can do in the psychical region and see which wins "—is not only the voice of the good sportsman and the fair and open-minded man, it is also good empiricism and good logic, and, above all, an expression of the truly scientific spirit.

SIR OLIVER LODGE'S ADDRESS

II.—THE PHILOSOPHY OF SCIENCE

By H. S. SHELTON, B.Sc.

COMMENT on Sir Oliver Lodge's broad philosophical survey of the field of science, as might, perhaps, have been expected, has been concentrated on one point. Incidentally, in one short paragraph, this year's "boss scientist" (as Lord Rayleigh so fittingly put it) stated that the study of psychical research had convinced him that human personality survives bodily death. There is, needless to say, nothing new in the belief, nor in psychical research, and every one acquainted with Sir Oliver Lodge was well aware beforehand that such was his personal opinion. There is, in the address, no discussion of the evidence. The opinion is stated in very few words. It might, indeed, well be ignored as a minor feature were it not that the journalistic instinct of many critics has magnified it so as to make it appear the main topic of the address. Thus the campaign of journalistic headlines compels the writer, much against his inclination, to devote some space to the well-worn theme.

In so doing, it is as well, even though superfluous, to preface such remarks by saying that the subject is one on which the writer is much less competent to speak than Sir Oliver Lodge. Sir Oliver Lodge, in spite of his many scientific achievements, really has, during more than thirty years, found the leisure to study the details of the evidence investigated by the Society for Psychical Research. Of such matters the writer knows little and cares less. His only qualifications for making any comment whatever are some knowledge of psychology, a careful study (several years ago, which has not recently been renewed) of that monumental volume by the late F. W. H. Myers, *Human Personality and its Survival of Bodily Death*, and such common sense as nature has endowed him with and circumstances allowed him to retain. For what such qualifications are worth, he will now say, as briefly as may be, how the statement appears to him.

Sir Oliver Lodge, and other men of science who hold similar views, appear to fall between two stools. On the evidential side, the writer has found nothing, either in Myers' book or elsewhere, which could carry conviction to, or even merit serious consideration by, any one not naturally predisposed to form the "spiritualist" conclusions. On the other hand, if the evidence proves anything at all, it proves far too much, and it is more logical to go to those who, for nineteen centuries, have stated dogmatically, as a matter of faith, that human personality does survive bodily death, and, moreover, told us more about it, than to attempt, in an amateur way, to build up a little heresy of one's own. These statements will, perhaps, bear some amplification.

On the evidential side, all serious investigators proceed on the well-known philosophic maxim : "Entia non sunt multiplicanda præter necessitatem." In all attempts to establish, by observation or experiment, the existence of survival after death, the would-be investigator has to consider at least the following four explanations of any phenomena he may observe: (1) trickery, conscious or unconscious; (2) that striking series of facts which psychologists are slowly gathering together concerning hypnosis and dual and multiple personalities; (3) telepathy; (4) ghosts. He will not invoke (3) until he has exhausted (1) and (2) and all other known explanations. He will not invoke (4) until he has exhausted (3).

Taking these in order, with regard to the first, few will need reminding that a well-known conjuror has never yet failed to reproduce every phenomenon credited to "spirits" that has been brought before him. Moreover, he is also known to have remarked that, for the detection of trickery of this kind, he would place more reliance on the acumen of two smart schoolboys than in the whole Council of the Royal Society.

The second is, scientifically, a problem of surpassing interest. The curious series of facts constituting multiple personalities, and other allied phenomena, are adding an important province to the realm of psychology, and are, indeed, doing something to redeem that science from the charge of verbalism and futility. But why invoke the "spirits"? Are not all these phenomena as readily explained in a perfectly natural manner as sleep unconsciousness and dreams? Their evidential value is nil. And, moreover, the very fact of their existence supplies an alternative

explanation for many phenomena that might otherwise be taken as supplying evidence of " possession."

The writer is not prepared to admit that there is sufficient evidence for asserting the existence of telepathy. Even this must be regarded as not proven. But even if we grant, for the sake of argument, that such a thing does exist, none knows better than Sir Oliver Lodge that the " spiritualistic" hypothesis is not advanced one iota. All the materialist would thereby admit as proved would be that, as the larynx can emit and the ear receive the atmospheric waves of sound, as the eye can receive the ætherial waves of light, so the undifferentiated nervous matter of the brain has some residual power of emitting and receiving vibrations of a wave-length previously unsuspected.

If we admit such an idea, which in the present state of scientific knowledge it would be rash folly to admit, all that follows is that the possible explanations of any unexplained residuum of " spiritualistic" phenomena are so increased that the residuum ceases to be worth investigating.

The above line of argument, it should be noted, is one which both Catholic and Freethinker (and everyone else) can accept without detriment to any views they may hold on matters of religion. To the sceptic it will naturally appeal. And the Catholic, though he believes on faith that there is a life beyond the grave, is not thereby committed to the opinion that Sir Oliver Lodge and the Society for Psychical Research have a shred of evidence worthy of serious consideration.

It is with great reluctance that the writer passes to the other horn of the dilemma on which, both in this and cognate matters, Sir Oliver Lodge has impaled himself. But he is open to a criticism from another quarter quite as deadly as any the materialist can bring against him. The Catholic, also, is capable of speaking to him in tones of sound common sense.

"So you are convinced that human personality survives bodily death," we can imagine him saying, "are you? That is very interesting. Perhaps you have evidence. Perhaps you have not. Personally, I should not like to base my belief on your evidence. But let us suppose you have, what then? You think you are in communication with disembodied spirits. There is nothing impossible in that. But my religion teaches me that investigations of your kind are better not attempted. If you will not accept our faith, at least accept the fact that we

have not dealt with matters such as these for nineteen centuries without learning something. Take our advice and leave it alone." [1]

And really, as a matter of common sense, granted that there is anything in Sir Oliver Lodge's views, the subject is one on which the Catholic Church should be heard. To put it mildly, they are not novices. And the subject really is in their line. It may, perhaps, not have occurred to him that (in his own words) to believe everything or to believe nothing are the two most logical attitudes on the matter in question. [2]

The Catholic, also, will be interested in Sir Oliver Lodge's final assertion of the existence of a transcendent God. He will congratulate Sir Oliver on his power of reasoning, It happens to be one of the latest defined dogmas of the Catholic Church that the existence of God can be inferred by man's natural reason. That Sir Oliver has come to the same conclusion is a matter for congratulation. Many (like the writer), whose intellect fails to follow the course of reasoning in such high matters, will envy him his perspicacity and intellectual power. But, if he is convinced so far, why does he not drop all these attempts at amateur theology and see what Rome has to teach him? It is really the most logical course. One of our most prominent journalists once said:

"It may be, Heaven forgive me, that I did try to be original, but I only succeeded in inventing all by myself an inferior copy of the existing traditions of civilised religion. The man from the yacht thought he was the first to discover England; I thought I was the first to find Europe. I did try to found a heresy of my own; and when I had put the last touches to it, I discovered that it was orthodoxy." [3]

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[1] In fairness to the Catholics, it should be said that I have never heard of any objection from that quarter to psychological research.

[2] Not having the position or the world-wide repute of Sir Oliver Lodge, I think it desirable to state explicitly what should be obvious from the whole discussion, that I am not, in this article, compromising any reputation I may possess as a writer on philosophy and matters scientific by expressing positive opinions on matters of religion. I am merely putting forward points of view. The "religion of all sensible men" is certainly the standpoint of this article. But if the "boss scientist" *will* introduce matters like this into his address, what can the critic do but write journalese?

[3] *Orthodoxy*, by G. K. Chesterton, p. 17.

Very natural, no doubt, but why try to found a new heresy? We are reminded of the "religion of all sensible men "—" that's what sensible men never tell," certainly not in presidential addresses to the British Association for the Advancement of Science.

It is with a feeling of relief that we pass to other ground, and proceed to discuss topics with which Sir Oliver Lodge, and the writer, are more competent to deal. No greater injustice could be done to that able and scholarly address than the injustice which has continually been done, to concentrate criticism on its weakest point. To some extent Sir Oliver has himself to thank. He should have remembered that he was not alone in feeling the fascination of creating a sensation, and of discussing matters with which he is scarcely competent to deal. Nevertheless, it is as well to remind readers of this journal that Sir Oliver Lodge is a man of science, that his address was given to the British Association for the Advancement of Science, and, moreover, that, in dealing with matters of science, he showed not only specialist knowledge, but that broad, clear-sighted, philosophic insight into fundamentals which, even among men of science, is rarely found. It is to this side of the address that attention should be directed, and, on this side, it is worthy of the highest praise.

It has, for several years, been a favourite theme with the present writer that the abstractions of men of science are often and again mistaken for realities. In mathematical processes, the chain of reasoning is long and involved. In all such reasoning, in whatever sense the conclusions may be true, may be absolutely valid, that sense is not the sense of material concrete reality. Hence all such reasonings, if definite and actual deductions are made from them, must be submitted once more to the concrete process of observation and experiment.

Simple and obvious as these statements may appear, they have important consequences in all applications of scientific reasoning to philosophy, to cosmology, to the affairs of every-day life. Numerous practical proposals, advocated by men of science and others (especially others) on scientific grounds, if these considerations are fully worked out, appear speculative and unpractical. That all men of science should realise, as the broad-minded and eminent ones do, the real meaning of their results and the limitations of their methods, is one great object

contemplated by those of us who are desirous of founding an efficient and valid methodology.

The support of so eminent a man of science, given in so official a capacity and in so public a manner, is of the highest value. Many of the assertions contained in the address, the main trend and aspect of it, need only to be mentioned. " Science should not deal in negations, it is strong in affirmations, but nothing based on abstractions should presume to deny outside its own region"—an admirable and valid saying, to which should be added the corollary that, as all affirmation is, of necessity, denial of the contradictory, science should not presume to make dogmatic and confident assertions outside its own region. In short, the limits of the applicability of scientific truths require careful philosophical delimitation.

" All intellectual processes are based on abstractions. Science makes a diagram of reality, displaying the works like a skeleton clock. . . . The laws of nature are a diagrammatic framework analysed and abstracted out of the full comprehensiveness of reality." Let us disregard, for the moment, the particular applications and regard the principles. The statements are true, valuable, practical. They are of the greatest service to the right understanding of scientific truths, to common sense in common life, to sanity in politics, to the advancement of the wider aspects of human knowledge. It is the main object of this essay to ensure that they shall not be ignored, that they should not be buried out of sight by the concentration of attention and criticism on the detail with which we have already dealt, and which, in view of the importance of the main current of the address, would much better have been omitted. Before proceeding to some of the special applications, on which there are controversy and difference of opinion, it will be well to indicate the significance of these few assertions, to emphasise them, and to express appreciation of Sir Oliver Lodge's sound judgment and philosophic insight.

Concerning particular applications, space will only allow us briefly to consider one or two. One of these concerns the present-day developments known as non-Newtonian mechanics and the Principle of Relativity. The statements in the address are an admirable support to those who are pressing upon men of science the essential truth and importance of fixity in fundamentals. By mathematical analysis and experimental investi-

gation, we are continually increasing the detail of scientific knowledge. Such detail often leads to valuable results in the practical affairs of everyday life. But there is continually the danger that the mathematician and the physicist should (more or less unknowingly) turn themselves into metaphysicians and give explanations of their results which, to every common-sense mind, are intrinsically and obviously absurd. Any one can do this if they concentrate attention on one small point and ignore the comprehensiveness of reality. When the offender has this concentration combined with a certain degree of positive ignorance, we call him a crank. When his facts are newly discovered and such that a high degree of skill is required to note and classify them, he is a not uncommon type of scientific investigator, an exponent of ultra-modern physics.

Let us consider this very question of variable masses. The great axiom is—mass is indestructible, it is impossible for something to become nothing. But an ignorant man could well devise many experiments on seaweed, catgut, wood, any moisture-absorbent substance, and demonstrate conclusively that mass varies with the weather or the season of the year. " I have more catgut in winter, weigh it and see," you can imagine him saying. " The fundamental laws of chemistry are wrong." Now while it is perfectly possible to prove that he has not more catgut, but only more or less moisture obtained from the atmosphere, to do so conclusively would be a long and troublesome analytical process, which the crank would not understand, and to which he could readily make a number of objections.

The indestructibility of mass, of substance, is simply unprovable. It is an axiom to which we fit our observations. All that chemistry can do is to show that certain apparent changes of mass are only apparent. It traces in detail the distribution of certain masses under certain conditions.

The point of these observations lies here. Without examining in detail the experiments on the velocities of a and β rays, we are entitled to say that the experimentalist who informs us that mass is a function of velocity is giving us information every whit as absurd as the crank who informs us that mass is a function of the season of the year, and more so than the crank who thinks he has discovered a perpetual motion machine. The experiments, no doubt, are valid, but they have been misinterpreted. Sir Oliver Lodge says that there is

actually an accretion of mass with velocity. There are a number of interpretations possible. It may be that that of Sir Oliver Lodge is the correct one. But certainly that of the exponent of non-Newtonian mechanics is wrong. On this point, no words can be clearer than those of Sir Oliver Lodge [1]:

"That mass is constant is only an approximation. That mass equal to ratio of force and acceleration is a definition and can be absolutely accurate. It holds perfectly even for an electron with a speed near that of light. . . . I urge that we remain with or go back to Newton. I see no reason against retaining all Newton's laws, discarding nothing, but supplementing them in the light of further knowledge."

On the question of metageometry, the address is not so clear, but, here again, we can apply still further the underlying principles. In Riemann's space, a line returns on itself. In the space of Lobatschewsky, "parallel" lines bend apart. Does either of these or Euclidean space represent actual space? To this question there is only one possible answer. The line returning on itself is not straight, and the bending parallel straight lines are neither straight nor parallel. No possible experiments can alter or modify this fundamental. It may be that non-Euclidean geometry is applicable to real existent conditions. It may be that the parallaxes of very distant stars are negative, and there may be means of proving that the

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[1] There is, however, one point on which Sir Oliver Lodge is not quite clear. He speaks of variable masses, and compares electrons to raindrops or a locomotive. Elsewhere, he says: "The dependence of inertia and shape on speed is a genuine discovery and, I believe, a physical fact." The writer is prepared to admit this only on the same assumption that is applied to raindrops—that the additional mass comes from somewhere. It is not clear whether or no this is Sir Oliver Lodge's meaning. If not, I would add it as a corollary. I regard the indestructibility of mass as as fundamental an axiom as the unchangeability of space and time, and I am not aware of any more fundamental measure of mass than inertia.

The idea has occurred to me that electrons, when their velocity exceeds a certain amount, may meet with some resistance from the æther, and that the very experiments which have occasioned a doubt as to its existence may be an additional means of proving it. As I have not had an opportunity of witnessing the actual experiments, I merely put this forward as a suggestion and with all reserve. I am informed that there is some objection on the ground of the path of the β rays, but that the experiments have not been performed with sufficient care to enable us to speak definitely, hence the reserve. But, personally, on present knowledge, I am inclined to think variable resistance more probable than variable mass.

stars which, by astronomical measurement, are found to be nearer, should ultimately be discovered to be farther. On such a question it is possible to admit evidence.[1] A non-Euclidean æther is as metaphysically possible as a centaur or a hippogriff. A non-Euclidean space is as contradictory as a round square. Our material lines may bend; our rays of light may bend; but our straight lines are not straight unless they are straight. It may be that we always see crooked, but that is no reason why we should not think straight. The writer would urge not only that we go back with or remain with Newton, but that we go back to or remain with Euclid. Non-Euclidean geometry, non-Newtonian mechanics, and the Principle of Relativity are admirable examples of the coherence of thought whatever may be the material supplied to it as foundation, but they must not be mistaken for reality.

Some physicists would try to inform us that there is no velocity greater than that of light. It may be that it is so. It may be that the æther of space, which, in spite of the relativists, we must emphatically assert is an assumption almost essential to the explanation of the world as we see and know it, imposes an impenetrable barrier upon more rapid motion. Even here, however, there is no sufficient evidence. But the physicist who says that there is anything in velocity that prevents a greater speed than that of light is talking absurdly. Velocity and limit are contradictory concepts. It is a round square and a crooked straight line over again. Nor should it be admitted too hastily that no actual velocity can exceed that of light. Even here the physicist is extrapolating unduly. All experiments on high velocities necessarily have reference to minute *electrified* particles, and it may be that electrified and non-electrified bodies differ in properties such as these. Moreover, once again, all he can say is that his equations apply only to velocities smaller than that of light. Once again, as so often before in the realm of practical science, we are bound to demur that it is not allowable to extrapolate an empirical rule one iota beyond the point where it is experimentally proved. Prof. Dewar discovered the importance of this principle when he wrongly estimated the temperature of liquid hydrogen. And the rationale of relativism

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[1] It is as well to be explicit and say that I have never heard it suggested that there is any evidence of the kind. The matter here briefly touched I have treated more fully in two articles in *Mind*, No. 73 and No. 88.

is just what the methodologist is anxious to discuss with the relativist. The point, however, will readily illustrate the difference between matters on which evidence is admissible and those on which evidence is impossible. It is possible (but improbable) that no actual velocity can exceed that of light. By all means let us investigate the evidence and gather more when we can. It is impossible and inconceivable that the limit to velocity can be the velocity of light.

So rarely does it happen that men of science are also philosophers that we must express our gratitude to Sir Oliver Lodge for placing considerations like these in a clear light and for showing that there are explanations to all physical facts not at variance with the laws of thought. The laws of thought, as the greatest philosophers of all eras have pointed out in one way or another, are the conceptual framework which we throw over the material of perceptual reality. Why there should be laws of thought and why these should possess validity over and above the empirical rules we call the laws of science is a problem we cannot discuss here. It will suffice to point out that it is so, and that those of philosophic training have always recognised the fact. The mathematician and the physicist on this point are continually blundering. One generation, that of Kelvin and Tait, will use the laws of thought as mathematical reasoning, and will mistake them for the laws of things. We thereby get grotesque estimates of geologic time, and the *Dissipation of Energy*. The next generation will make the inverse mistake. They will discover the peculiar behaviour of certain things and will mistake the laws of things for the laws of thought. All we are entitled to say is that electrons, under certain conditions, behave in a certain manner. The certainty and security of fundamentals continually needs to be emphasised by those who deal with the wider aspects of physical science. Space is space, and there is no such thing as crooked space. Velocity is a concept which does not admit a finite limit. The ultimate entities of the Universe are constant in quantity. Something cannot become nothing. Action at a distance is inconceivable. Truths like these can be misapplied, but they are more fundamental than any derived from experiment.

To the writer, the above is the most important aspect of the address. To him, to speak candidly, the "spiritualism" is a hasty and unwarranted assertion. The discussion of the dogmas

of religion seems entirely out of place, and, moreover, the reasoning is of such a character that the writer is unable to follow. The remarks concerning the origin of life and the contrast between the views of Sir Oliver Lodge and those of his predecessor, Prof. Schafer, there is no space to discuss. The main value consists in the assertion of the fixity of scientific fundamentals. And for support on this point the methodologist, and the philosopher who really possesses knowledge of matters scientific, will be grateful. The details of scientific experiment and of mathematical calculation are problems which lie within the sphere of science. Their interpretation, their interrelation, their co-ordination, are problems which properly belong to philosophy. On this point Sir Oliver Lodge is truly philosophic, and his remarks deserve the most careful attention of the scientific specialist, who is naturally more ready to listen to one eminent in his own sphere than to those whose knowledge of matters scientific is less specialised. The address is a valuable asset to those who maintain, against opposition from both sides, that the co-ordination of the facts and theories of science lies within the sphere of philosophy, and, moreover, that the co-ordination should not be a shadow or a figment but a solid reality.

SOME VIEWS ON LORD KELVIN'S WORK

By GEORGE GREEN, D.Sc.

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THE work of Lord Kelvin is so fundamental and his fields of activity so diverse that it is practically impossible to estimate the benefits conferred by it in his own time and still less possible to estimate those yet to come from his moulding and directing influence in the movements of his time. Broadly speaking, his gift has been to teach us how to discover the processes of Nature and how to bring them into common use. His pioneer work in the training of his students in experimental physics was the foundation of the modern laboratory. His numerous inventions and his constant occupation with practical industrial affairs as the daily duty of his life have wrought improvements of the ordinary conditions of life that are enormous, have helped to revolutionise our industrial system, and have pointed the line of further progress by establishing research as an essential part of industrial enterprise. His collected patents are almost as bulky as the volumes of his collected scientific papers, and the subjects to which they refer are as valuable in their potency for the extension of knowledge as for good and useful daily service.

In the field of pure science we find the same feature of his work. He not only adds to our knowledge; he is the interpreter and dispenser to mankind of the great works of others. When not engaged in independent search he is shaping and transforming the ideas of others for the daily use of his contemporaries, and making their ideas more fit instruments for future work. He brought to light the work of George Green of Nottingham and revealed its value. What he received from Carnot and from Joule he expounded in applications to the whole domain of Physics, and defined the limitations to our use of energy by discovering the great principle of Dissipation of Energy and the Second Law of Thermodynamics. He took the discoveries of Faraday and the investigations of Helmholtz on Vortex

motion, and derived from them a theory of matter which has illumined the whole region of molecular physics.

To write a full account of Lord Kelvin's work in science is practically to write the history of modern science and to indicate the bearing of modern lines of investigation. In almost every branch of science his work is fundamental. His labours have been extended by later workers and each field developed in detail so that each worker realises the greatness of Lord Kelvin's pioneering achievements only in his own domain. In the presence of so extensive a volume of material no explanation is required regarding the subjects dealt with in the following pages other than that they constitute the present writer's main line of interest in the work of Lord Kelvin and fall in best with his experience. The intention is to sketch roughly his own personal work with a view to arriving at the foundation of his attitude to modern views on molecular physics, and to indicate the bearing of his later work, with the developments which it has received since his death in 1907, on modern speculations regarding the mechanism of radiation.

Some guiding principle is necessary to explain the apparently miscellaneous and diverse nature of his earlier papers. The ideas promulgated by Faraday, and his success in establishing a relation between Magnetism and Light on the experimental side, the scope of the work of Green, and the development by Stokes of the analogy between equilibrium conditions of elastic solids and viscous fluid motion, accompanied by Lord Kelvin's own success in connecting Flow of Heat with Electrostatics and Attraction, seem to have firmly rooted in his mind the conception of the underlying unity of physical processes and, thus early in his career, made the achievement of uniting the known laws of Nature within a single scheme a dominating ambition of his life. Evidence of this appears throughout his works. One of the most prominent features of his writings is his fondness for mathematical analogies. Almost from the beginning of his writings we can trace the conscious extension of his range along this line towards that " comprehensive dynamics of ether, electricity, and ponderable matter, which shall include electrostatic force, magnetostatic force, electromagnetism, electrochemistry, and the wave theory of light " (Baltimore Lectures, Preface).

One of his earliest contributions to the *Cambridge Math.*

Journal points out the analogy between the steady motion of heat and the chief theorems on Attraction, thus uniting flow of heat with flow of force in electrostatics and paving the way towards the banishment of action at a distance ideas in the latter subject. The support which such an analogy lent to the views then being put forward by Faraday is clearly indicated at the end of Thomson's paper "On the Elementary Laws of Statical Electricity," of date 1845; and there is little doubt that this discovery deepened his interest in Faraday's researches and gave his thoughts an added stimulus in the direction of physical theories. From the importance of his mathematical work his interests grew and extended to the region of practical physics under the influence first of Faraday and afterwards of Joule. Being also closely in touch with the work of Stokes "On the Friction of Fluids in Motion, and the Equilibrium and Motion of Elastic Solids," which virtually brought two new regions within the scope of his mathematical analogies, he was naturally inspired by Faraday's discovery, in 1845, of rotation of the plane of polarised light in transparent bodies by a magnetic field, to attempt and to achieve the elastic solid illustration of Electromagnetic actions. His paper "On a Mechanical Representation of Electric, Magnetic, and Galvanic Forces," which appeared in 1847, marks the consolidation of his views with respect to the medium of electromagnetic action. Lord Kelvin in later life never hesitated to employ action at a distance principles in his later speculations as to the constitution of atoms and their interactions, whenever insufficiency of knowledge made such tentative methods expedient, but Faraday's discovery of 1845 seems to have convinced him of the necessity for some elastic solid explanation of the actions manifested in the ether as Electrostatic, Magnetic, or Electromagnetic forces. This same paper, which marks his decision in this matter, practically adhered to throughout his life, assisted largely in the development of Maxwell's views towards the electromagnetic theory of light which he reached in 1864.

In this connection, as in other important decisions regarding the larger questions in Physics, it is evident that the mathematical bent of Lord Kelvin's mind largely determined his attitude towards physical theories. A quotation which he makes from Green's writings, regarding general matter-of-fact explanations of physical processes, reveals clearly his own attitude of

mind : " I have no faith in speculations of this kind unless they can be reduced to regular analysis." Overcaution and entire avoidance of speculation not warranted by the analysis delayed his full acceptance of the teaching of Joule. The absence of a definite physical basis for the formulas brought forward by Maxwell was an impassable barrier to Lord Kelvin's acceptance of them as substantial theory, in spite of their power to meet the facts. Wherever an element of uncertainty remained, as in the application of the Boltzmann Maxwell Law, or with regard to pressure of radiation and the manner in which thermodynamics was employed in the theory of that subject, his attitude was one of entire distrust.

The explanations, inspired by Faraday's discovery of 1845, of Electrostatic, Magnetic, and Electromagnetic forces by various types of Strain in an elastic solid, and the Dynamical illustration which he provided in 1856 for the action of Magnetism on Light and for the rotary action of transparent bodies on polarised light, are typical examples of his requirements in the way of satisfactory explanation. The latter paper led ultimately to the analogy for ether of a fluid constituted of imbedded gyrostats; but being, in the years immediately preceding 1856, engrossed with the difficulties of reconciling Carnot and Joule and, later, with the application of the principles of thermodynamics to gases, to electrolysis, to thermoelectricity, to magnetism and other subjects, and with the development of the doctrine of available energy, he naturally did not regard the realising of his " grand object " as an object for immediate pursuit. His constant appeal to analogy, however, in his writings of this period bears testimony to his constant review of the range of mathematical analysis to discover the most promising line of advance towards his object. And when the tide of his thermodynamic researches had spent its first rush in the full stream of investigation emanating from the two great energy principles, and when other interests could reassert their claims, the appearance of Helmholtz memoir on the dynamical theory of Vortex Motion in 1858 inspired him to fresh efforts to accomplish his original aim. The promise of success which the vortex atom theory of matter for a time held out spurred him to eager mathematical investigations. The memoir on Vortex Motion, read first in April 1867, was undertaken, according to his own statement, " to illustrate the hypothesis that space is continuously occupied by an incom-

pressible frictionless liquid acted on by no forces, and that material phenomena of every kind depend solely on motions created in this liquid." As the investigation proceeded, it branched off into a long series of additions to General Hydrodynamics, including Motion of Solids through a Liquid, Motion of a Viscous Fluid, Turbulent Motion of an Inviscid Fluid, and followed later by Wave Motion in Dispersive Media and Waves on Water.

The memoir on Vortex Motion, with the underlying idea of all material properties being ultimately due to motion, taken · with the series of investigations in Molecular Theory to which it gave rise, virtually introduces us to modern molecular theory and carries us almost as far forward in forming a mental picture of the world of atomic actions as it is possible to go without the knowledge of the electron. Its immediate effect was to lead Lord Kelvin on to a vigorous attack on a host of hydrodynamical problems connected with vortex filaments and their stability, and with the motion of free solids through a liquid. Associated with this work came the completion and publication in 1871 of the memoir, partly written in 1849, on the Mathematical Theory of Magnetism—now much enriched by the hydrokinetic analogies arising from his hydrodynamical investigations. The questions of stability of various configurations of vortices have reappeared again in connection with the modern electron theory, and have led to important results. Difficulties of the vortex atom theory arose, however, in connection with the velocity of sound in gases, and with the relation of inertia to temperature, and in the explanation of chemical combinations and atomic weights. The advance of the atomic theory of electricity, and the impossibility, in Lord Kelvin's view, of accounting, by the aid of the vortex theory alone, for the infinite variety of chemical substances, crystalline configurations, or electrical or chemical or gravitational forces, led to his abandoning it; but not before the investigation had opened up possibilities of accounting for the properties required in a medium capable of producing electromagnetic actions by some complex foundation of vortex motion in a liquid. In this connection it led to extended efforts "to construct, by giving vortex motions to an incompressible inviscid liquid, a medium which shall transmit waves of laminar motion as the luminiferous ether transmits waves of light," an idea advocated for many years by Fitzgerald.

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After examination of the matter in his paper of 1887, " On the Propagation of Laminar Motion through a Turbulently Moving Inviscid Liquid," in which he derived equations similar to Maxwell's, the turbulent ether full of vortical motion did not satisfy Lord Kelvin, owing to the uncertainty that irregularity would not arise in the properties of the medium within the period of a wave or vibration, due to possible rearrangement of the turbulent state of motion within it destroying its average homogeneousness.

The two lines of investigation arising from considerations of vortex motion had a permanent influence in determining Lord Kelvin's later views. The abandonment of the idea that ether is a fluid, presenting appearances of elasticity due to motion, turned him once more to seek for some form of elastic solid ether, as this seemed to him to present the simplest and the only certain foundation of any theory fulfilling the requirements of the wave theory of light. The failure of the explanation of atomic properties by motion turned him towards the statical foundations of atomic structure dealt with in the Baltimore Lectures and later papers, and made him regard it as " extremely improbable that differences of arrangement of atoms all equal and similar could suffice to explain all the different chemical and other properties of the great number of substances now commonly called chemical elements." Practically the whole of his later work, emanating directly from his philosophic views—comprising the last six papers of vol. iii of his Collected Papers which relate chiefly to the proposed gyrostatic structure for ether, with the whole of the Baltimore Lectures, and the papers in vols. iv, v, and vi on Molecular and Crystalline Theory, on Voltaic Theory, Radioactivity, Electrons, and on Waves on Water—constitute a pursuance of his original aim of reconciling Optical and Electromagnetic Theory on some elastic solid theory, and of finding a relation between matter and ether consistent with this view.

Amongst the later papers, the subjects which received most of Lord Kelvin's attention were those relating to Atoms and Electrons and Waves on Water. Of these, perhaps the most interesting, and that bearing most strongly on modern views on the electrical theory of matter, is the first-mentioned, which he refers to as Atomic Electrostatics. Here, as always in the matter of foundations, Lord Kelvin is conservative, preferring

the definite groundwork presented for the ether in his own compressible elastic solid theory of 1888, and for matter simply a substance acting on the ether with a force depending on the distance.   The introduction of eighteenth-century views of atoms as mere centres of force is of course merely tentative, as explained in Appendix A to the Baltimore Lectures, where the necessary relations of atoms and ether depending on their relative motion is discussed.   As a simplest case, for the atom of matter, Lord Kelvin assumes a spherical nucleus occupying a portion of space without excluding the ether.   The atom produces by its action on the ether condensation and rarefaction at different distances from its centre, the total quantity of ether within its boundary being the same as in an equal volume of space free of matter, so that the outside ether is undisturbed. In such an atom, the conditions of free mobility through space are fulfilled for velocities less than the velocity of light.   Beyond this the essential quality of the atom is its positive electrification, and the law of force experienced by an electron placed anywhere within it.   The latter is taken the same as the law of force due to a uniform distribution of positive electricity within the boundary of the atom.   That the material nucleus may have additional qualities of its own is a condition derived no doubt from the failure of the vortex atom theory.   Differences in quality between atoms may be due in part at least to the quantum numbers of the electrons required to neutralise each atom.   Lord Kelvin, however, expressly disclaims the idea that any theory of matter can be founded merely on the interaction of positive nuclei with electrons.   "We might be tempted to assume that all chemical action is electric, and that all varieties of chemical substance are to be explained by the numbers of the electrons required to neutralise an atom or set of atoms ; but we can feel no satisfaction in this idea when we consider the great and wild variety of quality and affinities manifested by the different chemical elements.   It is possible that the differences of quality are to be wholly explained in merely Boscovichian fashion by differences in the laws of force between the atoms, and may not imply any differences in the numbers of electrons constituting their quantums."   As to the influence of radiation, the atom is assumed to be unmoved by ether waves, which, however, set electrons vibrating about their positions of stable equilibrium within the atom.

With these fundamental assumptions for electron theory it is interesting to find how great a range of physical actions are illustrated by simple combinations of electrons and atoms in his article "Aepinus Atomised," Baltimore Lectures, Appendix E, 1901, and later papers. Electrification by contact between different substances and electrification by friction appear as actions in which a smaller atom robs a larger of its electron. The difference of potential energy of a system of two dissimilar atoms in their initial and final configurations represents the energy radiated in the impulses occurring in the process of separation and in the oscillations preceding the final settlement in the new equilibrium position. It is thus a constant for each encounter of atoms. The positions of equilibrium and conditions of stability of equilibrium for various numbers of electrons within an atom are discussed for systems involving as many as twenty-one electrons. The possibility of more than one position of equilibrium for a given number of electrons, and the definite amount of potential energy radiated in a change to the more stable configuration, are points of interest in relation to the requirements of recent theory. Similar questions of stability appear earlier in Lord Kelvin's work in connection with crystalline configurations and with respect to the equilibrium of groups of columnar vortices revolving round their common centre of gravity illustrated by Mayer's well-known experiment with floating magnets of 1878. These questions have been dealt with mathematically by Prof. J. J. Thomson in his "Motion of Vortex Rings" of 1883, and in more recent papers (in the *Phil. Mag.*, 1904 and later), where valuable illustrations of possible mechanics of radiation and radioactivity are given, involving suggestions as to emission of energy occurring in the passage of a system from one stable configuration of motion to another involving less kinetic energy. Quite recently, too, the structure of an atom has been given in which emission of energy takes place in discrete quanta in accordance with Planck's theory of radiation. The investigations of the same author on the corpuscular theory of matter prove that the capacity of the electron theory to account for atomic weights is probably much greater than Lord Kelvin supposed. Taking only the positions of equilibrium of electrons in one plane, and assuming that atomic weight is proportional to the number of corpuscles in the atom, it is possible to obtain a

scheme representing the chemical elements as arranged in Mendelcjeff's table (*Physical Review*, April 1912).

The "Aepinus Atomised" article of 1901 illustrates a host of electrical actions and electrical properties of solids, liquids, and gases, such as electrolysis, chemical affinity, heat of combination, electric conductivity of solids and its changes with temperature, specific inductive capacity, very much as on other electron theories. Combinations of atoms in various configurations are obtained to account for crystalline formations and for the electrical properties of crystals. The results of experimental investigations on Radioactivity, which Lord Kelvin followed with the keenest interest, prompted him to visualise the actions within radioactive bodies by constructing model atoms having the properties of radium and polonium, and to extend his system of atoms and electrons to account for the various types of rays which experiment revealed. A typical paper of Lord Kelvin's belonging to this group is the one entitled "Electric Insulation in 'Vacuum,'" in which he compares the force required to pluck an electron from its atom with the breaking weight of the strongest steel. The bearing of this work of atom construction on the explanation of spectroscopic series, and of the general mechanism of radiation, is discussed fully in Lord Kelvin's latest completed paper "On the Motions of Ether produced by Collisions of Atoms or Molecules containing or not containing Electrons" (*Phil. Mag.*, September 1907). A clear statement given in this paper of Lord Kelvin's views regarding radiation is of importance in relation to the extensions referred to in the following pages : "The pulses described in §§ 11, 12, as due merely to mutual collisions between ponderable atoms (without consideration of electrons whether present or not), constitute a kind of motion in the ether, which, if intense enough to produce visible light, would, when analysed by the spectroscope, show a continuous spectrum without the bright lines, which, when seen, prove the existence of long-continued trains of sinusoidal vibrations of particles of ether in the eye perceiving them, and therefore also in the source, and in all the ether between the source and the eye. On the other hand, the vibrations of electrons referred to in § 13 would, if intense enough, produce bright lines in the spectrum." The main difference between Lord Kelvin's views and current ideas regarding atomic structure lies in his choice of static con-

ditions for atoms and electrons, when undisturbed by collisions. The difference was not wholly due to the fact that static conditions lend themselves to a simpler discussion. It is natural, however, that experimentalists should prefer a stable configuration of motion, as no doubt the aspect of motion is the one most prominently before them. All speculations in this direction are at present merely tentative suggestions awaiting confirmation. It is interesting to find, however, Lord Kelvin's latest description of the atom, given at the British Association Meeting at Leicester 1907, as a gun loaded with an explosive shell, recurring in another connection (*Phil. Mag.*, October 1913, p. 579).

Turning now to the other section of Lord Kelvin's work referred to above, which occupied his attention from 1886 onwards, but more especially in his later years, we shall find that it is complementary to the papers just discussed. The section of his work included under the title Waves on Water, containing as it does some of his most beautiful applications of the Fourier analysis which attracted him so much in his student days, recalls his early intimacy with the writings of the French mathematical school, especially those of Cauchy and Poisson. For the beginnings of this series of investigations in his own writings, we have to go back to the early papers on Hydrodynamics contributed in conjunction with Stokes to the *Cambridge Mathematical Journal* before 1849. Hydrodynamical analogies are continually appearing in his work on Magnetism and Elasticity and Electric Currents, and, as we have seen above, in his philosophic speculations on matter. A group dealing with diffusion forms the subject of a separate paper in vol. iii. The influence of Stokes' Hydrodynamical Papers—on the application of the Method of Images, on waves and on the work against viscosity of water required to maintain a wave—no doubt accounts largely for his special interest in purely hydrodynamical waves problems. The experiments of Froude on resistance experienced by models towed through water, and Lord Kelvin's own acquaintance with the sea in cable and yachting expeditions, brought him directly in contact with the problems of ship waves and the action of wind in generating waves at sea.

The earlier papers of the group all belong to the purely hydrodynamical aspect of the subject. In 1871 he gave the theoretical explanation of the influence of wind and of surface

tension on Water Waves illustrated by experiments carried out with the assistance of Prof. Helmholtz and Prof. James Thomson on one of his yachting expeditions, when becalmed in the Sound of Mull. The series of "Stationary Waves in Flowing Water" is evidently undertaken with the view of leading up to the problems of Ship Waves and Waves due to Wind. The solution of the Ship Waves problem was obtained long before it was published (in 1906). In a manner the investigation of water waves was more or less a recreation study to Lord Kelvin, being a natural interest aside from his more pressing practical affairs and from the deeper problems of matter and ether, and yet bearing on both and providing scope for the applications of his skill in Fourier mathematics, of which these papers contain many examples.

But from 1884 onwards the main purpose of the continued series of papers on Water Waves ceased to be merely the hydrodynamical value of the solutions of the several problems with which they are concerned, though these are interesting enough in themselves, and though Lord Kelvin preferred to confine himself in the main to their strictly hydrodynamical bearing as continuations of the Stationary Waves Group. The main interest of the later hydrodynamical papers is to be found in their bearing upon Optical questions requiring elucidation, as is clearly indicated in Lectures V to X of the Baltimore Lectures. "Take any conceivable supposition as to the origin of light, in a flame, or a wire made incandescent by an electric current, or any other source of light. One molecule, of enormous mass in comparison with the luminiferous ether that it displaces, gets a shock, and it performs a set of vibrations until it comes to rest, or gets a shock in some other direction. . . . We thus see that light is essentially composed of groups of waves; and if the velocity of the front or rear of a group of waves, or of the centre of gravity of a group, differs from the wave velocity of absolutely continuous sequences of waves, in water or glass, or other dispersively refracting mediums, we have some of the ground cut from under us in respect to the velocity of waves of light in all such mediums. I mean to say, that all light consists of groups following one another irregularly, and that there is a difficulty to see what to make of the beginning and end of the vibrations of a group." In Lecture VIII we find later: "A question is now forced upon us,—What is the velocity of a group of waves in the

luminiferous ether disturbed by ordinary matter? With a constant velocity of propagation, as in pure ether, each group remains unchanged. But how about the propagation of light sequences in a transparent medium like glass?" References are also made to difficulties that might arise in connection with refraction or interference phenomena if these were dealt with by consideration of groups of waves. Then again, taking the production of light from a molecule as a sudden beginning of a long regular group of waves followed by a gradual falling off, we are confronted with the question, how would irregularity invade the regular group in its passage through a dispersive medium?

With the single exception of the papers dealing with Ship Waves, in which Lord Kelvin had a special interest arising from his earlier hydrodynamical work, the problems solved in the later Waves Papers are the water wave analogues of the Optical problems referred to in the above quotations. We have " On the Front and Rear of a Free Procession of Waves in Deep Water" appearing in 1887 and later in 1904. In his last paper we have the graphical solutions for the motions of a finite group of waves, and the effect of a sudden beginning of regular vibrations is represented in the problem of determining the effect of a suddenly applied periodically varying pressure acting at a certain region of the water surface. In this connection it may be of interest to note that the optical analogue of the Ship Waves problem is the problem of the passage of a plane light pulse from air into glass or other dispersive medium, and that the Ship Waves solutions have since been used to illustrate the modus-operandi of the prism. It is clear that the intention of these papers is to provide illustrations of wave motion which might provide definite information as to the process of dispersion, and which might be useful in helping to clear up some of the difficulties which still remained in connection with the theory of group-velocity and the propagation of waves in dispersive media. They were, so to speak, models illustrating the Optical problems referred to in the Baltimore Lectures and in his papers on Atoms and Electrons.

From this point of view the most important paper of the section is that entitled " On the Waves produced by a Single Impulse in Water of any Depth, or in a Dispersive Medium " (*Phil. Mag.,* 1887). In this paper the displacement pro-

duced by an Impulse delivered at the origin is given for place χ and time t by ξ where

$$\xi = \frac{1}{2\pi} \int_0^\infty dk \cos [k\{\chi - tV\}] \quad . \quad . \quad . \quad . \quad (1)$$

where $V = f(k)$, V being the velocity of the Fourier train of wave-length λ, and $k = 2\pi/\lambda$. When t is large, the effect at any point is due to the trains whose phases agree or nearly agree at the point chosen for observation. The remaining trains being infinite in number and differing in phase can be assumed to produce zero effect. Thus the predominant trains at point χ are determined by

$$\delta [k \{\chi - tV\}] = 0, \text{ or } \chi = t \{f(k) + kf'(k)\} = t U$$

where U is called the group-velocity of the trains which produce the maximum effect at place χ and time t. In this the idea of group-velocity is restricted simply to mean the principle of stationary-phase as employed by Prof. Lamb in his investigation of Ship Waves (Hydrodynamics, § 253), but applied to the Fourier trains which constitute any wave disturbance. When this view is accepted, the difficulties referred to by Lord Kelvin in the passage quoted above are removed; and the results to which it leads are consistent with the dynamical theory of group-velocity given by Osborne Reynolds and Lord Rayleigh.

Strangely enough, this is the meaning attached to group-velocity in Lord Kelvin's paper of 1887, and the key to the explanation of the problems regarding groups of light waves in glass, and indeed of any problem involving dispersion, lay unnoticed in his earlier work. The development of the fundamental process of dispersion along the lines laid down in Lord Kelvin's original paper was completed by Dr. T. H. Havelock in 1908, in his paper on "The Propagation of Groups of Waves in Dispersive Media," *Proc. R.S.*, vol. lxxxi, and by G. Green in *Proc. R.S.E.*, 1909. Lord Rayleigh, however, has pointed out that the principle of stationary phase applied to the fundamental Fourier trains, as indicated above, does not account for an instantaneous propagation of any disturbance which occurs in any dispersive medium, thus calling attention to a gap between initial actions and those determined by group-velocity theory, which would call for some new method of determining the value of the above integral.

It is unnecessary to do more than indicate here the wide field of applications of the principle of group-velocity by stating one or two recent investigations depending on it. It has been applied in the difficult problem of Ship Resistance to determine the part of the total resistance arising from wave-production in experiments with models. The theory has been extended by Lord Rayleigh to deal with the case of media in which there is minimum wave-velocity such as water, when the influence of gravity and surface tension combined is to be considered; and the same writer has discussed its application in the case of Aberration in a Dispersive Medium.

The questions which formed the basis of Lord Kelvin's investigations on Water Waves, as to the cause of the formation of the front and rear of groups of waves travelling in a dispersive medium, and as to the manner in which irregularity invades a group of waves originally regular, from the mere kinematical point of view, have been satisfactorily answered. In view of the smallness of light waves, the applications of principles primarily derived for the case of infinitely extended media to groups of waves in lenses and prisms is fairly direct; nevertheless, a consistent development of many parts of Optical Theory from the point of view of Group-velocity would still be a useful undertaking.

The questions raised by Lord Kelvin, however, have a physical as well as a geometrical aspect. The problem regarding the falling off from regularity of a group is simply, How is the distribution of energy to be determined when a regular group of waves enters a dispersive medium? The kinematical investigations in Lord Kelvin's work and its extensions are thus intimately connected with and are the necessary preliminaries to the study of the passage of energy by means of wave motion through a dispersive medium, and have thus a very important bearing on the modern Theory of Radiation. Some rather important results in this connection can be very simply derived from the solution given by Lord Kelvin in 1887 for the case of the waves produced by a single impulse in a dispersive medium. His evaluation of the integral in equation (1) is as follows:

$$\xi = \frac{\cos\left[k\left\{\chi - tf(k)\right\} \pm \frac{\pi}{4}\right]}{\sqrt{\mp 2\pi t\left\{2f'(k) + kf''(k)\right\}}} \quad . \quad . \quad . \quad (2)$$

where k determines the wave-length, λ, of the particular group of Fourier wave-trains which predominate at point χ at time t. The ambiguous sign in the denominator is to be chosen so as to make the expression positive. By the principle of stationary phase, the relation between k and χ is

$$\chi = t \{f(k) + kf'(k)\} = tU$$

where U is the group-velocity corresponding to wave-length λ.

Consider now the wave energy contained in the medium, at time t, from the place where wave-length λ predominates to the place where wave-length λ + δλ predominates, that is, the energy corresponding to wave-length λ. The extent of the medium concerned, at time t, is

$$\delta\chi = t \{2f'(k) + kf''(k)\} \, \delta k$$

and, as the energy per unit length of the medium is proportional to the square of the amplitude, the total wave energy in the medium associated with wave-length λ is as follows :

$$E\delta\lambda = (Amplitude)^2 \times A\delta\chi \text{ where A is a constant}$$
$$= constant \times \delta k = constant \times \delta\lambda/\lambda^2$$

Thus we arrive at the result that the energy corresponding to wave-length λ, and carried along through the medium, is independent of the time elapsed from the beginning of motion, and of the place in the medium where the Fourier trains of wave-length λ predominate, and of the dispersive quality of the medium itself. This means that the energy associated with each wave-length remains unchanged during its distribution throughout the medium, and is therefore the same at all times as the energy, belonging to the wave-length considered, in the initial pulse, before its resolution and transformation by the medium into energy of wave-motion. The same is of course true for any form of initial pulse, and the theorem is an illustration of the fact that the group-velocity U is the velocity at which a certain quantity of energy, that belonging to Fourier trains of wave-length λ, as given by Fourier's theorem, moves through the medium—a theorem proved originally by Lord Rayleigh for the case of a regular group of waves (*Sound*, vol. i. Appendix). One case of the theorem is that with any form of initial pulse the maximum energy per wave-length is always associated with the same wave-length, and depends only on the form of the initial pulse itself. The importance of the result in connection with radiation lies in the fact that radiant energy, emitted at a fixed temperature, has always the same distribution of the energy among the various

wave-lengths, and that the law of radiation given by Planck may be a statement of the distribution of energy per wave-length in a series of similar pulses which constitute the radiation, that is, without actual wave motion.

To test this idea, let. us take the case of two pulses following each other in close succession, and let us assume, as is generally done in connection with light pulses, that the component pulses are equal and opposite. If we consider the effect of the combined pulses at a point very distant from the source, the predominant wave-lengths at the point belonging to the positive and negative parts of the original disturbance will be very nearly equal. Thus for the case of the long waves, the theory of group-velocity indicates that the wave-length λ, and therefore k, varies very slowly with χ and therefore $\dfrac{dk}{d\chi}$ is nearly zero. Accordingly we may take as the expression representing the displacement due to the combined pulse

$$ u = \frac{d\xi}{d\chi} = \frac{- k \sin\left[k \left\{ \chi - tf(k) \right\} \pm \frac{\pi}{4} \right]}{\sqrt{\mp 2\pi t \left\{ 2f'(k) + kf''(k) \right\}}} $$

The energy corresponding to the region of wave-lengths from λ to $\lambda + \delta\lambda$, estimated exactly as in the case of a single pulse, is now

$$ E\delta\lambda = \text{constant} \times k^{3}\delta k \, ; \; \text{or, } E\delta\lambda = \text{constant} \times \frac{\delta\lambda}{\lambda^{4}} $$

This is of course the law of radiation arrived at by Lord Rayleigh by an application of the Boltzmann-Maxwell theorem of partition of energy to the ether in a closed rectangular space containing radiant energy. Again the result is true for any form of pulse consisting of equal and opposite parts, as has been proved by E. T. Whittaker in *Monthly Notices, Astr. Soc.*, 1906. The same note explains how, by an application of thermo-dynamics, we can deduce from the above that the radiation of a body at temperature T absolute is proportional to $T\lambda^{-4}\delta\lambda$. Thus, so far, the pulse form of radiant energy satisfies the requirements. It is impossible to proceed further without introducing some speculation as to the mechanism of radiation.

A prominent feature of modern doctrine with respect to the mechanism of radiation is the idea that the emission of energy takes place, not gradually, but in a statistically regular sequence of finite and perhaps nearly equal quantities, or quanta ; and it is suggested by some that the absorption of energy likewise

takes place by finite steps.   If we consider the radiation from a system of atoms and electrons, such as is presented by the kinetic theory of gases, enclosed in a perfectly reflecting enclosure at a fixed temperature, the view expressed by Lord Kelvin in the passage quoted earlier is that the emitted energy may consist largely of pulses due to collisions.   This view may still be regarded as in harmony with modern requirements of Planck's theory, as it would naturally involve the emission of energy in discrete quanta from a molecule, when an electron was expelled from it or detached by the influence of other molecules. The sudden expulsion would in this way constitute a pulse, of some definite form depending on the constitution of the atom, which would carry with it a definite quantity of energy into the enclosure.   As is pointed out above, there is no need to suppose that this energy takes the form of actual wave motion. The energy per wave-length is the same in the unresolved form of the pulse, as when resolved mathematically by Fourier's theorem or experimentally by any form of resolving apparatus ; and would restore the radiated energy to the system equally effectively in this form in the process of absorption as in the form of regular wave-trains.

These suggestions are similar to those put forward by Prof. Sir J. J. Thomson—namely, that the quanta of energy which have been proved to exist, do not indicate a molecular structure for radiant energy, but merely that emission occurs when some system within the atom is ruptured and that the change involves a definite quantity of energy.   It is clear that if we adopt the idea that the quanta of energy introduced by Planck are to be identified with pulses all of some definite form, considering the agreement of Planck's formula with experimental facts, we must regard these pulses as constituting practically the whole of the energy emitted by the radiating body.   In this line of speculation the steadiness of the emission of such a sequence of pulses at any given temperature is to be accounted for by the existence of some instability or weak connection in the atomic constitution, leading readily to expulsion of an electron or rearrangement of the atomic system in some new equilibrium configuration; and in the statistical steadiness of the conditions within the enclosure this instability may belong to all or, though less likely, only to one large group of the colliding molecules, all of this group being in a similar state of motion or atomic constitution which is subject to variation with increase of temperature. Some

such explanation would be necessary to account for the variation in frequency and intensity of the sequence of pulses with temperature, which is required to secure their agreement with the law of radiation given by Planck. At any rate, the form of Planck's law of radiation, with the distribution of energy per wave-length constant for any particular temperature, combined with the emission of energy by discrete quanta, which has been satisfactorily confirmed, strongly suggests some kind of pulse as the fundamental constituent in radiation.

It would therefore be extremely desirable to determine from Planck's law the form of pulse which would be in agreement with the law at any temperature, as this might lead to important information as to the actions going on within an atom to which radiant energy is due. The difficulty of such a problem is obvious, as the consideration of pulses all in one plane does not seem to comply with the actual conditions of the mechanism of radiation we have assumed. For the case of two dimensional motion, however, the form of pulse required for agreement with Wien's Law has been recently discovered by Dr. R. A. Houstoun, being published in *Proc. Roy. Soc.* He finds that the initial form of displacement

$$\xi = \frac{\cos \frac{5}{2} \theta}{(h^2 + x^2)^{\frac{5}{4}}}$$

where $\theta = \tan^{-1}\frac{x}{h}$, leads to the expression for the energy per wave-length,

$$E = \text{constant} \times \lambda^{-5} e^{-\frac{ch}{\lambda}}, \text{ with } c = \text{constant}$$

From the point of view taken in this article, it is important to remark that the above initial form is one of a series of initial forms given by Lord Kelvin for "Initiation of Deep Sea Waves" (*Proc. R.S.E.* 1906).

An important aspect of the pulse hypothesis with regard to the genesis of radiation referred to above is that the form of the pulse is understood to be definite at any given temperature, and accordingly the various characteristics of the pulse, or sequence of pulses, which vary with the temperature may be used as a measure of it. In the above, the variation of the constant h allows for the representation of pulses belonging to different temperatures, h being in fact inversely proportional to the absolute temperature. We also have $h^{\frac{5}{4}}$ inversely proportional to

the maximum ordinate in the initial pulse.   In this we are virtually connecting temperature with some definite characteristic of the group of molecules concerned in the emission of the sequence of pulses.   Certainly the artificial procedure of referring to temperature as something belonging to a space full of radiant energy of a definite constitution per wave-length does not arise in connection with the pulse view of radiation.   The applications of thermodynamics to an enclosure full of radiant energy are of course in reality applications to the matter within the enclosure. They of necessity depend on the existence of a pressure on the enclosure and on the radiating body applied by the radiation, and this has been fully established by experiment.

The above pages give in outline the developments in the direct line of Lord Kelvin's later work in both its aspects.   His attitude with regard to other lines of investigation on the law of Radiation may be understood from his opinions regarding the Boltzmann-Maxwell Law of partition of energy and regarding the pressure due to radiation.   With regard to the former, he says, in Appendix B of the Baltimore Lectures : " I have never seen validity in the demonstration on which Maxwell founds this statement, and it has always seemed to me exceedingly improbable that it can be true."   With regard to the latter, in a letter to Prof. Larmor of date May 8, 1907, already published in Proc. Roy. Soc. 1908, Obituary Notice, we have the statement: " There are certainly very wonderful ' push and pull ' forces in the action of light on movable bodies in high vacuum (and also in not very high vacuum, as shown in Varley's communication to Royal Society ' Proceedings ' of about 1871, demonstrating cathode torrent of ' negatively ' electrified particles).   I do not, however, think that there is any foundation for push and pull in Maxwell's (a, β, γ) formulæ, or in the (a, β, γ), (P, Q, R) of your leaves."   This latter subject has been cleared of uncertainties in the interval since Lord Kelvin's death.   The difficulties of the former still remain.   Lord Kelvin preferred the direct line of attack on the difficulties of the subject of Radiation on the ground clear of fundamental uncertainties presented by problems of Wave Motion.   The developments outlined in the preceding pages, mixed with some speculation on the rôle of the pulse in the genesis of radiation, serve to show how directly the main sections of his later work bear on the foundations of the modern theory of radiation.

THE DISPLACEMENT OF SPECTRAL
LINES BY PRESSURE

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Up to the year 1896, the Fraunhöfer lines in the solar spectrum had been regarded as fixed marks of reference, subject to no ·possible change in position. In that year, L. E. Jewell,[1] when engaged in carrying out some measurements of their positions for Rowland's "New Table of Standard Wave Lengths," discovered certain systematic differences between the wave lengths of the metallic lines in the solar spectrum and of the corresponding lines in the arc and spark spectra obtained experimentally: the differences of wave length were found to vary from line to line, proving that the displacements were not due to the Döppler effect, arising from a motion in the line of sight. He suggested, as a possible explanation, that the wave length of a line might depend upon the physical conditions under which it was produced, or, in other words, that the vibration period of an atom might depend to some extent upon its environment, and that presumably an increase of density or of pressure would produce a damping effect upon the vibrating and radiating systems.

Following upon this suggestion, Humphreys and Mohler[2] investigated experimentally the effect of pressure upon the positions of the lines in metallic spectra, by placing the arc in a vessel containing air, with an arrangement by which the pressure could be varied by known amounts. This proved but the commencing point of a long series of experiments by Humphreys,[3] Hale and Kent,[4] Anderson,[5] Duffield,[6] Rossi,[7] Gale

[1] *Astroph. Journ.* **3**, p. 92, 1896.
[2] *Ibid.* **3**, p. 114, 1896.
[3] *Ibid.* **4**, p. 249, 1896; **6**, p. 169, 1897; **22**, p. 217, 1905; **26**, p. 18, 1907.
[4] *Ibid.* **17**, p. 154, 1903.
[5] *Ibid..* **24**, p. 221, 1906.
[6] *Ibid.* **26**, p. 375, 1907; *Phil. Trans.* A. **208**, p. 111, 1908.
[7] *Proc. R.S.*, A. **83**, p. 414, 1910; *Phil. Mag.*, **21**, p. 499, 1911.

and Adams,[1] and others, who have shown that the phenomenon is a very complicated one. There are, in reality, two separate effects involved: as the density of the substance in the arc is increased the spectral lines are broadened, in some cases symmetrically, in others very unsymmetrically towards the red; and superposed upon this broadening is a progressive displacement of the lines towards the region of longer wave lengths. Gale and Adams[2] divide the various spectral lines into five main classes according to their behaviour under pressure:

1. Lines which are symmetrically reversed. These lines are the ones which are most readily and most accurately measurable, and in general are amongst the strongest lines in the spectrum.

2. Lines which are unsymmetrically reversed. These lines are not so numerous as those belonging to the first class, and the reversals are as a rule fainter. Most of the *enhanced* lines[3] belong to this class.

3. Lines which remain bright and fairly narrow under pressure.

4. Lines which remain bright and symmetrical, but become wide and diffuse under pressure. Most of the lines in the metallic spectra belong to these two classes, whose distinction is more or less arbitrary.

5. Lines which remain bright and are widened very unsymmetrically towards the red. These lines are almost all in the yellow-red portion of the spectrum and are all enormously displaced, but owing to the lack of symmetry and the extent of the widening, it is difficult to measure the displacement with any great degree of accuracy.

They also found that most of the characteristics of the lines in the arc spectra under pressure were retained in the spark spectra, but that in the latter case the lines were much more diffuse, and the accuracy of measurement was accordingly correspondingly reduced.

Experiment has shown that the amount of the displacement is practically independent of whether the line is or is not

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[1] *Astroph. Journ.* 35, p. 10, 1912.

[2] Gale and Adams, *ibid.* p. 15.

[3] The *enhanced* lines are lines which appear in the spectrum when strong spark discharges are used. Their presence indicates the characteristic difference between the spark and the arc.

reversed ; that is to say, other things being equal, emission and absorption lines are similarly and equally affected. The displacement has also been found to be independent of the nature of the gas surrounding the arc. This indicates that the phenomenon is primarily due to a change of density of the metallic vapour rather than to a change of pressure. The two terms have been used rather loosely and indiscriminately, but the distinction is of some importance from the theoretical point of view. All the facts point to the displacement as arising from the closeness of the packing of the radiating molecules. It is certain that with increase of pressure of the surrounding gas the density of the metallic vapours in the arc increases, because the electrodes are found to burn away faster, reversals become more frequent, and there is an increase in the brilliancy of the arc. Moreover, as Larmor has observed, " mechanical pressure arises merely from the translatory motions of the molecules, and these are so slow as hardly to count in connection with radiation periods."

All the experiments agree in proving that the displacement is proportional to the increase in pressure, at least for pressures up to a limit of 100 atmospheres ; and also that it increases with the wave length for lines of the same series. There has been some diversity of opinion as to the actual law of its dependence upon wave length, and a knowledge of the law is of great importance as a means of testing the theory. Humphreys's experiments seemed to indicate a linear relation, and indeed Sanford,[1] in discussing this subject, has used the fact that certain theories do not give a linear relation as an argument against them. More recent investigations have, however, negatived this result. Duffield[2] found that a linear law would not hold in the case of the spectra of iron, gold or silver, and that the displacement varied with a higher power of the wave length than the first. Rossi[3] showed that, in the case of vanadium, " the displacement seems to be roughly proportional to the square or a higher power of the wave length." The more complete investigation of the iron spectrum by Gale and Adams[4] has shown that, in this case, the displacement varies as the cube of the wave length. By plotting their results upon a large scale, with wave lengths as abscissæ and displacements as ordinates, they found that the

[1] *Astroph. Journ.* **35**, p. 3, 1912.   [3] *Astroph. Journ.* **34**, p. 21, 1911.
[2] *Loc. cit. ante.*   [4] *Loc. cit. ante.*

lines in the spectrum could be separated into four well-defined groups: the displacements of the components of each group varied as the cube of the wave length, whilst the displacements in the four groups are in the ratio $1 : 1 \cdot 5 : 3 \cdot 4 : 6 \cdot 6$. For titanium, on the contrary, the experiments indicate a dependence upon the square, and not upon the cube of the wave length.

The same experimenters obtained some interesting results in connection with the enhanced lines of titanium. In general it was found that there was but little difference between the displacements of corresponding arc and spark lines at the same pressure: the enhanced lines provided the exception, for with them the displacements of the spark lines are much larger than the displacements of the arc lines. In general also, the displacements of the spectral lines are practically identical, whether the arc is surrounded by hydrogen or carbon dioxide, but with the enhanced lines, which are known to be strengthened in a hydrogen atmosphere, there was an increase in the displacement amounting to about 25 per cent. at a pressure of 4 atmospheres.

These results are of importance in the study of solar phenomena, and as Gale and Adams remark: "The fact that the enhanced lines show materially larger displacements both at the sun's limb and also under pressure than do the other lines, strengthens greatly the view that pressure is the effective agent in producing the solar displacements." In the sun, of course, there are necessarily a number of complicating factors, such as scattering and absorption and varying displacements due to different levels; but a more complete study of the whole phenomenon in all its aspects should be of considerable value in any discussion of the relative merits of the various solar theories. In determinations also of the radial motions of stars, based upon the measurements of the displacements of spectral lines due to the Döppler effect, the possibility of there being a displacement due to pressure must be considered: if the reversing layer of a star is under heavy pressure, the displacements resulting from this cause will be quite appreciable.

To explain these phenomena several theories have been advanced. The first attempted explanation was based upon Lommel's[1] theory of absorption and fluorescence, and attributed the phenomena to the damping of the vibrations to which the

[1] *Wied. Ann.* **3**, p. 251, 1878.

emission of light is due. The theory was in agreement with observation in so far as it required a displacement of the bright emission lines towards the region of longer wave length to follow the increased damping consequent upon the closer packing of the molecules; but for the absorption lines, it required a widening unaccompanied by any displacement, whereas experiment shows that both emission and absorption lines suffer the same displacement. A modification of the theory was attempted by Wilsing,[1] but was unsatisfactory, and we must look to the electric rather than to the mechanical properties of the medium to find an explanation.

Such an explanation was offered by Fitzgerald,[2] who supposed that when the pressure was increased the luminous vibrations were slowed down owing to the increased specific inductive capacity of the medium in which the vibrations take place. In fact, if we imagine the vibrating systems as small Hertzian oscillators, vibrating in a medium of specific inductive capacity K, the frequency, N, of the vibrations emitted is such that N^{-2} varies as K. Thus when K is large N is small. Now an increase of pressure causes an increase in the specific inductive capacity of a gas, and so it follows that there is a *vera causa* for some shift towards the longer wave lengths of the emitted vibrations. The same argument has been in a more general form expressed by Sir Joseph Larmor[3] thus: " Each molecule individually, through the agency of its plastic field of force or æther strain, provides a yielding region in the æther in which the effective stiffness is diminished. The elastic energy which maintains the free vibrations of the radiator is located in the field of force in the adjacent æther; and, by dynamical principles, any loosening of the constraints in that field such as an adjacent molecule would produce, which would itself be somewhat intensified by equality of period, must in general tend towards increasing the free period, involving displacement of the radiation towards longer wave length."

Humphreys,[4] using this theory, obtained a pressure shift about three hundred times larger than the observed value. The calculation was, however, implicitly based upon the assumption that the surrounding medium was continuous right up to the vibrator in question. This is not permissible; since we are

[1] *Astroph. Journ.* 7, p. 317, 1898.   [2] *Ibid.* 26, p. 120, 1907.
[3] *Ibid.* 5, p. 210, 1897.   [4] *Ibid.* 26, p. 30, 1907.

not dealing with statistical or averaged effects, but considering a single vibrator, some hypothesis must be made as to the molecular constitution of the medium. This was done by Sir Joseph Larmor,[1] who considered a spherical vibrator of radius a, which acts as a simple Hertzian oscillator, and replaced the surrounding gas by a medium of specific inductive capacity K, assumed continuous, but extending only up to a distance ka from the centre of the vibrator. Since the electric field of such a vibrator varies, as regards distance, according to the inverse cube law, the static energy in the field outside and up to a distance r from the centre of the oscillator, supposed alone in free æther, is proportional to

$$\int_r^\infty r^{-6} \cdot 4\pi r^2 dr \text{ or } \frac{4}{3}\pi r^{-3}$$

and so, in the case considered, since where the specific inductive capacity is K, the electrical energy is altered as compared with a vacuum in the ratio K^{-1}, it is evident that the total static energy is altered in the ratio

$$a^{-3} - (ka)^{-3}(1 - K^{-1}) \text{ to } a^{-3}$$

and since the frequency is increased as the square root of this ratio, it follows that

$$\frac{d\lambda}{\lambda} = \frac{1}{2k^3} \cdot \frac{K-1}{K}$$

The value of K which occurs in this equation is not the specific inductive capacity as determined by ordinary static experiments, but the value appropriate to light waves of a frequency corresponding to the wave length λ, and by the electromagnetic theory of light is defined by means of the relation $K = \mu^2$, in which μ, a function of the wave length, is the refractive index of the gas for the wave length λ. Thus Larmor's theory gives a displacement of amount $d\lambda$ where

$$\frac{d\lambda}{\lambda} = \frac{1}{2k^3} \cdot \frac{\mu^2 - 1}{\mu^2}$$

Using the value of μ for air at normal temperature and pressure, and the observed values of $d\lambda/\lambda$, Larmor obtained k=8, and concluded that the dielectric influence of the surrounding medium is a *vera causa* of the right order of magnitude. It seems more reasonable, however, to use the value of μ corre-

[1] *Astroph. Journ.* **26**, p. 120, 1907.

sponding to the arc temperature (say $2730°$ abs.). The value of (μ^2-1) is then reduced to one-tenth of its previous value, and, with this modification, the theory gives $k = 3$, *i.e.* the surrounding medium must be regarded as extending to within a distance of three times the molecular radius from the vibrating molecule; but at this temperature the average distance apart of the molecules is about sixty times the molecular radius. It seems, then, as though the above estimate of k is much too small, and if a larger value be substituted in the formula for $d\lambda$, the displacement obtained is much smaller than the observed value.

Moreover, if this theory were true, the displacement should vary with the nature of the gas surrounding the arc. In fact, for a gas μ is nearly unity, and $(\mu-1)$ varies as the density (Gladstone and Dale's Law), so that the displacement obtained should be proportional to the density of the surrounding gas. For example, in the case of arcs in atmospheres of air and carbon dioxide at the same pressure, the displacements should be respectively in the ratio of $2:3$, whereas Rossi was unable to obtain any differences in the displacements in the two cases beyond the limits of experimental error. Gale and Adams did indeed find an effect in the case of the enhanced lines of titanium, but it was in a direction opposite to that given by the above formula. Another objection to the theory is that experiment shows that the displacement varies accurately as the pressure. Now, for a gas (μ^2-1) is proportional to the density or pressure, and k^3 may be expected to vary approximately inversely as the pressure, so that this theory requires a displacement varying as the square of the pressure. Further, other things being equal, $d\lambda$ is proportional to λ, and this has been disproved by experiment.

It must be concluded that although the effect of the surrounding medium pictured by Larmor and Fitzgerald must exist, the resulting displacement is many times smaller than that experimentally observed, and that an explanation of the facts must be sought in another direction.

A different theory which was advanced by Humphreys[1] attempted to explain the effect by means of the mutual interaction of atomic magnetic fields, in a manner analogous to the Zeeman effect. It is well known that the periods of the radia-

[1] *Astroph. Journ.* 23, p. 233, 1906.

tions emitted by a source of light are changed under the action of a magnetic field. Humphreys argued that this being so, the luminous particles must have a magnetic field of their own, and consequently, since they can be acted upon by an external magnetic field, they must of necessity be acted upon by the fields of the neighbouring particles. He took for his model of the atom that pictured by Sir J. J. Thomson, in which a number of coaxial rings of electrons rotate inside a sphere of positive electricity uniformly distributed; and with certain assumptions as to the radius of the sphere and the number of electrons contained in the atom he was able to calculate the strength of the atomic magnetic field. In the case of the iron atom, by assuming it to contain 5,000 electrons the strength of the magnetic field at the centre of the atom was found to be $5\pi \cdot 10^7$ C.G.S. units. Humphreys [1] also found that the observed displacement could be accounted for by means of a strength of field of $45 \cdot 10^7$ units. This is a field ten thousand times as strong as the field of the strongest electromagnet used in producing the Zeeman effect, and it seems *a priori* improbable that the atoms of all metals could have such enormously strong magnetic fields without their existence being revealed in other ways. Humphreys admitted that the result was, at first sight, somewhat startling, but argued that the magnetic properties of atoms when luminous might be vastly different from the magnetic properties of cold masses of the pure elements. The Zeeman effect, however, seems to disprove the existence of atomic fields of such magnitude. In the elementary theory of this effect, the assumption is made that the atomic magnetic fields are small compared with the externally applied field, and the agreement between the calculated separation of the components into which the original line is resolved and the observed separation is sufficient justification of the assumption. The error in Humphreys' calculations appears to lie in the assumption as to the number of electrons which are contained in an atom.

Modern researches in connection with radioactivity indicate that this number is roughly equal to half the atomic weight. This may be, and probably is, an under-estimate. A model of the hydrogen atom which contains only one electron appears to be too simple and too unstable to be the true one : yet it should

[1] *Astroph. Journ* **27**, p. 194, 1908.

be noted that Dr. Bohr[1] has recently, using this model and basing his work upon Planck's theory of the discontinuity of emission of energy, obtained an explanation of Balmer's formula for the positions of the lines in the hydrogen series, although the discussion was not completed by finding whether the lines have their proper intensities. But even if this estimate of the number of electrons in an atom is not accurately true, it is certainly very much nearer the truth than is the number assumed by Humphreys. If then instead of supposing the iron atom to contain five thousand electrons we suppose it contains only thirty, the atomic strength of field is only the one-four hundred and fiftieth part of that necessary to account for the observed separation. The conclusion is inevitable that the atomic magnetic fields are such that their mutual influence is entirely negligible, and incapable of accounting for the observed pressure shift. There are other considerations which justify this conclusion. As Humphreys himself states, if this theory were true the lines which give large Zeeman effects should also show large pressure displacements, whilst those with small Zeeman effects should be shifted but little. The connection between these two phenomena has been investigated by King,[2] who compared the Zeeman separation of a large number of lines with their pressure shifts as determined by Humphreys, and found a complete lack of connection: for example, in the case of iron, the ratio of the Zeeman displacement to the pressure shift varied from 0·78 to 15·5, and in the case of several lines showing large pressure displacements, no Zeeman effect could be observed even with the most intense magnetic fields.

Prof. O. W. Richardson[3] formulated another theory which sought to explain the displacement by means of sympathetic vibrations occurring in the surrounding atoms. To quote his own words: "The fact that an atom A is emitting light shows that it is surrounded by an alternating field of electric force. This alternating electric field will produce forced vibrations of equal period and, under certain conditions, of like phase in neighbouring atoms. The electric field due to the forced vibrations will react upon the emitting electron in the atom A and in such a way—as will be shown—as to increase the period of the

[1] *Phil. Mag.* July and September 1913.
[2] *Astroph. Journ.* **31**, p. 433, 1910 ; **33**, p. 250, 1911,
[3] *Phil. Mag.* **14**, p. 557, 1907.

latter. It will be necessary then to calculate the reaction at A due to the forced vibrations set up in the atom at B by a given vibration at A, to sum this up for all the atoms B which occur, and to find the effect of the resultant reaction on the period of A."

Working on these lines, by a straightforward but rather tedious piece of analysis Richardson arrived at a displacement $d\lambda$ of the wave length λ given by

$$\frac{d\lambda}{\lambda} = \frac{e^2\lambda^2(\mu^2 - 1)}{6\pi^2mc^2a^3}$$

where μ is the refractive index of the surrounding gas, c is the velocity of radiation in free æther, e and m denote the electronic charge and mass, and a is the radius of a sphere within which it is impossible for the centre of an atom of class B to lie and is supposed to be between a and 2a, where a is the atomic radius. Supposing that the surrounding gas is air at the arc temperature (2730° abs.), so that $\mu^2 - 1 = 5 \cdot 9.10^{-5}$, and taking for a a mean value of $1 \cdot 5.10^{-8}$ cms., Richardson calculated that for a wave length $\lambda = 4.10^{-5}$ cms., $d\lambda/\lambda = 9.10^{-5}$, which is about one hundred times as large as the average value obtained experimentally for the wave length used. The cause of this discrepancy is easily found. Richardson first calculated the effect of one atom of class B on the atom A, and then obtains the effect for the whole of the surrounding gas by multiplying this by the number of atoms per unit volume and integrating throughout the whole volume external to the atom A. Thus the surrounding medium was implicitly treated as continuous right up to the vibrating molecule A, which, as has been remarked above, is not permissible. If, as in Larmor's theory, the surrounding medium be treated as continuous outside a sphere of radius ka concentric with the atom, and if k be calculated from the above formula using the observed mean value of $d\lambda/\lambda$, it is found that k is approximately 5, which is too small since the atoms of the gas are, on the average, under these conditions at a distance apart which is equal to fifty or sixty times the atomic radius. If, on the other hand, a larger and more probable value of k be used, $d\lambda/\lambda$ is again much smaller than the observed value and the theory cannot be regarded as affording an adequate explanation of the pressure shift. Even apart from the numerical disagreement there are the additional objections that, as with the theory of Larmor, it gives a shift proportional to the square of the

pressure and also to $(\mu^2 - 1)$, both of which relations are contradicted by experiment.

There remains only one other theory which need be seriously considered and which has been advanced independently by Livens[1] and Havelock.[2] This theory will be discussed in somewhat greater detail as, in the author's opinion, it is probably the correct one. It has been mentioned above that it is certain that, under the conditions of the experiments, an actual increase in the density of the metallic vapour in the arc takes place simultaneously with an increase in the pressure of the surrounding gas. It is to this density change in the incandescent vapour that the present theory attributes the observed displacement. The theories of Larmor and of Richardson both attempted to explain it by means of some influence exerted by the surrounding gas, and in both cases they were found incapable of accounting for a displacement of the observed magnitude. No account was taken by them of the neighbouring metallic atoms of the same free period. Richardson[3] indeed expressly ruled these out ¦of consideration by asserting that their effect is to cause only a broadening of the lines, unaccompanied by any displacement, but no reasons were given to justify the statement.

Thus the present theory is concerned with the vibrations emitted, not by a single vibrator, but by an aggregate of similar vibrators with the same free period. The method of procedure consists in forming the equation of motion of a typical electron and then making a summation with respect to all the electrons in a unit of volume. The essential point of the theory consists in the introduction into that equation of a force acting upon the electron and arising from the electric polarisation of the surrounding medium. It is assumed in order to satisfy theoretical requirements that each electron may be surrounded by a sphere of a radius sufficiently large for it to contain a great number of electrons, but yet, at the same time, small when compared with the wave length: the matter inside this sphere is imagined removed. Then if a single electron is placed at its centre O, the force on this electron when an electric field of strength E is acting is not simply eE, as it would be if the electron were

[1] *Phil. Mag.* p. 268, August 1912.
[2] *Astroph. Journ.* **35**, p. 304, 1912.
[3] *Loc. cit. ante.* p. 562.

completely isolated: there is an additional term arising from the polarisation of the surrounding matter, and since only the matter in the immediate neighbourhood of the electron produces any appreciable effect on it, this polarisation (which is, of course, a vector quantity) may be assumed constant and equal to its value at O ; and just as, in the theory of magnetism, any distribution of magnetism may be averaged out into a volume and surface distribution of "imaginary magnetic matter" so, in the present case, the effect of the polarisation of the medium is equivalent to that of a surface distribution of electricity on the wall of the spherical cavity, of density $P\cos\theta$ at any point, where P denotes the magnitude of the polarisation and θ is the angle between its direction and the line from the point to the centre of the sphere, and so the force on the electron is at once obtained as $\frac{4}{3}\pi eP$ in the direction of P. If now the matter which was removed from the sphere be replaced there will, due to it, be an additional force, esP, which, as Lorentz[1] has shown, vanishes if the molecules have a regular cubic arrangement. In general for a gas, S will be small, and the complete expression for the force of the typical electron due to the electric intensity may be written in the form $e(E + 4\pi aP)$, where a is approximately equal to one-third in the case under discussion, but for solids and liquids it may depart widely from its value. The polarisation is analogous to the magnetic vector called the "intensity of magnetisation" and defined as the magnetic moment per unit volume. It is equal to Σer, where r denotes the displacement of any electron from its position of equilibrium and the summation is with regard to all the electrons per unit of volume.

The equation of motion of the typical electron when vibrating under the action of an external periodic electric intensity E may accordingly be written in the form

$$m\ddot{r} + h\dot{r} + mn_0^2 r = e(E + 4\pi aP)$$

The term $h\dot{r}$ represents a frictional or resistance term. Its presence is found to be necessary to account for the phenomena of absorption and of selective dispersion, although its exact physical significance is obscure. Lorentz sought to explain it as arising from the disturbance of the motions of the electrons consequent upon molecular collisions, but although his hypo-

[1] *Theory of Electrons* (B. G. Teubner, Leipzig), p. 306.

thesis gave a term of the above type, its magnitude was too small to account for the observed facts of absorption. The term $mn_0^2\,r$ is a force of elastic type, tending to draw the electron back to its mean position. The electron, if isolated, and under the action of this force, would emit radiation of frequency n_0, which may accordingly be called the "natural free period" of the electron.

The root of the present theory is contained in the fact that the electron, when in the presence of other electrons, will emit radiation of a frequency differing from n_0. It has been shown by Larmor (vide *Æther and Matter*) that a system of electrons will emit no radiation if, and only if, a certain condition holds, viz. that

$$\Sigma e\ddot{r} = 0 \text{ or } \ddot{P} = 0$$

If then a gas is in such a condition that it is emitting radiation, $\ddot{P}$ must be different from zero, and it must be concluded that it is electrically polarised. The gas on the whole will not necessarily exhibit any signs of polarisation, because the polarisation will change rapidly from point to point in both magnitude and direction, but in the neighbourhood of each point it must be assumed that there exists a polarisation P definite as regards magnitude and direction, so that the equation of motion of an electron in it is given by

$$m\ddot{r} + mn_0^2 r = 4\pi aeP$$

The frictional term has been dropped from this equation because it only becomes important when the electron is acted upon by a periodic force whose period is nearly equal to its own natural free period. That this is permissible is also evidenced by the fact that light from a flame or arc may be made to interfere with a path difference of millions of wave lengths, showing that the electrons maintain their vibrations undamped through an enormous number of periods.

Consider, therefore, the ideal case of a system consisting simply of N similar electrons per unit of volume. Then $P = Ner$, and may be eliminated from the equation of motion giving

$$m\ddot{r} + (mn_0^2 - 4\pi Nae^2)r = 0$$

and so radiation is emitted of a frequency n given by the equation

$$n^2 = n_0 - 4\pi Nae^2/m$$

or, since the second term is found to be small, n is given by

$$n = n_0 - 2\pi Nae^2/mn_0$$

Thus the frequency of the emitted radiation differs from the natural frequency by an amount

$$dn = - 2\pi Nae^2/mn_0$$

and the corresponding change of wave length is given by

$$\frac{d\lambda}{\lambda} = - \frac{dn}{n} = \frac{Nae^2\lambda^2}{2\pi mc^2}$$

since $\lambda = 2\pi c/n$.

Now the number of electrons per unit volume may be assumed proportional to the density and therefore to the pressure ; and so when the pressure is increased there is an increase in the wave length of the emitted light which is proportional to the increase in pressure and also to the cube of the wave length. The increase is moreover independent of the nature of the gas surrounding the arc. These results are all in accordance with experiment.

In the more general case in which the gas emits a number of spectral lines, corresponding to electrons with different free periods, P is given by ΣNer, the summation being with regard to the different free periods. If light of frequency n is emitted, the equation of motion of an electron becomes

$$m(n_0^2 - n^2)r = 4\pi aeP$$

and using the relation $P = \Sigma Ner$ to eliminate P, the frequencies of the emitted light are given by the equation

$$\Sigma \frac{4\pi Nae^2}{m(n_0^2 - n^2)} = 1$$

For the value of n near n_0 only the term in the summation which contains $(n^2 - n_0^2)$ in the denominator need be retained as a first approximation, and this value of n is thus the same as that first obtained. It should be noted that now N will probably vary from line to line, and one cannot expect to deduce any general law of variation of displacement with wave length, but other things being equal, the result points to a variation proportional to the cube of the wave length. This law may be expected to hold for lines which have a common origin.

The effect of a change of density upon the positions of the absorption lines may be treated in a somewhat similar manner. If the electrons are set into vibration by the periodic electric force E of frequency n (varying as e^{int}), in an advancing light wave, the typical equation of motion becomes, in the usual way,

$$m(n_0^2 - n^2 + ihn)r = e(E + 4\pi aP)$$

and by the electromagnetic theory of light

$$4\pi P = 4\pi\Sigma Ner = (\mu^2 - 1)E$$

μ being the refractive index of the medium for the frequency n. Putting a $= \frac{1}{3}$ for simplicity, which is very nearly true for a gas, we obtain, by eliminating E and P from the above equations, the relation

$$\mu^2 + 2 \quad - \frac{4}{3} \quad \pi\Sigma \frac{Ne^2}{m}(n_0^2 - n^2 + ihn)^{-1}$$

For values of n near the free period n_0 we may for a gas neglect the effect produced by all the electrons other than those with this free period: since $n_0^2 - n^2$ in the denominator of this term is then small, the imaginary part, ihn, now becomes important, signifying absorption. Therefore, near an absorption band, it follows that

$$\mu^2 - 1 = 4\pi Ne^2\{m(n_0^2 - n^2) + ihn\}^{-1}$$

where n_0^2 is defined by means of

$$n_0^{2} = n_0^2 - 4\pi Ne^2/3m$$

If s is the real refractive index of the gas and k its absorption coefficient, then $\mu = s - ik$, and there results

$$sk = 2\pi Ne^2 hn\{m^2(n_0^2 - n^2)^2 + h^2n^2\}^{-1}$$

The centre of the absorption band, defined as the position of maximum absorption, is evidently give by

$$n = n_0' = n_0 - 2\pi Ne^2/3mn_0$$

(approximately), and so, due to a given change of pressure, the position of the absorption band is shifted by exactly the same amount as the corresponding emission line, in agreement with experimental results.

Moreover, if the width of the absorption band be defined as the distance between the two places where the absorption has a value which is some definite fraction of the maximum absorption, the width is found to be proportional to h, and since the resistance coefficient will increase with the pressure, a symmetrical broadening of the absorption band is to be expected on this account. That the broadening observed is not always symmetrical is due to other and obscure causes which need not be discussed here.

This theory is thus seen to give a very satisfactory and complete explanation of the main experimental results, with

perhaps one exception. The shift has been shown to be proportional to N, which was assumed to be proportional to the density of the vapour in the arc. Experiment shows it to be proportional to the pressure of the surrounding gas. Can these two be assumed proportional to one another? No experiments seem to have been conducted which could decisively settle this point, and in the absence of further evidence it seems legitimate to assume that the proportionality holds until it should be disproved.

It remains now to examine whether the quantitative agreement between theory and experiment is as good as the qualitative. Unfortunately, the comparison is made somewhat uncertain by a lack of definite knowledge of N, the number of electrons per unit volume in the arc, emitting vibrations of a given period. Humphreys' experiments gave, as a result of measurements of a large number of iron lines ranging round $\lambda = 4.10^{-5}$ cms. values of $d\lambda/\lambda$ per atmosphere varying between 2.10^{-6} and 4.10^{-7}. Using the formula

$$\frac{d\lambda}{\lambda} = \frac{Ne^2\lambda^2}{6\pi mc^2}$$

an approximate value of N can be calculated.

Taking $e/mc = 1.77.10^7$, $e = 4.7.10^{-10}$, which are the mean values of the best recent determinations, values of N are obtained which range between $1.7.10^{16}$ and $8.5.10^{16}$.

This is the approximate number of electrons per unit volume which are concerned in the production of a given spectral line for iron. For the other metals tried by Humphreys N is found to have about the same value. Now if the arc were an ideal gas at 2730° absolute temperature and a pressure of one atmosphere, the number of molecules per cubic centimetre would be 4.10^{18}. The conditions in the arc are too uncertain to permit of the estimate of the vapour density, but at first sight it does not appear that the two results are discordant *inter se*. The question is really, however, whether the above estimate of N is a reasonable one. To determine this it is interesting to compare this value with the value determined by other methods. Hallo[1] deduced N for the case of sodium vapour in a flame from measurements of the breadth of an absorption line and of the magnitude of the magnetic rotation of the plane of polarisation, and obtained for the constant $\rho = 4\pi Ne^2/m$ of the dispersion

[1] Diss. Amsterdam, 1902, *Arch. Néerl.* (2), **10**, p. 148, 1905.

formula the value $\rho = 7\cdot65.10^{23}$. The value of the e/mc was deduced from the same experiments to be $2\cdot04.10^7$, and the tolerable agreement with the values found by more direct methods serves as a measure of the degree of accuracy attained. This value of ρ gives $N = 3.10^{14}$ as the number of electrons per unit volume in a flame coloured by sodium vapour.

Another mode of experiment, devised by Macaluso and Corbino, was used by Geiger,[1] depending upon the displacements of interference bands, and values of ρ were obtained varying with the wave length between the limits for

Sodium	.	.	.	.	$1\cdot63.10^{23}$ to $4\cdot83.10^{23}$
Potassium	.	.	.	.	$0\ 82.10^{23}$ to $21\cdot0.10^{23}$
Lithium	.	.	.	.	$5\cdot2.10^{23}$

values which are of the same order of magnitude as the one above. Now Hallo (*Dissert.* p. 92) estimated the number of molecules present per unit volume in his experiments, and concluded that only a small fraction of these molecules are, at any instant, concerned in the emission or absorption of light, and that accordingly the mere presence of a sodium molecule in the flame is not a sufficient condition for its taking part in radiation or absorption : to do so, it must necessarily be in some special state. The exact nature of that state is unknown, but only a small fraction of the molecules are in it at any given instant.

If now this result be accepted it would appear as though the values of N required to account for the observed pressure displacement are possibly rather large; if, on the other hand, these are decreased the calculated value of $d\lambda/\lambda$ will become smaller than the experimental. There is the further difficulty that if this conclusion is true there is no reason why the number of electrons emitting radiation of any given wave length should increase proportionately to the density, as it has above been assumed to do. Too much stress must not, however, be laid on these objections. Hallo's result is by no means conclusive, and the conditions existing in the arc are so different from those in a flame coloured with sodium vapour, and so little is known about the exact nature of these conditions, that it cannot be said with certainty whether the numerical agreement between theory and experiment is good or otherwise, and to draw premature

[1] *Ann. der Phys.* **23**, p. 758, 1907; **24**. p 597, 1907.

conclusions would be very rash. It should be remembered also that too good an agreement cannot be expected when one recollects that some of the assumptions which underlie the theory are somewhat ideal, and are certainly departed from in nature. The atom has been pictured above as a collection of electrons each vibrating about a position of equilibrium, and each, by its vibrations, emitting radiation of a definite frequency and so giving rise to a single spectral line. The limitations of mathematical analysis and our lack of knowledge of the definite arrangement of the electrons inside an atom compel some such simple assumption, which indeed is in a sense justified by the success with which it has explained many of the phenomena of absorption and dispersion. Yet it is much more probable that one has really to deal with the vibrations of groups of electrons, which are jointly responsible by their radiation for the production of a number of spectral lines; the existence of spectral series supports this view. The limitations of the above theory are shown in a marked manner by one of the results obtained; it was proved that with increase of density there is, apart from the displacement, a symmetrical broadening of the absorption lines. In many cases such actually occurs, but in many others there is a marked dissymmetry in the broadening, generally towards the direction of longer wave length. The case of mercury vapour, investigated by R. W. Wood,[1] is a very striking example. Of such abnormal effects, as they may be called, the theory in its present form can give no explanation. Neither can it account for the anomalous behaviour of the enhanced lines. The chief experimental results have, however, been—qualitatively, at least —explained by it in a remarkable manner, and therein lies the justification for the belief that its fundamental assumptions contain the germ of truth. For the present this must suffice; and just as in the development of other branches of physics such ideal conceptions as, for instance, those of a perfect fluid or of a perfectly rigid body have been found most fruitful, so also the present theory may be regarded as throwing some light upon a complicated series of phenomena. A more definite discussion must wait until experimental physicists have obtained a completer knowledge of the structure of the atom.

[1] *Phil. Mag.* August 1909.

A SUGGESTION CONCERNING THE ORIGIN OF RADIOACTIVE MATTER

By H. S. SHELTON, B Sc.

THE suggestion here put forward was written by me several years ago, in 1908, but, finding that it had been anticipated, I made no attempt to publish it. Recent correspondence in scientific journals indicates the probability that others may take it up and expand it. I therefore take this opportunity of stating it explicitly.

The suggestion is, briefly, that radioactive substances, particularly uranium compounds, are synthesised from other elements as a result of the conditions of great temperature and pressure found in the Earth's interior.

The anticipation will be found in Prof. Rutherford's *Radioactive Transformations* (p. 194), where the suggestion is credited to Dr. Barrell. It is mentioned there only in a sentence with no indication whatever of the possible implications of the idea. So far as I am aware, this is the first suggestion of the kind that has been ·made.

The manner in which it arose in my own mind will best be indicated by quoting verbatim from my MS. written in 1908:

" The result of Prof. Joly's investigations discloses a great disparity between the known radioactive content of the crust of the Earth ($3·6 \times 10^{-12}$ parts of radium per unit mass) and the calculated radioactive content of the interior ($4·6 \times 10^{-14}$ parts of radium per unit mass less the necessary allowance for the radioactive content of the crust). In consequence, on the usual supposition concerning the interior of the Earth, we need to assume either an almost entire absence of radioactive matter in the interior, or an interior, with an absence of convective action, heating gradually to some colossal temperature. May not the following suggestion provide a possible solution of the difficulty ?

" Is it not possible that extreme physical conditions, particularly of temperature and pressure, may affect the rate of,

stop or reverse the process of radioactive decay? So far as our present knowledge goes, no change of conditions has any appreciable effect on radioactive decay, but we are inclined to forget the infinitesimal nature of such changes in proportion to the colossal energy equivalent involved in intra-atomic change.

" The great majority of ordinary chemical actions, especially those which occur in nature, are not appreciably affected, and are certainly not reversed, by a few degrees of temperature or by a small change of pressure. The energy equivalent of radioactive decay is, mass for mass, many thousand times greater than that of the most violent chemical action, consequently we are not entitled to infer the irreversibility of this change until we are able to control changes of physical conditions proportionately greater than those we commonly apply to chemical reactions.

" But, in the interior of the Earth, these plutonic conditions actually exist. Mr. Clarence King has calculated that the pressure would probably be measured in millions of atmospheres, and Sir George Darwin has shown that theories of tidal action necessitate the assumption that there is continual addition to the heat stored in the interior. . . . The difficulties are removed if we combine this idea of Dr. Barrell concerning the origin of radioactive matter with the assumption of a very slow convective action in the Earth's interior. It is not then necessary to assume that radioactive substances are confined to the Earth's surface, or that the interior of the Earth possesses any colossal temperature. According to this hypothesis, when conditions of temperature and pressure exceed a certain critical amount, the energy will be stored metachemically, in the form of radioactive compounds. As these find their way very slowly to the surface, energy will be given off again in their slow disintegration." [1]

After the lapse of several years, what is there to add to the passage I have quoted? There is very little of any moment. The problem remains now very much as it did then. Quite recently there had been a discussion in the columns of *Nature*, in which Mr. Arthur Holmes and Dr. Schiller took part, but

[1] Sir George Darwin's paper is published in the *Philosophical Transactions*, Series A, 1879; the passage in question is found on p. 592. For Prof. Joly's statement see his Address to Section C of the British Association, 1908. Mr. Clarence King's paper was published in the *American Journal of Science*, Jan. 1903.

neither of them seemed to have gripped the problem. Mr. Holmes tried to give reasons for thinking that uranium, one of the heaviest known substances, would not be found in the interior of the Earth, but would be concentrated in the outer layer. His argument was based on its distribution in the acid and basic rocks of the Earth's crust. He put forward as speculative the idea that radioactive decay might be inhibited by great heat and pressure. I think there can be no doubt that the first idea is the more speculative of the two. By what conceivable means could radioactive matter be concentrated in the crust to the degree required by theory? On the other hand, if we admit that external conditions can inhibit radiochemical action, why should it not be reversed? Ordinary chemical actions are reversible, given the necessary change of conditions. I do not mean to suggest that uranium would be built up from radium and emanation, merely that it would be synthesised from other elements given the necessary conditions.

On the question of the speculative nature of the suggestion, it must be admitted that, in a sense, speculative it is. We have never been able, by artificial means, to vary the rate of radioactive decay. But, so far as reasoning can be applied to such matters, what inference is simpler? Uranium compounds are continually and slowly decaying at a constant rate. It is, therefore, a temporary element. And a temporary element must have had a beginning. The argument is as sound for uranium as for radium. The time scale only is altered. On the supposition that the rate of decay is a constant quantity, an origin of uranium is a necessary inference. If this is not the origin, what is?

Scientific men are often slow to appreciate the simple and natural inferences from their discoveries. Is not the fact that uranium and radium are elements in every sense but one a clear indication that other elements are not elements in the old-fashioned sense, but that they could be more correctly described, as some one has suggested, as chemical primaries?

The necessity for some such idea can be found in other departments of science. As I have indicated elsewhere,[1] it is

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[1] See *Contemporary Review*, June 1913, "On the Age of the Sun's Heat"; *Knowledge*, Jan. 1910, "A Theory of the Structure of the Solar Photosphere." An article by the Messrs. Jessup, in the *Philosophical Magazine*, January 1908, though not directly dealing with the problem, is of interest in the same connection.

impossible, without some such hypothesis, to correlate geologic time with the duration of solar heat. Other lines of thought lead to the same idea. But it is impossible to deal with remote implications in a brief note. In view of current scientific views, I think it desirable to publish the idea for what it is worth, and to indicate the origin of the first suggestion of the kind I have been able to discover.

THE INFLUENCE OF NUTRITION AND THE INFLUENCE OF EDUCATION IN MENTAL DEVELOPMENT[1]

By F. W. MOTT, M.D, F R.S.

In the last lecture I pointed out to you that the brain consists of innumerable nervous units or neurones and that these nervous units or neurones are collected into groups, systems, and communities having different functions, but that broadly speaking they form three great groups or classes, viz. (1) efferent sensory chains of neurones; (2) efferent motor chains; and (3) association chains of neurones (fig. 1). A neurone consists of a nerve cell and all its branches; one branch forms a nerve fibre which is called the axon because it forms the central axial core of the nerve, the other branches, like those of a tree, are called dendrons. The grey matter of the cortex covering the surface of the brain which is the seat of consciousness consists of countless millions of nerve cells and processes and thus gives it its grey appearance.

Innate Potentiality of the Neurones, and Brain Development

In the child's brain before birth these cells are packed closely together, and at one period they have no processes; as the brain develops and grows these cells, which are termed neuroblasts (neurone-formers), send out processes which, extending and branching like a tree, lead to an increased complexity of structure. This capacity to grow and develop is inherent in all the neuroblasts of the brain, but in order to grow and develop they must be fed by the blood with suitable food. Just as some individuals with abundance of food-supply do not develop and grow because they are unable to take it, or if they do to assimilate it, so it is with the neurones; if there is an inborn failure to take up from the blood and assimilate the food supplied, they will not develop and grow.

[1] Third Chadwick Trust Lecture, continued from SCIENCE PROGRESS, October 1913.

The neurone is a complex cell behaving like a living organism; it nourishes itself and is not nourished. Now the neurones forming the grey matter of the cortex are the most complex and latest developed ontogenetically and phylogenetically, consequently the germinal determinants of these cells are less fixed and stable, therefore more likely to undergo pathological mutations than other cells of the body under the influence of chronic poisoned conditions of the blood of the parents. Whether this be so or not, it is certain that these cells are the latest to mature and become capable of active employment, thus they are more susceptible to arrest of growth, and development by prenatal and postnatal nutritional failure, or by poisoned conditions of the blood. Various forms of failure of development of the brain occur owing to the lack of innate capacity or specific energy of the neurones to grow, and since the brain does not grow the skull-bones also fail to grow, and we have what is termed a microcephalic idiot. It was at one time thought that the brain was prevented from growing by the closure of the bones of the skull, and surgeons attempted to remedy this by removing pieces of the skull so as to allow the brain space to grow; but experience proved that the operation did not cause the brain to grow and the operative treatment of microcephalic idiocy was given up. A little reflection and observation would have shown that the brain is the master tissue and determines the growth of the skull, and the reason why the skull closed early was the natural response to the cessation of the dynamic force of growth of the nervous structures of the brain. I have already alluded to the fact that all the tissues of the body will suffer in order that the brain may grow; in starvation the brain hardly loses any weight. The brain weight of infants dying of exhausting diseases does not seem to suffer, and the experiments of Donaldson at the Wistar Institute (already alluded to in the previous lecture) show that imperfect nutrition does not lead to arrest of growth and development of the brain. An inborn germinal lack of capacity of the neurones forming the anatomical basis of mind to develop and function properly cannot be remedied by improved nutrition of the body, and this is shown by the fact that mental deficiency is found in children of all grades of society; in fact, the majority of cases of feeble-minded children are ineducable because of an inborn physiological deficiency.

THE BLOOD SUPPLY AND ITS QUALITY, IN RELATION TO GROWTH AND FUNCTION OF THE BRAIN

Have nutrition and education then no influence in mental development? Let us first consider the subject of nutrition from a physiological standpoint. The brain in order to grow and function requires a proper supply of oxygenated blood containing the necessary materials out of which the nervous matter can be assimilated and built up. We know that if the secretion of the thyroid gland is lacking owing to congenital absence of the gland, the brain is arrested in its development, and the child is a cretinous idiot. Medical science has shown that if the child receives daily a small quantity of thyroid gland (obtained from sheep), it stimulates the brain cells to grow and probably supplies the blood not only with an excitant to growth but some essential substance for the growth of the brain tissue. The reason why there is such a large blood supply to the grey matter of the brain is that important bio-chemical processes occur there, constituting the physiological basis of mental activity. In all mental operations nervous energy is used up; the neurones are the agents for the storage and liberation of nervous energy; and its liberation is the physiological basis of mental activity, whether it be in simple or complex processes. The neurones automatically store energy when they liberate it, but there is a reserve store for emergencies. Now liberation of nervous energy, that is, conversion of latent neuro-potential into active neuro-potential involves oxidation; consequently oxygen is essential for the process. This is shown by the fact that unconsciousness results if the cortex of the brain is deprived of arterial blood for a few seconds.

We are conscious of the external world and our own personality and existence by continuous stimuli arising from the external world and from our own body. If those stimuli were cut off, we should lose consciousness, notwithstanding that the blood supply to the cortex of the brain continues. The neurones of the cortex of the brain, besides innate potentiality to function, require also the stimulus from the external world together with a proper supply of oxygenated blood; and this implies a sufficient number of red blood corpuscles provided with an adequate quantity of the red

colouring matter—hæmoglobin. Not only may an impoverished blood deficient in red blood corpuscles and other essential constituents be the cause of a mental functional deficiency by depriving the nervous elements of their capacity to grow, develop, store, and liberate energy, but a poisoned condition of the blood is a far more frequent cause of acquired failure of mental energy in infants and children as well as in adults. Such impoverished and poisoned conditions of the blood in infancy arise in a large majority of cases from gastro-intestinal disturbances owing to improper feeding, and may, if continuous, interfere with bodily nutrition and brain development. The intelligent mother accepts such warnings as fits of screaming, restless sleep, crying without obvious cause, refusal of food, and convulsions ; she does not think the infant exhibits these symptoms from temper, but as an evidence of suffering requiring maternal sympathy and protection, and she seeks the cause in order to remove it. Now, imperfect nutrition and poisoned conditions of the blood brought about by fermentation and putrefaction in the gastro-intestinal canal from improper feeding, and from acquired or inherited disease, may not actually arrest the growth of the neurones of the brain any more than they very materially interfere with the growth of the child, and cause arrest of development; but such unfavourable conditions of nutrition at the time when the brain is undergoing its most active development cannot but be harmful. I told you in my first lecture that during the first three years after birth the greatest increase in the weight of the brain occurred, and that at three years old it had trebled its weight at birth. Even if with an unfavourable bodily nutrition of the infant the brain grows and develops to nearly treble its weight at the end of three years, we cannot therefore assume that it has in no way suffered from mal-nutrition, any more than we can assume that because a child has grown in stature a few inches less than a well-nourished child, it has not seriously suffered. You naturally ask : How then has the brain suffered? It has suffered constitutionally, as the child has suffered constitutionally ; it is less able to resist the effects of stress from any cause ; it is more liable to exhibit signs of nervous irritability, convulsions of teething, and if the child is infected by the micro-organisms of pneumonia, tubercle, whooping-cough, measles, or scarlet fever, the brain as well as other

parts of the body has less vital resistance to the poisons produced by the organisms.

INFANT FEEDING

Children often suffer from over-feeding and from being given unsuitable food that sets up gastro-intestinal irritation, vomiting, and diarrhœa with various manifestations of nervous irritability due to absorption of bacterial poisons by the blood. The greatest preventable cause of infant mortality and constitutional weakness of the child after birth is improper and insufficient feeding. Other preventable causes of infantile mortality are congenital syphilis and tubercular meningitis. Collective responsibility should not be undertaken to replace parental responsibility, but to educate and assist it; and this is the method adopted by health visitors. This system of educating the mothers is beginning in a right way by giving every infant a better chance for growth of body and mind. Collectivism and individualism should work together by improving the mother's health and instructing her how to nourish her offspring. Now, there can be no doubt that the natural food for the infant up to the time that it has teeth is the mother's milk, which is the only perfect food for the baby during the first nine months of its life, and only under exceptional circumstances is it justifiable to employ artificial feeding, in the interests not only of the infant but of the mother also. For not only has nature provided the milk glands, but also an internal secretion by the cells which occupy the position in the ovary whence the ovum that developed into the child came, and this internal secretion has the special function of stimulating the secretion of milk. Prof. Karl Pearson in his second Chadwick Lecture showed the fallacy of statistics in regard to infant mortality and various modes of feeding in town populations. His argument was that the statistics showed that infant mortality only corresponded with the health and habits of the parents; it did not seem to matter whether the child was breast fed or artificially fed, nor did it seem to point to one form of artificial feeding being superior to another. Are we therefore to conclude that it does not really matter whether a child receives the nourishment nature itself provides or not? No! The reason why artificial feeding of .infants appeared in statistics to be good, or better than breast-

feeding, is that poor and destitute women unable to purchase milk are very numerous. They themselves have large families which, owing to their own nutritional failure, they are not able to rear.

Stimulus in Relation to Development of the Brain

There are two other factors to consider beside innate potentiality of the neurones and their supply of the necessary materials for growth by a pure and adequate blood supply. They are the stimulus to growth by the physical and chemical excitation of the nerve endings in the sense organs and bodily structures. Let us consider this a little more fully. The infant learns to know its own existence and the desires necessary for its life by its organic sensibility; the nerve endings in the skin, muscle, tendons, and joints carry messages continually to its brain, inciting the desire to breathe, to take nourishment, and to perform the calls of nature. The special sense organs associated with the muscle sense—which contributes to every other sense—are especially represented in the grey matter covering the brain; they are the avenues of intelligence and by motor reaction and adaptation the source of information concerning the external world. As I pointed out in my first lecture, preparedness for function by myelination is first shown in the structures of the cortex which serve as the arrival platform of sensations of organic and bodily sensibility, of smell and of taste; then of vision, and lastly of hearing; these, combined with the kinæsthetic sense, constitute the primary perceptive centres. A simple experiment shows that the chains of neurones which constitute the peripheral receptor (sense organ), the transmitter, and the central perceptor have the power of transforming cosmic energy into neural energy. The experiment is this : if you take a pair of fine electrodes connected with an electrical apparatus discharging an interrupted electrical current, and place them on the tongue, a sensation of taste is produced; if on the skin a vibratile sensation is felt; if the eyeball is excited a bright light is seen; and if the nerve of hearing is stimulated a noise is heard. Since the stimulus does not vary in any one of these experiments it necessarily follows that each sensory nervous mechanism has the power of transforming the stimulus and producing a specific effect on consciousness. The neurones then not only act as receptors,

but transformers of energy, and they use up oxygen in the vital functions associated with this specific transformation. Moreover, traces of the specific effects are left in the perceptor cortical neurones constituting memory. Now what will be the effect on the growth of the neurones forming the central perceptor for vision, if the child is born blind, and all light stimulus is thereby cut off from the brain? Experiment has answered this question. A microscopic examination of the visual area of the brain of a puppy whose eyes were removed at birth was compared with a normal puppy, and the accompanying figures show that the cells of the grey matter of the blind dog were small and shrunken as compared with the cells of the grey matter of the normal dog. Stimulus, therefore, is necessary for development and growth of the neurone.

Helen Keller and Laura Bridgeman in Relation to the Tactile-Motor Sense

You may ask how it was that Laura Bridgeman and Helen Keller, both blind and deaf in early life, were able to develop such a high degree of intelligence when the two principal avenues of intelligence were cut off in early life. My answer is this: Look at this diagram of the child's brain at three months and you see every part of the grey matter of the cortex is connected by fibres capable of functioning; all the elementary perceptor centres of the special senses are connected by association fibres with the kinæsthetic sense area and the motor efferent area. The child at three months is no longer capable of an elemental sensation; the visual and tactile-motor senses have become associated; the child has learnt to handle things seen and to memorise the meaning of things seen, as regards other qualities than form and colour. Now both Laura Bridgeman and Helen Keller were not affected with blindness and deafness till such a time after birth had elapsed for a very complete development of the association systems. Sensory stimuli had poured in through all the sensory avenues for twenty-six months in the case of Laura Bridgeman and for nineteen months in the case of Helen Keller; consequently we should not expect those regions of the brain which had served for seeing and hearing —which had been shut off by damage to the transmitter—to undergo atrophy and arrest of development the same, as if no stimulus of light or sound had ever affected them. It may be

asked, How could these areas of the brain be utilised when cut off from the external world by interruption to the transmitter? The kinæsthetic sense (or sense of movement) is the sense which contributes to every other sense; it is especially associated with vision and touch, but also with hearing in the movements of the lips and tongue in the production of articulate sounds. Now this kinæsthetic sense and the tactile sense were not interrupted in the cases of Laura Bridgeman and Helen Keller. The innate potentialities of the brains of these two remarkable beings must have been of the best, and the greatest credit is due to that pioneer Dr. Gridley Howe for finding his way to Laura Bridgeman's intelligence through her finger tips. His plan was to teach her by raised types and then by the manual alphabet.

One of the most interesting psychological studies that I know of is *The Story of My Life*, by Helen Keller. She was evidently a precocious child, for at six months she could utter articulate sounds; even three months after the illness which made her blind and deaf she uttered the word "water." She walked at one year, and as she says, "During the first nineteen months of my life I had caught glimpses of broad green fields which the darkness that followed could not utterly blot out." In the first months after her illness she says: "My hands felt every object and observed every motion and in this way I learned to know many things," and she indicated her wants by gesture language encouraged by her mother. She lived a normal life on a farm *sans* sight and hearing, but was wonderfully intelligent and exercised reason in her actions. She was always happy when she could keep her mind and fingers busy. Systematic teaching by Ann Mansfield Sullivan was commenced when she was seven, the system being the association of tactile-motor verbal symbols made with the finger in the palm of the hand with the tactile-motor impression of objects. Everything had a name and each name gave birth to a new thought. She remarks: "At the first I was only a little mass of possibilities; it was my teacher who unfolded and developed them." At the age of ten she learned to speak. She was taught by a Miss Fuller, and the method, in Helen Keller's own words, was this: "She passed my hand lightly over her face and let me feel the position of the tongue and lips when she made a sound." In reading her teacher's speech she was dependent on her fingers, she placed

her hand on her teacher's throat, mouth, and face, and read the vibrations and movements of the mouth and expressions of the face; the same movements she learned to reproduce and thus learned articulate speech. The sense of movement combined with touch and smell were in her case the sole avenues of stimulus to the brain from the external world, but inasmuch as all the primary sensory areas including hearing and vision are connected with these areas by association channels, the whole brain responded to the stimulus and developed to the full its innate educable possibilities.

SLEEP AND MENTAL DEVELOPMENT

We now come to the last factor requisite for proper development of the brain and especially its efficient function—sleep—that sweet unconscious quiet of the mind which permits all the vital bodily functions to continue (although less actively) while the cortex of the brain rests and the whole organ stores energy and recuperates. Sleeplessness is a sign of nervous irritability and is cause as well as effect of mental fatigue and nervous exhaustion. Darkness, stillness of the body, and silence favour sleep by removing the principal causes of wakefulness and activity of the mind. Habit fortunately permits of sleep under the most unfavourable conditions; still, the sleep of young children must necessarily be a broken one in the single-room tenement dwellings of the poor of our large cities. This is an important unhygienic condition relating to mental development; for insufficiency of rest to the brain tends to failure of mental energy. The growing infant requires plenty of sleep; so also does the growing child, and especially is it so when the child is suffering from bodily ill-health or nervous irritability. When I was in Chicago recently I observed that all the children in the Special School for Tuberculosis were made to lie in bed for an hour in the afternoon.

The question of nutrition in relation to mental development, ability, and efficiency is one that until quite recently was not properly considered by the authorities; for until the mother's health and her mode of feeding her offspring became a part of social reform, the most important step in relation to nutrition and mental development was left out. Statistics of Willesden and Chester (which I throw on the screen) show that not many children in these localities were suffering from imperfect nutri-

tion when medically inspected. The minor ailments were the chief cause of trouble, viz. defective teeth, adenoids, large tonsils, defective vision, and especially parasitic head affections. Eye strain from errors of refraction may lead to nervous affections in children with a neurotic or neuropathic temperament; adenoids and large tonsils are a very frequent cause of deafness and consequent mental dullness.

The extension of the meaning of education by collective responsibility to the bodily welfare of the child from birth onwards is one of the greatest steps made towards increasing the educability of the child when it arrives in the school. We have seen that the factors underlying educability are first and foremost the germinal inborn potentialities derived from progenitors (Nature); secondly, those conditions of nurture which are favourable to the morphological development of inborn potentialities, viz. bodily nutrition, sleep, and stimulus.

The Influence of Education on the Development of the Mind

The teacher is powerless to develop intelligence where there is an absence of the material basis of mind, or an inherent low functional value and ready fatiguability; so that sustained attention, necessary for the acquirement of knowledge, fails. The former condition is quite hopeless, the latter may not be due to inborn defects, but to bad nurture; therefore preventable and, in a measure, curable.

The object of education should be to establish physical, intellectual, and moral efficiency in the child by drawing out and developing the good inborn qualities, by installing and fixing good habits, and by repressing, controlling, and preventing as far as possible the acquirement of bad habits. In the acquirement of good or bad habits early in life when the mind is most susceptible, imitation and suggestion play a most important part; thus an inborn virtue such as an amiable and confiding disposition may under the influence of bad companionship lead to the ready acquirement of vicious habits. The teacher has only a partial influence in forming character and education for efficiency. Home influence, good as well as bad, companions in school and out of school, chance and opportunity, all play their part in the general making of success or failure in the final product of education. Home influence is the most important

factor in efficiency, especially in the formation of character; the individual efforts of good parents, especially of good mothers, cannot be replaced by the collective efforts of society in schools and institutions. Yet much may be done by health visitors and domiciliary visits of school nurses in improving the home conditions of the child, and thus helping the teachers in their work of education. When the home conditions are impossible for the child, the relief of the parents of responsibility for its care has been attended with marked success; so also the poor material furnished by waifs, strays, and orphans formerly dragged up in the workhouse has been made into more or less efficient material in the industrial schools and Barnardo's homes. Social reform has thus made great progress in the interest of the child by the extension of the meaning of education. I have been much impressed by the growing interest teachers take in their pupils; especially have I had the opportunity of observing this in the teachers at special schools. They know of the home life of their pupils, and show interest in understanding the cause of the physical and mental defect from which the child suffers. It seemed to me almost pathetic that teachers with such intelligence, human sympathy, and untiring energy in their work should be entrusted with the almost hopeless task of trying to draw out from mental defectives initiation or efficiency. It is otherwise in the special schools for tuberculosis, deaf and dumb, and blind children; here there is educable material which will in future make for efficient service. The open-air schools for the treatment of tuberculosis which I visited at Birmingham made me exclaim: "Why, these children look healthier than the normal! Why not have all the children taught in open-air schools?"

The special schools for the deaf and the blind yield gratifying results to the teachers, because in the majority of cases the children are not mind-blind or mind-deaf; they are educable because the material basis of mind in the brain is there, and the teacher finds her way to the mind of the blind through the finger-tips and to the mind of the deaf through sense of sight. The deaf child, by watching the movements of the lips, is able to speak by imitating the movements. Do not these facts show the great importance of training the tactile-motor sense and the sense of movement (kinæsthetic sense) in our normal schools?

The kinæsthetic sense is one of the most important which can be cultivated; it is the essence of the *joie de vivre* in play,

FIG. 1.—Diagram to illustrate afferent kinæsthetic system conveying impulses from tactile corpuscles, from muscle and tendon by way of the sensory nerves to the spinal cord, and thence to the cerebrum and cerebellum.

The efferent motor projection systems from the cortex cerebri and the cerebellum are shown terminating at the spinal motor efferent neurones, which transmit impulses by the motor nerves to the muscles. The numerous pyramidal cells of the cortex represent the association system of neurones which link up all the perceptor centres with the sensori-motor region.

which is instinctive in children and animals; and not only do the feelings aroused in connection with it give pleasure, but they are stimulating to growth of body and mind. Every movement of the limbs leads to ingoing currents of nervous energy (*vide* fig. 1). Bodily fatigue from exercise arises more from accumulation of fatigue products (that is, chemical substances) in the muscles than from exhaustion of the nervous structures; indeed, the nerves as conductors do not get fatigued.

THE EVOLUTION OF ASSOCIATION OF THE EYE AND THE HAND

A study of the association of the eye and the hand is of great interest in showing the reciprocal simultaneity in the development of the visual directive and the tactile-motor executive faculties. In the animal series it is not till we reach the primates (apes, anthropoid apes, and man) that we find dissociation of the fore limbs from progression; the nose is lifted from the ground and the sense of smell and capture of food by the mouth gives place to capture of food by the hand guided by vision. The primates are microsmatic, that is to say the olfactory nerves and the structures of the brain subserving the sense of smell are relatively poorly developed, but the structures of the brain which serve the function of vision, hearing, and touch are largely developed. It is not till we reach the primates in the animal series that the eyes are set with their visual axes parallel and that therefore these axes are capable of convergence; consequently by accommodation the image is always made to fall on the yellow spot. Moreover, it is not till we reach the primates that a yellow spot is found to exist. The panoramic vision of the macrosmatic quadrupeds is replaced in the primates by binocular stereoscopic vision. But with the development of binocular stereoscopic vision, there has simultaneously developed the sterognostic sense, or the sense arising by the association of the experiences of the visual directive and tactile-motor executive faculties, by which the mind can recall the visual image of an object handled or touched. Every object seen is associated with the experiences of touching and handling it, and makes us conscious of its realities of form, of smoothness, of roughness, of hardness. A little reflection will show how great a part this association of the eye and the hand has played in the progressive evolution of the brain as an organ of mind. Now some people have the power of visualising, that is, summoning to the

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mind's eye images to a remarkable degree, and all possess it to some degree. Yet as Galton truly remarks : " Our bookish and wordy education tends to repress this valuable gift of nature. A faculty that is of importance in all technical and artistic occupations, that gives accuracy to our perceptions and justness to our generalisations, is starved by lazy disuse, instead of being cultivated judiciously in such a way as will on the whole produce the best return. I believe that the serious study of the best method of developing and utilising the faculty without prejudice to the practice of abstract thought in symbols is one of the many pressing desiderata in the yet unformed science of education." This appeal of Galton emphasises the importance of educating the association of the eye and the hand.

The child has imagination, and it loves to picture in its mind's eye visions of the beautiful. What greater proof can we have of this than the universal popularity of Hans Andersen's fairy tales, *Alice in Wonderland*, and *Peter Pan*. The child is naturally idealistic and romantic, and its character can be studied best in its ideals and play, because there is no repression. Now it is well to train a child to give expression to its ideas and ideals, not only by words, but by acts, especially by the hand, the instrument of the mind, and yet the mind's instructor.

In this country Mr. Cooke has been a pioneer in teaching free-hand drawing by children on proper lines ; and those who are interested in this important branch of education should read *New Methods in Education*, by J. Liberty Tadd of the Adirondack Schools.

THE ORDER OF DEVELOPMENT OF THE PHYSIOLOGICAL FUNCTIONS OF THE BRAIN IN RELATION TO EDUCATION

It will be observed from what I have said in my two previous lectures—in which I dealt with the morphology of the brain and its development—that the earliest parts of the cerebral cortex to exhibit functional capacity are those areas which serve as the receptors of the organic and general body sensibility and the special senses. A very little time after birth the motor area is myelinated, and therefore prepared to react in response to sensory stimuli, whether coming from the body itself in the form of organic needs or from without in response to stimuli from the external world. The former are fundamental to the preservation of the individual, for upon the organic needs are

based the desires which excite the brain, through the senses, to explore the external world in order to gratify them. This is well exemplified by observing that an infant at first conveys all objects, that it sees and grasps, to its mouth. A simple elemental sensation soon after birth becomes impossible; for every simple sensation tends to reflex activation, and each phase in that motor reaction which occurs is immediately followed by incoming sensory stimuli registering in the mind the successive movements brought about (*vide* fig. 1). At the same time each experience perfects the association of the sense of movement with the mental image of the sensation; thus the memory of the visual image is associated with the memory of the movements of the eyes necessary for it to be clearly seen; likewise the memory of the visual image of an object is associated with the memory of the movements of the arm and hand by which it was grasped. Thus it may be truly said that the muscular sense contributes to every other sense, and all the sensory areas of vision, hearing, smell, taste, and touch become linked up by the bonds of associative memory with the muscular sense. Now the muscular sense is combined with the active sense of touch; but it is better to speak of it as the kinæsthetic sense, for this includes the sensation arising from the stretching of tendons, the movements of joints, as well as of movements of the muscles. All the sensory receptor spheres of the brain are associated with the voluntary efferent motor sphere (*vide* fig. 3), and every sensation in the infant tends to activation, that is motor expression; for it is by handling and touching parts of its own body that it becomes aware of its own personality, and by motor reaction to sensory stimulus it learns the reality of things in the world external to it; consequently with the progressive evolution of the child's mind there is constant sensori-motor association. Not only is there association of each sensory sphere with the motor and kinæsthetic spheres, but there is also an association of all the sensory spheres with one another; so that a simple sensory stimulus from within or without the body revives in the memory a complexus of previous sensory experiences which are termed "percepts." The perceptive faculty of associative memory of concrete images of previous experiences with elemental time and space relations and the acquisition of appropriate motor reactions under the influence of the will, is also possessed by

all the higher animals; and while the infant is crawling on all fours like an animal, it possesses only these animal faculties of mind. As the child obtains the erect posture and the fore limbs are dissociated from progression, it begins to acquire the human faculties of forming concepts and of giving expression to them by speech, the primary incitation of which is hearing; and later writing, reading, and measurements of time and space, in which vision plays a dominant part, are acquired. As these human faculties are evolved, so the processes of abstract thought and reasoning by associative memory of symbols—particularly in a cultured and civilised environment—gradually tend to replace in the child associative memory of concrete images. I have already alluded to the importance of freedom from restraint to the child's natural instincts of curiosity and play, and Mr. Edmond Holmes, in *What Is and What Might Be; A Study of Education in General, and Elementary Education in Particular*, says: "There is nothing that a healthy child hates so much as to have the use of his natural faculties and the play of his natural energies unduly restricted by pedagogic and parental control." We should indeed recognise that one of the child's greatest assets is its childishness. It should be interested in its lessons because it enjoys them, and not to win prizes and rewards, which in a number of instances only indicates an ability to receive, retain, and retail information. Knowledge in later life will be its own reward, ignorance its own punishment.

Holmes asks: "Does elementary education, as at present conducted in this country, tend to foster the growth of the child's faculties?" According to Holmes the answer is an emphatic No! "For in the school, as I have sketched it, the one aim and end of the teacher is to prevent the child doing anything whatever for himself, and where independence is prohibited the growth of every faculty must needs be arrested, the growth of every faculty as of every limb and organ being duly and suitably exercised by its owner."

From what I have previously said it will be observed that perception and expression are interdependent, and an educational policy or system which does not make self-expression, in other words sincere expression, its aim, is necessarily fatal to the normal psycho-physiological development of the mental faculties.

The kindergarten system introduced by Froebel, and lately modified and developed by Dr. Marie Montessori, is based upon

Fɪɢ 2 — Two groups of cells one from the occipital cortex of a normal dog, the other, pale undeveloped cells, from the dog with the eyes removed at birth (After Berger)
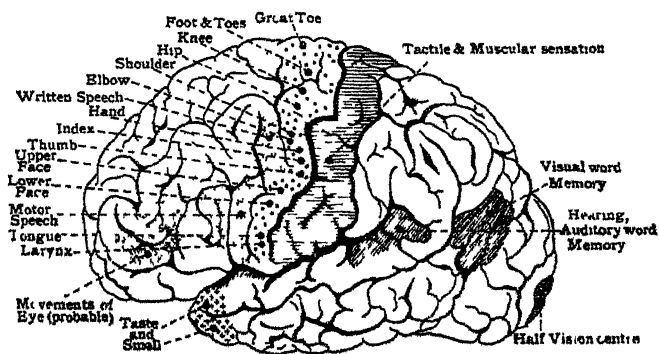
Fɪɢ 3 — A lateral view of the left hemisphere, showing the localisation of the various motor and sensory spheres and speech centres

The greater part of the visual centre is on the mesial surface of the hemisphere and is not shown like-wise the greater part of the auditory centre lies in the floor of the Sylvian fissure and is covered up and not shown

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sound psycho-physiological principles, such as I have outlined in the development of the structure and function of the brain. The Board of Education in England, recognising the importance of this work, issued, in October 1912, a special report on the subject by Mr. Holmes; it is probable that the study by Mr. Holmes of this system, the fundamental object of which is self-education by the pupils themselves, a system in which there is neither reward nor punishment of the ordinary kind, and in which there are no time tables, no set lessons, and no classes, led Mr. Holmes to write the book above mentioned— *What Is and What Might Be.*

The first stage in the Montessori system, as would be expected from what I have just said, is the development of the senses, mainly touch, then sight and hearing; this is accomplished by various sorts of games and by drawing the attention of the child to the association of things, names, and ideas. Such operations are preliminary to writing and reading, but naturally lead up to both.

As Mr. Holmes says, the first impulse of the ordinary teacher is to tell a child how to do a thing which it has never attempted before; the second is to rush to the child's aid, who having been allowed to try his hand at something new, is confronted by a difficulty and is in doubt as to his next step; the third is to correct his mistakes for him, instead of leaving him to correct them himself. Dr. Montessori in Mr. Holmes's words has " rediscovered " Froebel's master principle of "auto-education"; the teacher is the *director* of the spontaneous work of the child, " she is a *passive* force, a *silent* presence." Dr. Montessori employs an extensive variety of apparatus suitable for educational games by which the children are interested and stimulated to acquire knowledge, and her educational system is an original and practical expression of sound psychological principles; these principles are based upon the anatomical and physiological order of development of structure and of function of the organ of mind.

Little has previously been said in respect to the sense of smell and taste, but the cultivation of these senses is of more use than many people imagine; for they are a daily source of keen gratification; they frequently serve to revive pleasant associations and they are the best natural protector against unsound food, unwholesome drink, and vitiated air. It is a remarkable fact that most mineral and vegetable substances that are poisonous are acrid, unpleasant, pungent, or bitter, and readily excite

nausea, disgust, and rejection from the mouth when tasted, likewise all foul and many poisonous odours excite nausea, disgust, and aversion; whereas pleasant tasting and smelling substances found in nature are usually wholesome and nutritious.

PAIN AND PLEASURE IN RELATION TO MENTAL DEVELOPMENT

The associative memory of painful and pleasurable feelings plays an important part in mental development. The sense of well-being and pleasurable feeling is a vague state of consciousness clothed and enriched by perceptual and intellectual associated memories which we *desire* to experience again, and they form an accompaniment of the healthy activity of the functions of body and mind when not exceeding the ordinary normal powers of reparation that the organism possesses. The preservation of the individual and the species depends not only upon the gratification of the desires, but also upon the protection of the body from physical and chemical injury by pain; moreover, the senses of smell and taste are sentinels to the alimentary and respiratory systems, protecting them from injury, by exciting nausea and disgust or reflex acts such as coughing, sneezing, and vomiting. These are states of consciousness which there is *no desire* to experience again, and when associated with perceptual and intellectual memories their causes can be avoided.

There is evidence to show that if pain is felt in the optic thalamus the perceptual concomitants with which it is associated are registered in the cortex cerebri; for the optic thalamus is connected with every part of the cerebral cortex, the seat of associative memory and recollection. The cortex is not the perceiver of pain but the perceiver of the causes which produced it and by which it may be avoided. The cortex can be cut and stimulated without producing pain; not so the optic thalamus. If by associative memory of the conditions and instrument which cause pain, revival of pain occurred, what would our state of mind be normally? Pain is the great protector of the body from injury. One of the trite sayings of Oliver Wendell Holmes was "That clergymen and persons without wisdom consider pain a mystery; it is a revelation!" We can understand therefore the great biological significance of pain in evolution. Richet indeed is right in asserting that instead of considering pain as an evil we ought to consider it fundamental to human progress, for as instinct is blind, intelli-

gence is necessary to avoid pain which by associative memory it foresees and prevents in innumerable ways, whether arising from direct bodily injury or a craving due to the non-gratification of the organic needs of the body, *e.g.* hunger, thirst, the desire for fresh air, for sleep, for exercise, for recuperation and repose after muscular or mental fatigue and for the satisfaction of the sexual appetite. It is not too much to say that the affective life or subjective feeling of the child as well as of the adult depends largely upon the organic sensibility (cænæsthesia), the source and foundation of all stable perceptual associations and of the vast majority of habitual actions. It is necessary to remark that the subjective attitude of the individual determines the severity of pain felt, as much as the intensity of the stimulus. We know how an irritable state of the nervous system enhances pain, whether it be due to inflammatory conditions of the peripheral nervous structures, of the chains of neurones forming the transmitter to the seat of consciousness, or of the central receptor which in certain abnormal mental states (*e.g.* neurasthenia and hysteria) may evince hyperæsthesia or anæsthesia.

The Control of the Emotions and Inculcation of Good Habits

In the formation of character no problem in education is more important than the acquirement of self-esteem, self-reliance, and self-control; but this education of self, to be effective in the struggle for existence in our social organism, must be tempered by sympathy and unselfishness to others for the essence of social evolution and progress is altruistic egoism. It is never too early to begin to inculcate in a child the habit of self-control; thus it should be taught to acquire the habit of control of the primitive emotions of anger, of fear, and of disgust in infancy, and to limit or repress their motor reactions; but their repression or suppression should in great measure be determined by the nature and intensity of the cause of the emotional disturbance. Crying and screaming of an infant is a protective appeal to the mother for relief of pain or the satisfaction of a natural desire or organic need, but this natural expression of a physiological necessity may become the expression of a bad temper; thus a child, who learns that it can get its own way in obtaining something it desires against its parents' wishes, very soon contracts the bad habit of falling

into a passion whenever it is thwarted. The indulgent mother to stop the fits of crying, screaming, and outbursts of angry temper too often yields to the child's will, and gradually but surely a weakening in the development of self-control occurs, which has a profound influence upon the development of character; especially is this the case in a child with an inborn unstable temperament. The influence of education on self-control is well illustrated in the lines of *Childe Harold* where Byron doubtless refers to his own bringing up:

> I have thought
> Too long and darkly till my brain became,
> In its own eddy boiling, and o'er-wrought,
> A whirling gulf of fantasy and flame.
> And *thus untaught in youth to tame*,
> My springs of life were poisoned.

The emotion of fear is protective; the instinctive reaction is either flight or concealment; naturally therefore darkness is associated with this emotion, and it is not surprising that children and savages should have an inborn tendency to fear the dark. Seeing that there is this natural tendency of children to fear darkness, some discretion is required in overcoming the dread of a naturally timid child to sleep in the dark, and harm may be done by too rigidly applying the principle of forcing it to go to sleep without a light, especially if it has become accustomed to one in its infancy. The habit should be gradually broken, if it has been contracted. Much injury is done to young children by ignorant nurses and servants by frightening them with stories of ghosts and bogeys. Indeed, the tempers and morals of many children have been ruined by mothers leaving the care of their children to ignorant and vicious nursemaids.

Another bad habit which may be contracted by the child in early life is an unnatural desire for sympathy; too often an only child of indulgent parents, sometimes under the cloak of the possession of a fondly supposed æsthetic or artistic temperament is allowed to contract the habit of unreasonably soliciting sympathy whenever opportunity offers; and the penalty in later life is paid by the unnatural development of the self-regarding sentiment, a precursor so frequently of functional, nervous, and mental disorders.

While it is highly desirable to train children to exercise control over the primitive emotions, it is essential that they

should not be so suppressed as to injure the natural spontaneousness of the child. The natural expression of the emotions is motor reaction, and when emotions or passions are pent up by voluntary restraint they are apt to lead to exhaustion of mind and body.

The suppression of the manifestation of tears and anger from fear of punishment, especially if the punishment does not fit the crime, may produce a sulky habit in the child; and this pent-up anger and fear may in later life tend to the formation of a character in which hatred and revenge find a suitable soil for development. By suppressing the manifestation of an emotion or passion it becomes continuous and contemplative. For as Shakespeare says:

> Give sorrow words: the grief that does not speak
> Whispers the o'er-fraught heart and bids it break.

A child in earliest infancy manifests by characteristic expression the emotion of disgust; this emotion and its instinctive rejection of bitter, acrid, and nauseous substances by spitting out and vomiting is protective in the highest degree; thus it is natural for a child to show signs of disgust and anger when nasty medicines or unpalatable food are given to it. But a child may acquire a habit of screaming and rejecting with tears and signs of anger wholesome food when it sees other food intended for adults. Here the child owing to the initiation of a bad habit is behaving contrary to the instinct of preservation, and the only course to adopt is to give it no food until its natural food is accepted. Too often, however, an indulgent or ignorant parent yields to the child, and very soon a bad habit is firmly installed, which may later be a determining cause of bodily ailments and weakened self-control.

Children are, like many animals, naturally curious, and this instinct of curiosity is closely associated with the emotions of surprise and wonder. Curiosity in children manifests itself by inquisitiveness regarding the natural phenomena they observe and their causation; too often this instinct in which science has its roots is repressed by " don't ask questions," or some foolish commonplace answer is given to their inquiry, which upon reflection the child knows to be untrue. Every child is a natural philosopher, and all natural phenomena, the result of perception, that the child is fit and capable of understanding,

should be explained, or the child should be told truthfully, "I can't explain the fact." It is, however, in my opinion a mistake to lead the young child too far into experiences which an adult alone can understand and appreciate in their full biological significance.

SEX AND EDUCATION

With the dawn of the sexual passion at puberty, a new and intense emotional phase of existence occurs, which even when it is mature and developed, may not be shown in daily conversation, yet as a deep and silent undercurrent of consciousness and silent thought is continually influencing character and behaviour. Now and again, it reveals itself by springing to the surface and bursting its bonds in a flood of passion; still there are many people who can and do go through life without manifesting to the external world the profound influence which the sexual passion has on their behaviour.

But "still waters run deep," and in the majority of people this silent undercurrent of emotion, although not manifested to the external world, nevertheless occupies a large place in the conscious and subconscious self; it suffuses silent thought and consciousness with an emotional tone, which may find outward expression in æsthetic and religious forms and observances. It is a more important factor than any other in the formation of character, for it must be conceded that human motives and conduct originate in great measure from the depths of the passion engendered by the natural attraction of the sexes; but inasmuch as the bodily characters that distinguish the sexes are different, so are the mental characters. Although each sex is represented in all the cells of the body, the sexual organs peculiar to each sex make dominant by their internal secretion the male or female secondary sexual bodily and mental characters. Observation and experiment show that the opposite sexual character is present in the somatic cells, but it is latent or recessive.

It is an important fact to bear in mind in the education of the two sexes, that there is as radical a biological difference in the mind of the woman to that of the man as there is bodily difference, and this different mental attitude peculiar to sex shows itself especially in the contrast of emotional feelings and their manifestations; moreover a woman is different intellectually; she has quicker perception and association of ideas, she deliber-

ates less and arrives intuitively at a judgment quicker than a man. She has, however, less mental energy and power of will than a man. Being constitutionally different from a man, a woman's physical and mental education, in order to bring out her noblest and best qualities, should not be identical with that of a man. I may here remark that co-education of the two sexes in adult life has not proved a great success. Just as a woman prefers a manly man and despises an effeminate man, so a man is attracted to a womanly woman and is repelled by a mannish woman; this is the natural consequence of sexual attraction and should be duly borne in mind in the education of girls; the feminine charms and graces should not be sacrificed lightly by copying slavishly man's physical and mental development. Still it is an acknowledged fact that social conditions prevent a very large proportion of marriageable women from fulfilling the natural functions of motherhood, and they have therefore only to consider their own individual life and its preservation. Education and intellectual development of women to enable them to earn their own living and thus become efficient social units, will not make them any less capable of becoming good mothers, provided there is in their training ample scope for natural physiological development, and the normality of the reproductive organs is not interfered with by too strenuous mental or physical exercise. It is necessary to give a word of warning against girls being pressed at schools by night work and competitive examinations, just at the time when the reproductive organs are commencing to function and exercise a profound influence on the mind. Nor do I regard it as wise to overdo sports and games at a period of life when important physiological processes connected with the storage of energy and nutrition are called for by Nature in the preparation of its supreme effort of reproduction. Over-pressure at schools and competitive examinations at puberty and early adolescence is often due to the ambition of parents, but it not infrequently leads to a nervous or mental breakdown, especially if the child has an inborn neuropathic tendency.

We have now seen that a healthy mind can only exist in a healthy body; and it is becoming widely recognised that the essential feature of education should be to develop the inborn physical and mental qualities that make for efficiency and thus to prolong the period of individual productiveness and civic worth.

ENZYMES AS SYNTHETIC AGENTS

II. IN PROTEIN METABOLISM [1]

By J. H. PRIESTLEY, B.Sc., F.L.S.

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Much of the work upon the synthesis of carbohydrates has been done with a view to solving the questions of constructive metabolism in the plant, but in the study of protein metabolism attention has been chiefly directed to the problems presented in the animal organism.

In the present paper, which is a survey of recent work, the point of view taken will be the bearing that some of this work upon proteins may possibly have upon constructive metabolism in the plant. Considered from this standpoint, the facts, so far as they are known to the present writer, may perhaps be summarised without presenting too familiar an aspect.

The problem of presentation is simplified in some respects by the fact that, at the present time, questions of molecular symmetry need scarcely be considered. They will emerge when our knowledge of the various phases leading to the natural proteins becomes much more detailed.

In considering the possible significance of enzymes in the construction of these complex bodies, it will be possible to draw attention to only a few groups of problems out of a very wide range. In plant physiology at the present time, the following questions seem to the writer worthy of attention in that they may suggest opportunities for experimental attack through a study of enzyme activity.

1. The manner in which nitrogen is first included in the simpler substances from which the protein is subsequently formed.

2. The possible relation of carbohydrate metabolism to the synthesis of proteins.

3. The rôle played by enzymes in the hydrolysis of storage forms of proteins.

4. The significance of recent attempts to produce a reversible reaction in definite cases of the hydrolysis of proteins by enzymes.

[1] For Part I. see SCIENCE PROGRESS, July 1913.

For the sake of clearness these four questions will be considered separately, though obviously they are all sub-divisions of the one general problem of constructive nitrogen metabolism in the organism.

1. The First Inclusion of Nitrogen

There is now a very general consensus of opinion that the nitrogen absorbed by a green plant through its roots may have been presented to it in various forms, the most suitable being nitrates, though ammonium compounds may readily be utilised.[1]

The greater value of nitrates to the plant may be due to the fact that in the reduction of these compounds energy may be liberated that can be utilised in synthetic processes. Thus the agricultural chemist is familiar with the fact that the employment of nitrate as a manure is followed by accelerated vegetative growth; and there is considerable evidence that energy supplied in the form of nitrate may be in part utilised in the series of katabolic changes connected with respiration and growth.

The nitrate supplied to the plant has to be reduced, as in the protein molecule it occurs in association with hydrogen. The distribution of nitrates within the plant indicates that this reduction usually occurs in the tissues of the leaf, as it is here that the nitrates are found to disappear. If then enzymes are employed in this reduction, they should be present in the leaf. Enzymes with this power of reducing nitrate have been obtained from plant tissues by some investigators, but they do not represent the only possible agency for the reduction of nitrates.

Bach has suggested that formaldehyde, so often reported as present in leaves, might reduce the nitrate to hydroxylamine, this subsequently giving rise to formaldoxime and ultimately to formamide:

$$\overset{\text{H}}{\underset{}{\text{CHO}}} + NH_2OH \rightarrow \overset{\text{H}}{\underset{\text{H}}{\text{C}}} : N \,.\, OH + H_2O$$

and

$$\overset{\text{H}}{\underset{\text{H}}{\text{C}}} : N \,.\, OH \rightarrow \overset{\text{H}}{\underset{}{\text{CO}}} \,.\, NH_2$$

Formaldoxime.   Formamide.
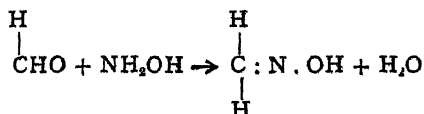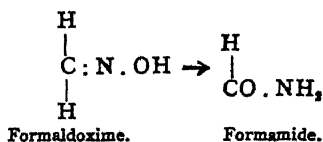
[1] Hutchinson and Miller, "The Assimilation of Nitrogen by Higher Plants," *Journal of Agricultural Science*, vol. iv. p. 282, 1912.

Recently Baudisch[1] has shown that daylight alone may reduce nitrates in solution to nitrites, and that both nitrates and nitrites are readily reduced by aldehydes in presence of light with the ultimate production of ammonia and amino-compounds.

In view of these possibilities, the aid of reducing enzymes or reductases may not be necessary; on the other hand their presence may enable protein synthesis to continue in the dark. In any case, the evidence is increasing that enzymes of specific or general reducing activity are present in both plant and animal tissues.[2]

There are indications that reductases would be more frequently met with in plant extracts if it were not for the presence, in the same juice, of oxidising enzymes or oxidases. These two types of enzymes may exist side by side in the same cell, and both exert their full activity without interference, because their respective spheres of action may be limited by living semi-permeable membranes. In crushing the tissues to extract the enzymes, these controlling membranes are destroyed, the two enzymes come in contact, and the reductase may be destroyed or its activity neutralised.

The immediate result of the reduction of a nitrate in the living cell must be the production of the very poisonous nitrite. This body must be again transformed immediately or the death of the cell will follow. It may therefore be very difficult to detect the transitory appearance within the organism of the nitrite, and thus establish the existence of a reducing action. On the other hand, as we have seen, in experimenting " in vitro," there is the difficulty of extracting the reductase in an active condition.

Irving and Hankinson obtained evidence of the presence of a nitrate-reducing enzyme in the plant by placing chloroformed leaf tissue in a solution of asparagin and potassium nitrate.

The sap of the plant was acid, and in an acid medium, if nitrites are formed in the presence of asparagin, gaseous nitrogen must be liberated. Thus—

$$2HNO_2 + \underset{\substack{| \\ CH_2 - CONH_2 \\ \text{(Asparagin.)}}}{CH.NH_2 - COOH} \rightarrow \underset{\substack{| \\ CH_2 - COOH \\ \text{(Malic acid.)}}}{CH.OH - COOH} + 2N_2 + 2H_2O$$
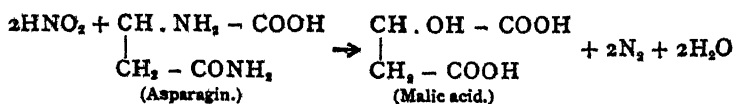
[1] Baudisch, *Ber. deut. Chem. Ges.* **44**, p. 1009, 1911.

[2] On animal reductases, see D. F. Harris and H. J. M. Creighton, *Proc. Roy. Soc.* **85** B. p. 486, and *Bioch. Journ.* vi. p. 429. On plant reductases, see Kastle and Elvove, *Amer. Chem. Journ.* **31**, p. 606, 1904, and Irving and Hankinson, *Bioch. Journ.*, vol. vii. p. 87.

Considerable quantities of nitrogen were given off by these chloroformed plants in the presence of asparagin, and there seems little doubt that this was evolved as the result of reduction of nitrate by the reductase present in the tissue.

There is no reason at present to anticipate that enzymes play a part in the stages that follow upon the formation of nitrites until at length the "amino" linkage, NH_2, is reached. The intervening compounds are likely to be so highly reactive that the successive changes are probably instantaneous and there seems no necessity to assume the intervention of a catalyst.

The suggestions of Bach and Baudisch previously mentioned raise the question whether the synthesis of proteins can be considered independently of carbohydrate metabolism. Aldehydes play a part in the suggested reactions, and it is during photosynthesis that such bodies are likely to be formed in the plant. Moreover, recent work in another direction has drawn attention to the possibility of the intervention of enzymes in the passage from the carbohydrate to the amino-acid. This work will be considered in the succeeding section.

2. Relation of Carbohydrate Metabolism to Protein Synthesis

In the preceding section reference was made to a possible inter-relation between the metabolism of carbohydrates and protein synthesis.

The result of the brilliant synthesis of proteins from amino-acids carried out in the chemical laboratory by Emil Fischer and others [1] has been to focus attention upon the amino-acids as the primary bodies from which must start the synthesis of proteins within the organism. In the plant these amino-acids have to be constructed.

We have seen that the nitrogen, however supplied, is probably brought gradually to the NH_2 grouping, but we have still to ascertain whence the organic acid or aldehyde is obtained, with which this NH_2 grouping may be linked. In this connection the recent work of Dakin and Dudley upon the activities of an enzyme they have termed glyoxalase, may have great significance for students of plant metabolism.

[1] See R. H. Plimmer, "Chemical Constitution of the Proteins," I. and II. Biochem. Monographs, pub. Longmans, Green & Co., for a valuable summary of recent work.

Dakin and Dudley [1] have obtained this enzyme from various animal tissues, such as the muscle and liver of exsanguinated dogs or rabbits. It possesses the power of accelerating very markedly, even under "in vitro" conditions, the reversible reactions by which methyl and phenyl glyoxal are converted into lactic and mandelic acid respectively.

$$CH_3 - CO - CHO + H_2O \;\rightleftharpoons\; CH_3 . CHOH - COOH$$

Methyl glyoxal. Lactic acid.

$$C_6H_5 - CO - CHO + H_2O \;\rightleftharpoons\; C_6H_5 - CHOH - COOH$$

Phenyl glyoxal. Mandelic acid.
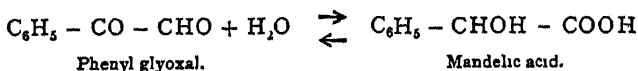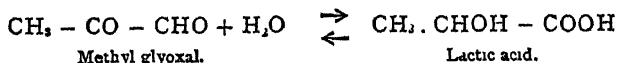
The enzyme is readily obtained in aqueous extract, and the extract loses its activity on being heated to 60° C. Dakin and Dudley failed to precipitate and separate the enzyme by the addition of alcohol, but they succeeded in obtaining an active preparation by precipitating it with solid ammonium sulphate and then dialysing the suspension in water.

The wide distribution of this enzyme may be of considerable significance in reference to the inter-relation of carbohydrate and protein metabolism.

Methyl glyoxal or a closely allied substance is obtained from the action of sodium phosphate on glucose. It is therefore possible to obtain from glucose both organic acids and aldehydes, and these are bodies from which amino-acids may readily be derived. Thus it may be anticipated that within the living cell alanine may be derived from methyl glyoxal, in much the same manner in which glycine has been obtained from glyoxal. The relation of glucose to amino-acids and to organic acids might then be expressed in the following manner : [2]

Glucose.

$$C_6H_{12}O_6$$
$$\downarrow \uparrow$$

Lactic acid. Methyl glyoxal. Alanine.

$$CH_3 . CH . OH . COOH \;\rightleftharpoons\; CH_3 . CO . CHO \;\rightleftharpoons\; CH_3 . CH . NH_2 . COOH$$
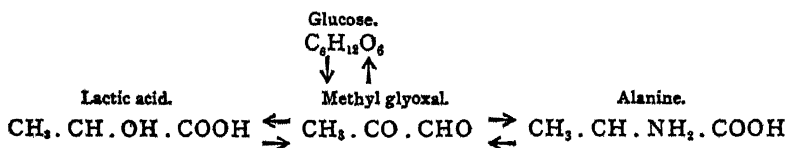
The distribution of this enzyme is clearly of importance to the animal physiologist and may account for the production of glucose in the glycosuric organism, but its distribution in the plant must be known before its significance in plant metabolism

[1] Dakin and Dudley, *Journ. Biological Chem.* xiv. p. 155, and xiv. p. 423.

[2] *Ibid.* xv. p. 127, 1913. Alanine has not yet been synthesised directly from methyl glyoxal.

can be estimated. Dakin and Dudley have found glyoxalase in yeast, an organism which is capable of solving its synthetic problems upon a diet containing glucose and ammonium compounds as the sources of its carbon and nitrogen respectively. If subsequent work should show the enzyme to be widely distributed in the plant kingdom, it will have to be seriously considered as a possible aid in the production of amino-acids, and as one link in the chain relating carbohydrate metabolism to protein synthesis.

3. The Hydrolysis of Protein Reserves

Logically the next step would seem to be to consider the part played by enzymes in the subsequent construction of proteins from the primary amino-acids. Unfortunately this field is almost untouched, and it is impossible to attack it directly. In this section the present position of our knowledge of the hydrolysis of the storage proteins of the plant will be first discussed. This will be followed in the subsequent section by a consideration of the attempts that have been made to produce reversible catalysis in the hydrolyses of proteins by altering concentration conditions.

At the present time, when the decomposition products of the hydrolysis of proteins are still incompletely known, and when the series of hydrolytic changes accompanying that hydrolysis cannot be pictured, it is natural that there should be considerable confusion in the definition of the proteinases, the enzymes or groups of enzymes which are responsible for the hydrolysis.

In the animal kingdom the simplest classification is based upon the distribution of the enzymes within the body, and by this means it is possible to distinguish three groups of proteinases, viz. the peptase (pepsin) of the gastric secretion, the tryptase (trypsin) of the pancreas, and the ereptase (erepsin) of the intestinal mucus.[1]

In addition to this difference in origin, the peptases are usually credited with an activity, restricted to slightly acid solutions, which does not produce complete hydrolysis of the protein digested, the products formed being albumose and peptones.

[1] For a general account, see Euler, *General Chemistry of Enzymes*, translated by Pope, pp. 33 *et seq.*

Tryptase on the other hand is regarded as being capable both of acting in neutral, slightly acid, or alkaline solutions, and of carrying the digestive hydrolysis as far as the production of polypeptides and amino-acids. Recent work suggests that this difference in the extent to which hydrolysis is carried is not really significant, but that, if sufficient time be allowed, amino-acids will be found among the products of peptic digestion.[1]

Ereptase activates hydrolysis from the point at which peptase is usually regarded as ceasing to act. Acting upon albumoses and peptones, it converts them, apparently completely, into polypeptides and amino-acids.

In plants these three groups of enzymes cannot be separated by any reference to their distribution. The peptic type of enzyme seems to be chiefly represented in the secretions of insectivorous plants, such as Nepenthes. These enzymes Vines terms ecto-peptases to distinguish them from the internally held and controlled enzymes of similar catalytic activity in protein hydrolysis, such as the endo-peptase present in yeast.

The enzymes which are usually regarded as active in re-converting the deposits of aleurone grains within the seed into amino-acids were first described as tryptic, because their activity resulted in the formation of amino-acids from the proteins hydrolysed. Vines in a series of papers[2] has built up a strong case for interpreting all cases of so-called "tryptic" digestion in plants as due in reality to two enzymes, acting on two different stages. The first stage from protein to peptone is regarded as due to the catalytic action of a peptase—an ecto-peptase in the excretion of Nepenthes capable of acting in the presence of hydrochloric acid or organic acids, but inactive in neutral or alkaline solutions, and an endo-peptase in the tissues of the seedling and elsewhere, incapable of action in the presence of hydrochloric acid. The second stage from peptone to amino-acid is regarded as due to the catalytic activity of a widely distributed ereptase capable of acting in either acid, neutral or alkaline solution.

Vines was led to suspect the existence of these two stages by noticing the different effect exerted upon the rate of the two

[1] Lawrow, *Zeit. für Physiol. Chem.* 28, p. 513.

[2] Vines, *Annals of Botany*, xi. p. 563, xii. p. 545, xv. p. 563, xvi. p. 1, xvii. p. 237, xviii. p. 289, xix. pp. 149 and 171, xx. p. 113, xxii. p. 103, xxiii. p. 1, xxiv. p. 215.

stages of the hydrolysis, by various antiseptics used in the course of his investigation. If the same enzyme was responsible for both stages of the hydrolysis, then the change in velocity in the two stages produced by the addition of the reagent should be proportionately the same, but it was very far from being so. Guided by this clue he subsequently succeeded in isolating from hemp seed and from other sources extracts of the two enzymes which were each strictly limited in their activity to one stage of the hydrolysis. This separation had proved to be possible owing to the fact that while ereptase is readily soluble in water, the endo-peptase present with it is practically insoluble in distilled water but readily soluble in solutions of sodium chloride.

The importance of these investigations from our present point of view is obvious; everything points to the complex series of changes which ultimately effect the conversion of a protein into an amino-acid, occurring under the action of a series of enzymes or groups of enzymes. In view of the fact that of the animal enzymes, peptase acts with the greater celerity on complex proteins, it should perhaps be regarded as the first group of enzymes in the series, and the protein in its decomposition would then come under the action of three groups of enzymes successively :[1]

"Pepsin" or Ecto-peptase group	"Trypsin" or Endo peptase group.	Ereptase group.
Protein → Albumoses.	→ Peptones.	→ Amino-acids.

Clearly then, when an attempt is made to follow this series of reactions with enzyme catalysts in the direction of synthesis, it would seem advisable to attempt to follow these steps in the reverse order. But the significance of ereptase has been recognised only in comparatively recent times, and as the work we shall have to consider in the succeeding section has been carried out with the other enzymes of the series, the starting point for the synthesis has been a vaguely defined admixture of bodies instead of amino-acids of definitely known composition.

In the case of the plant, the question is at present complicated by the incompleteness of our knowledge of the amino-

[1] See Bayliss, *Nature of Enzyme Action*, 2nd ed. p. 115. It is as yet impossible, however, to correlate with any certainty this series of three enzyme groups with the phenomena of proteoclastic digestion in the plant.

acids formed upon the digestion of the protein food reserves. Very little is known beyond the fact that asparagin seems usually to be the chief amino-acid formed, accompanied by certain quantities of leucin and tryrosin. These amino-acids pass up the stem of the germinating seedling and seem to disappear in the leaf contemporaneously with the beginning of photosynthetic activity. But our knowledge of these phenomena is still far too nebulous to make speculation profitable regarding the part played by enzymes in this subsequent synthesis in the leaf.

The succeeding section of this paper will therefore consist simply of a critical review of certain supposed syntheses of proteins with the aid of enzymes, under "in vitro" conditions. The earlier and more significant stage in synthesis, the first linkages of the amino-acids, unfortunately cannot be discussed at all from the standpoint of enzyme catalysis,[1] owing to the fact that no reversible syntheses with the ereptase group of enzymes have been described.

4. Protein Synthesis by Reversible Catalysis, from the Products of Protein Hydrolysis

The probability is that the experiments now to be described provide sufficient evidence to establish the fact that the catalysis of protein hydrolysis can proceed in the reverse direction, that of synthesis, under the action of the same enzyme; but owing to the difficulty of identifying with chemical exactitude either initial or end products, very little definite information has yet been obtained of the course of such a synthetic reaction.

The so-called "plastein"[2] formation obtained by Danilewski and his co-workers is a typical example of this class of experiment. This investigator found that by leaving concentrated solutions of Witte's peptone in contact with rennet, precipitates were obtained which gave characteristic protein reactions.

Preparations of peptase introduced into peptones produced the same result. This work has since been confirmed and extended, other enzymes being employed, and in some cases similar precipitates have been obtained from solutions initially containing amino-acids and polypeptides.

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[1] Lawrow, Hoppe-Seyler's *Zeit. f. Physiol. Chem.* 51, p. 1.
[2] See Euler, Trans. Pope, *loc. cit.* p. 265, for summary of this work.

In respect to the difficulties of interpretation, this work is typical of this class of investigation. In the first place there is no certain evidence that the precipitates obtained are composed of proteins ; according to some statements they contain too little nitrogen to be classed as protein although they give the reactions of bodies of this class. There is certainly no evidence that they represent the protein bodies from which the peptones were derived by previous hydrolysis, consequently the relation of the reaction to the catalysis of a reversible hydrolysis is not clear.

This brings us to the second outstanding difficulty, namely that it is not at all clear that the production of these bodies is to be associated in any way with a catalysis of a chemical reaction. Under the existing conditions nothing would seem more probable than a precipitation due to the withdrawal of water from some of the more complex colloids present. The precipitation would in that case be equivalent to the phenomenon of " salting out," and if there were any protein-like bodies present which did not form part of the original precipitate, they would almost certainly be carried out of solution by the precipitate as the result of adsorption.

In a less degree the same criticism applies to the experiments of Taylor,[1] who in the first place subjected 400 grams of protamin to complete tryptic digestion, and then, converting the products of hydrolysis into carbonates, subjected them to the action of a considerable quantity of tryptase. At the end of five months, about 2 grams of protamin, weighed as sulphate, were recovered from the solution.

If these experiments are regarded as synthesis under the concentration conditions existing, a certain amount of support is afforded to this point of view by other phenomena.

In the first place, this is the simplest explanation to give of the retardation of protein hydrolysis produced by the accumulation of the products of hydrolysis. The equilibrium point in a reversible reaction is being approached, and if the products of hydrolysis are present in sufficient quantity a reversal of the reaction in a synthetic direction may be expected. Secondly, such a reversal of the reaction is the simplest explanation of the changes in conductivity of a tryptic digest

[1] A. E. Taylor, Univ. of California Publ. *Pathol.* i. p. 343 ; and *Journ. of Biol. Chem.* iii. p. 87.

upon concentration. Bayliss[1] found that as the hydrolysis of caseinogen by tryptase proceeded, the conductivity of the solution increased, but that after concentration of the solution, in the presence of the enzyme, the conductivity diminished. This certainly seems to point to a reversal of reaction in the direction of synthesis.

More recently Brailsford Robertson[2] has made a very full study of one reaction of this type. His investigations deserve fuller description because of the attempt he has made to meet the theoretical difficulties created by the concentration conditions which are found necessary to bring about these reactions.

In Robertson's initial experiments 400 c.c. of N/50 potassium hydroxide saturated with casein were, after complete digestion, concentrated to 70 c.c. To this solution were added 30 c.c. of a 10 per cent. solution of Grubler's pepsin. Within two hours a precipitate had formed which was shown to be one of the constituents—paranuclein A—of the mixture of proteins which had been previously hydrolysed.

At first sight this was again to be interpreted as simply a case of a reversible reaction undergoing catalysis, under the concentration conditions existing, in the direction of synthesis; but further experiments rendered this simpler explanation impossible. In the first place, if this were purely a catalytic action, then the enzyme catalyst could produce no change in the point of equilibrium of the reaction. But as a matter of fact Robertson found that by adding the pepsin in sufficiently concentrated form, synthesis could be brought about in a solution containing the products of hydrolysis without any previous concentration whatever of this solution.

Further, it was found possible to obtain the reversible synthesis in lower concentration of enzyme and substrate by simply raising the temperature, and in the end ready reversal of the hydrolytic action was obtained at a temperature of 65° C., a temperature ten to fifteen degrees higher than that at which the normal hydrolytic activity of pepsin is known to occur.

Now obviously these facts cannot be explained upon the usual assumption that the enzyme present is behaving as a normal organic catalyst, in fact the last experiments referred to clearly point to a synthetic action, if catalytic, as resulting from

[1] Bayliss, *Nature of Enzyme Action*, 2nd ed. p. 53.
[2] T. B. Robertson, *Journ. Biol. Chem.* iii. p. 95 and v. p. 493.

the activity of a catalyst of a different nature from the original pepsin employed.

Before proceeding to consider Robertson's theory as to how these phenomena may best be correlated with theories of enzyme catalysts, it will be well to consider critically the validity of the evidence upon which the theory of enzyme catalysis is to be extended to cover new phenomena.

It is obvious that the statement that we are dealing in these experiments with a reversible catalysis induced by enzymes implies that we are satisfied with the evidence in reference to two points. These are (1) that the body produced after concentration and addition of the enzyme is really " paranuclein A," and identical with one of the original bodies hydrolysed; (2) that this body is actually produced in the solution as the result of chemical action of a synthetic nature.

On both these points it is necessary at present to withhold a definite opinion.

With regard to the first point, the paranucleins are a group of bodies which are indefinitely characterized and separated to a large extent upon the evidence of the phosphorus content. The percentage of phosphorus in the bodies produced in these experiments was by no means always constant or identical with that usually associated with " paranuclein A." At the same time it was well within the limits usually associated with this class of bodies and it would be natural that the results of synthesis, like the starting point of hydrolysis, should be an admixture of bodies.

In view of the difficulty of characterising " paranuclein," considerable importance attaches to the comparison by Gay and Robertson[1] of the immunity reactions produced by paranuclein, and by this body synthetically produced from the products of hydrolysis.

These two bodies apparently possess, as tested by subcutaneous injections into guinea pigs, identical and specific antigenic properties which are not present in the original products of peptic digestion. It has, however, to be remembered that the products of peptic hydrolysis would contain this " synthetic paranuclein," if present, in considerable dilution, and the effects produced by injections might therefore be much less marked.

[1] Gay and Robertson, *Journ. of Biol. Chem.* xii. p. 233.

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At the present time it is perhaps advisable to regard the identity of the "synthetic paranuclein" as an open question, especially in view of the recent experiments of Bayliss.[1]

The latter investigator has thrown considerable doubt on the second point at issue in relation to this reaction, viz. its chemical nature. His experiments point definitely to a colloidal precipitation between the enzyme and a colloid present in the peptic digest owing to the method of its preparation. If this other colloid is first removed from the digest, for instance by addition of hydrochloric acid up to a concentration of 0·5 per cent. then upon subsequent filtration and neutralisation it is impossible to get this precipitate formed upon the addition of the enzyme. On the other hand the precipitate given by the acid, upon redissolving in the smallest possible quantity of alkali, is readily reprecipitated by the addition of pepsin.

This suggests that the appearance of this precipitate is due to the precipitation of oppositely charged colloids, a view which is supported by the comparative rapidity with which it is brought about. Bayliss strengthens the evidence for this hypothesis by showing that a similar precipitation may be produced in the products of the peptic hydrolysis by the additions of other substances than pepsin, that is to say, by other colloids which are not enzymes.

It is therefore unnecessary at the present time to do more than glance at the interesting hypothesis of reciprocal catalysis put forward by Robertson[2] to reconcile in the simplest manner these apparently new types of enzyme catalysis with the Van 't Hoff view that enzymes, behaving as normal catalysts, must, given proper concentration conditions, accelerate synthetic actions. Robertson suggests that at high concentration the enzyme may be present in a dehydrated form and that this form, which is stable at higher temperature than the normal enzyme, may be responsible for the catalysis of the synthetic reaction.

If further investigation should show the necessity for it, this ingenious hypothesis will certainly deserve serious consideration. But it cannot be too strongly emphasised that advance in a complex series of problems such as these will probably be facilitated by a rigid adherence to the simplest possible

[1] *Journ. of Physiology*, xlvi. p. 236.
[2] *Journ. of Biol. Chem.*, v. p. 510.

explanation of observed phenomena until investigation shall establish beyond a doubt that the simple explanation, such as that an enzyme as a catalyst obeys the physico-chemical laws governing the definition of a catalyst, will no longer cover the whole of the ascertained phenomena.

It is therefore considered premature for the same reason to discuss Euler's[1] suggestions as to anti-enzymes being active in synthesis. It is not yet clear that simpler explanations will not suffice.

Euler points out that various investigators have found that the result of subcutaneous injection of enzymes into the animal organism is the production of specific anti-bodies. These bodies, termed in some cases anti-enzymes, have been reported to exhibit catalytic activity, and it is suggested that they act in the direction of synthesis and not of hydrolysis.

It is quite possible that this hypothesis may ultimately prove of value, but it is at present unnecessary for the explanation of the observed reversal of enzyme action. It is also perhaps worth pointing out that the terminology adopted is a little unfortunate, because the term anti-enzyme has often been used in reference to specific cases, and the implication has been that the anti-enzyme concerned produced its inhibiting effect directly upon another enzyme,[2] and not necessarily by accelerating a reaction against its normal equilibrium conditions. Furthermore, such bodies as Euler refers to should surely have no claim at all to the name of enzyme. An enzyme has been generally regarded as an organic catalyst, and these bodies cannot be regarded as, in any sense of the word, chemical catalysts. They seem to act in defiance of the laws of mass action.

Finally, in considering the various aspects of the subject reviewed in this paper, the writer would emphasise the fact that there is no pretence of giving more than partial glimpses of a very extensive problem. Both in relation to carbohydrate and protein metabolism, the physiologist anxious for guidance in his attempt to outline experimentally the highways of metabolic activity in the organism is bewildered by the variety of hypothesis permitted him by the fruitful discoveries of

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[1] Euler, *loc. cit.* p. 267 (Eng. ed.).

[2] See, for instance, Czapek upon anti-oxidase, *Ann. of Botany*, or the use of the term anti-glyoxalose by Dakin and Dudley, *Journ. of Biol. Chem.* xiv. p. 463.

organic chemistry. If a student of the problem is ever to pass from the contemplation of the work of his colleagues to experiment, then he must resolutely close his eyes to many of these alluring possibilities and, concentrating his attention upon one feature of the problem, learn by experience what facilities physical and chemical methods provide him for its experimental solution.

In these pages an attempt has been made to consider the enzyme as a possible agent of synthesis with a view to submitting the problem to subsequent investigation in the laboratory. No one is more conscious than the writer of his inability to treat this side of a general problem with adequate freedom and confidence, and he would greatly appreciate the criticisms and suggestions of others who are more conversant with the questions discussed or who are approaching them from different points of view.

THE PHYSICAL ASPECT OF THE OPSONIC EXPERIMENT

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THE recognition of the principle that prevention is better than cure, obvious though it may seem, has of late years exercised a considerable influence on medical research. Side by side with the development of preventive sanitation has advanced the investigation into the reasons why infection is escaped by certain individuals. The importance of leucocyte and serum as protective agents has been fully established, and the reinforcement of their potency by vaccine therapy, general hygiene, and the like, has led to the foundation of a new school of medical practice. That the serum alone may overcome the intruding microbe of disease is an accepted fact, and the discovery by Sir Almroth Wright of its important rôle in the vital phenomenon of phagocytosis has still further focussed attention on it. As the serum is a fluid, and as a fluid can hardly be credited with vital activity, the part which it plays in the process of immunity is capable of investigation by the methods of physics and chemistry. On account, however, of the complex nature of the substances involved, little advance has been made by purely chemical methods. In place of these, the mathematical methods of physical chemistry which deal with velocities of reaction, and equilibrium states, have been applied, and in this direction considerable progress has been made by Arrhenius and others. Thus in this case at least the application of mathematics has been of service to medicine. But immunity does not depend on the serum alone. The leucocyte is, as I have said, a factor in the destruction of the intruding microbe, and it is with this aspect of the question that I propose to deal.

The phenomenon of phagocytosis as it applies to disease

may be described as the ingestion of micro-organisms by leucocytes. Wright has shown that this ingestion is more rapid when it takes place in serum from an immune animal, than in that from a non-immune. This fact can be quantitatively determined by the method of measuring the degree of phagocytosis devised by Leishman. The experiment as performed in the laboratory is as follows: An intimate mixture of leucocytes and micro-organisms is placed in an appropriate vessel and kept at blood heat. After about fifteen minutes, a sample drop of the mixture is taken out, placed on a microscope slide, and spread out into a thin film. The film is fixed and stained by a method which causes leucocytes and organisms to assume different colours. The number of micro-organisms ingested by (say) 100 leucocytes is counted—and this divided by 100 gives the average content. If two experiments are performed in this manner, one with an unknown serum and the other with a serum which is known to be normal, and if the average content with unknown serum is divided by the average content with the normal serum, a value is obtained which is called the *opsonic index.*

The phenomenon which has taken place between the two types of cell, leucocyte and micro-organism, is a complex one. Leaving the mode of action of the immune serum out of account, each ingestion may be considered as having taken place in two stages: firstly, collision between a leucocyte and an organism; and secondly, the inclusion of that organism in the protoplasm of the leucocyte.

The stage of collision, and the conditions which lead up to it, are obviously capable of statistical treatment—just as the kinetic theory of gases can be treated from a statistical point of view. It may be argued, however, that no comparison can be drawn between the conduct of a molecule of a gas and that of a living cell; that whilst, where there is no life, particles may follow random paths, such will not be the case with living cells which are apparently capable of voluntary movement and effort. (Such movements are no doubt chemio-tactic and only simulate voluntary movements.) But on the other hand it must be remembered that the leucocyte is in an environment of particles of food of an equally tempting nature which are scattered at random in its vicinity. It need only browse at random as a cow browses over a fat pasture.

Let us, for convenience, divide the leucocytes which have been counted into groups, according to the number of organisms they contain; and let the number of leucocytes in any particular group m be y_m. Thus y_0 denotes the number of *empty* leucocytes counted, and y_1 denotes the number of leucocytes which contain one micro-organism. Now if we compare two records of counts which give the same average, and if collisions occur at random, the distribution of leucocytes amongst the various groups should be the same in the two records, apart from errors of experiment; and such is found to be the case. Certain workers have adopted the proportion of empty cells, in place of the mean content, as a basis of comparison for obtaining the opsonic index. That there is a relation between the proportion of empty cells and the mean is true; but to estimate the activity of a community on the basis of the proportion of individuals who have failed to obtain work, is hardly as fair as to compare average work performed. If, however, such a relation exists— that is, if the average content can be calculated from the proportion of empty cells—we shall have a method by which an estimation may be made in a few minutes with very little trouble.

The mathematical treatment is as follows : When a leucocyte which contains, say, 5 organisms collides with a free organism, it becomes a member of the group which contains 6; and the rate at which such collisions occur is proportional to the number in the group 5. Similarly, an individual in group 6 passes into group 7 on collision, and the rate at which such collisions occur is proportional to the number in group 6. Thus the population of group 6 is increased at a rate proportional to the number in group 5, and depleted at a rate proportional to the number in its own group (6); or, for group m :

(1) $$\frac{dy_m}{dt} = (y_{m-1} - y) \, \phi(t)$$

where $\phi(t)$ is a complex factor denoting the probability of an ingestion occurring.

(1) It depends on the number of micro-organisms which are free at the moment, but this is being gradually diminished as the time goes on.

(2) It depends on the concentration of certain factors in the serum, and this also decreases as the time passes.

(3) It depends on the temperature, which may be constant, or may be allowed to vary.

Thus $\phi(t)$ is a function of the time which, in the present state of our knowledge, we cannot define. Under the conditions of experiment all the leucocytes were originally empty, hence for $(m = 0)$,

$$\frac{dy_0}{dt} = - y_0\phi(t)$$

the existence of groups containing a minus number being impossible.

From these two equations we can eliminate the time, and consequently all the unknown factors; and we have:

$$\frac{dy}{dz} = y_{m-1} - y_m$$

$$\text{where } z = \log_e \frac{a_0}{y_0}$$

a_0 being the initial number of empty leucocytes.

From this equation we have:

$$y_1 = y_0 z$$

$$y_2 = y_0 \frac{z^2}{2!}$$

$$\vdots$$

$$y_m = y_0 \frac{z^m}{m!}$$

Now the average content is:

$$\frac{0y_0 + 1y_1 + 2y_2 + \ldots my_m + \ldots}{y_0 + y_1 + y_2 + y_3 + \ldots y_m + \ldots}$$

$$= \frac{y_0\left(z + 2\frac{z^2}{2!} + 3\frac{z^3}{3!} + \ldots\right)}{y_0\left(1 + z + \frac{z^2}{2!} + \ldots\right)}$$

$$= \log_e \frac{a_0}{y_0}$$

This is, then, the relation between the average content and the proportion of empty cells, and gives a practical method of estimating the average which is of considerable value when the proportion of empty cells is not too low.

The *opsonic index* is thus :

$$\frac{(\log a_0 - \log y_0) \text{ unknown}}{(\log a_0 - \log y_0) \text{ normal}}$$

In this calculation ordinary logarithms to base 10 may be used.

In Table I. a close agreement between observed and calculated figures is shown. The first column gives the group number—*i.e.* containing 0, 1, ..., etc. Observed and calculated figures of numbers of cells in each group are tabulated side by side in the other columns. The first three experiments are from Fleming (quoted by Greenwood, *Biometrica*); the latter two are by Harvey (*Biometrica*, vii. p. 64).

TABLE I.

	Obs.	Calc.	Obs.	Calc.	Obs.	Calc.	Obs	Calc.	Obs.	Calc.
Cells containing 0	19	(19)	99	(99)	41	(41)	620	(620)	632	(632)
,, 1	59	57·89	227	206·8	126	119·1	282	296·3	282	290
,, 2	98	88·2	208	216·1	154	173·1	79	70·8	65	66·5
,, 3	88	89·7	134	150·5	164	167·7	16	11·29	16	10 1
,, 4	65	68·24	78	78·63	121	121·8	2	1·349	4	1·1
,, 5	37	41·58	34	32·85	62	70 8	1	0·131	1	0·1
,, 6	17	21·12	9	11·44	36	34·3	—	—	—	—
,, 7	8	9·192	7	3·415	35	14·2	—	—	—	—
,, 8	5	3·501	3	0·8921	5	5·17	—	—	—	—
,, 9	2	1·185	0	—	2	1·67	—	—	—	—
,, 10	1	0·361	0	—	3	0·48	—	—	—	—
,, 11	0	—	0	—	1	—	—	—	—	—
,, 12	1	—	1	—	—	—	—	—	—	—
Mean . .	3·005	3·047	2·0825	2·0832	3·040	2·9065	0·50	0·478	0·48	0 45887

In the foregoing argument two factors have been neglected which may operate during an experiment. In the first place, it is very probable that the faculty of ingestion will diminish as the leucocyte fills up; and in the second, if under the conditions of experiment sedimentation be permitted, and if there be a difference in the specific gravities of micro-organism and leucocyte, then the engorged leucocyte may move into a thicker swarm. These two factors will operate in contrary directions, the former causing a decreased appetite and the latter an apparent increase of appetite. I have seen no indication of decreased appetite in the figures which I have examined, but comparison between the column with heading "Mean observed" and the column "z" (mean calculated) in Table II.,

TABLE II.

Description.	Observed.				z.	$\frac{c}{b}$ from SD [a] $= e^{\frac{c}{b}z} \times$ mean
	$a_{0'}$	$y_{0'}$	Mean.	SD.		
Fleming, Norm. S.B. . .	400	19	3·005	1·8207	3·04702	0·032
„ Norm. S.A. . .	750	41	3·040	1·8927	2·9065	0·056
„ No. 2. . .	800	99	2·0825	1·5397	2·0832	0·059
„ T. Ch. . .	1,000	152	2·145	1·6401	1·88388	0·120
„ 10 Norm. .	1,010	111	2·571	1·8448	2·20818	0·121
„ T. A. . .	1,100	58	3·7291	2·3820	2·94263	0·142
Greenwood . . .	20,000	1,428	3·6797	2·6031	2·63946	0·196
Strangeways I. . .	1,000	219	1·927	1·7370	1·51869	0·292
„ II. S.C. .	1,000	279	1·521	1·4885	1·27655	0·294
„ II. C. & S. .	1,000	198	1·888	1·7209	1·61949	0·277
„ IV. S.C. .	1,000	243	1·706	1·6086	1·41470	0·294
„ III. . .	1,000	188	2·014	1·7239	1·67132	0·293
„ V. . .	1,000	192	2·119	1·8207	1·65026	0·271
„ VI. . .	1,000	207	1·901	1·6091	1·57504	0·246
„ VII. . .	1,000	240	1·851	1·6730	1·42712	0·289
„ IV. . .	2,000	495	1·689	1·5825	1·39635	0·282

shows that an apparent increase may occur. It must, however, be clearly borne in mind that this apparent increase, though true for the particular experiment, is not a true measure of Immunity as it affects the host, from which the serum is drawn, for in the swirl of the blood-stream such sedimentation cannot occur. It is, in short, an experimental error which should be eliminated if a correct estimate of Immunity is sought for. The point may be investigated as follows: If there be an alteration of appetite with ingestion, equation (1) takes the form:

$$\frac{dy_n}{dt} = (f_{m-1} \, y_{m-1} - f_m \, y_m) \, \phi$$

As the variation of appetite is a very slight one, we may use the approximations:

$$f_m = b + cm \text{ or } f_m = b - cm$$

And we find:

for $f_m = b + cm$

$$y_m = y_0 \frac{b}{c} \left(\frac{b}{c} + 1\right) \cdots \left(\frac{b}{c} + m - 1\right) \frac{(1 - e^{-\frac{c}{b}z})^m}{m!}$$

$$\text{Mean} = \frac{b}{c} \left(e^{\frac{c}{b}z} - 1\right)$$

$$\text{(Standard deviation)}^2 = e^{\frac{c}{b}z} \times \text{mean.}$$

For $f_m = b - cm$

$$y_m = y_0 \frac{b}{c}\left(\frac{b}{c} - 1\right)\ldots\left(\frac{b}{c} - m + 1\right)\frac{(e - 1)^n}{m!}$$

$$\text{Mean} = \frac{b}{c}\left(1 - e^{-\frac{c}{b}z}\right)$$

$(\text{Standard deviation})^2 = e^{-\frac{c}{b}z} \times \text{mean}.$

The figures in Table III. show the result of a calculation

TABLE III.

	Strangeways No. 1.	
	Obs.	Calc
0	219	(219)
1	267	267 4
2	219	211·78
3	129	137·45
4	70	79·38
5	50	42·437
6	26	21·473
7	13	10·427
8	5	4·9035
9	2	2·2475
10	0	1·0087
11	0	0·44484
Mean . . .	1·927	1·9168
S.D	1·7370	1·7347

on the basis of increased appetite. Table II. gives in the columns headed "Mean" and "z" a comparison between observed means and z, and in the last column I have added values of $\frac{c}{b}$ calculated roughly from the equation:

$$(\text{Standard deviation})^2 = e^{\frac{c}{b}z} \times \text{mean}.$$

An exceedingly interesting result is obtained.

It will be observed that in the case of one worker, Dr. Fleming, the first three values lie between 0 03 and 0·06, and that the last three vary between 0·12 and 0·145. In Dr. Greenwood's experiment the figure rises to 0·196, in spite of the magnitude of the experiment; whilst Dr. Strangeways' experiments show values varying from 0·247 to 0·295 (i.e. twice as great as Fleming's latter figures, and six times as great as his first three). The consistency of the figures obtained by the different workers

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points to differences of method, and is in itself an indication that the apparent increase of appetite is due to the artificial conditions of the experiment.

From the above analysis we see:

(1) That the phenomenon of phagocytosis can be satisfactorily treated from the physical point of view as a random interfusion between two perfectly intermixed systems of particles, each of which is evenly distributed, in which ingestion takes place when individuals of opposite type have collided.

(2) That the average content can be calculated from the proportion of leucocytes which remain empty at the conclusion of the experiment, the actual observation involving a minimum of labour. And that this method eliminates, to a large extent, the personal factor of the particular investigator.

(3) The frequency distribution obtained, and the consequent relation between the average content and the proportion of cells, is independent of $\phi(t)$. For example, the same relations will hold good in an experiment conducted at constant temperature and in one in which the temperature has been allowed to vary in any way whatsoever. In other words, the frequency distribution, or the relation between the populations in the various groups, is obtained after the elimination of an unknown chemical law which governs the velocity of reaction, and is thus independent of it.

HISTORY OF THE VIEWS OF NERVOUS ACTIVITY

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It is always instructive to trace the growth of an idea, to be able to watch the notion of something, even of so elusive a thing as the nerve-impulse, grow gradually in clearness and in definiteness as the centuries roll on.

The term "nerve-impulse" is of course wholly modern. It would not be profitable to go farther back than the time when the Greek philosophers imagined that the nerves were hollow and conveyed "spirits" through the pores (poroi) of their substance.

The Alexandrine School of Greek Anatomy, founded as far back as 300 B.C. by Ptolemy I., recognised the functional difference between sensory and motor nerves. The two best known teachers in it—Herophilus and Erasistratus—devoted much attention to the nervous system; they dissected the nerves to their origins in the brain and spinal cord, they displayed the veins of the brain and investigated its cavities or ventricles, believing that in the Fourth of these, in the Medulla Oblongata, the soul was situated. The meeting place of the venous sinuses of the coverings of the brain is still known as the Torcular Herophili. The physiology taught by Claudius Galen of Rome (131–200 A.D.) was an outgrowth of the Alexandrian. Galen had the clearest conception of the nerve-trunks as merely conductors of something—he called it spirits—to or from the brain and spinal cord. The doctrine of spirits in general he elaborates so as to recognise three kinds of spirits—natural, vital, and animal. We can hardly understand the nerve physiology of the Middle Ages without some notion of these three kinds of spirit. Briefly it was this : the food in the intestine is absorbed into the portal vein and goes to the liver, where it is worked up into blood which is endowed with natural spirits, or, in modern language,

with the powers of nourishing the tissues of the body. The crude blood was then supposed to pass from the liver to the right side of the heart whence most of it percolated through the septum to the left ventricle. This process to some extent refined the blood. In the left auricle in diastole, air was sucked into the heart; which brought about two results, the cooling of the innate heat of the heart and the generating of vital spirits. The vital spirits were carried by the blood in the arteries to all tissues and organs to enable them to perform vital functions. The blood with its vital spirits that went to the brain was supposed to undergo a sort of distillation or refining for the last time, with the result that the animal spirits were separated from it and carried to the body by the nerve-trunks. The animal spirits in motor nerves made muscular movements possible, those in sensory nerves were productive of sensations.

We still speak of animal spirits, of " a man of spirit " and so forth; and the expression "the vapours of alcohol" or "fumes of drugs ascending to the brain " are based on the analogous ascent of vital spirits from the heart to the brain. As recently as the time of Queen Anne (1708) the *Daily Courant* advertised a perfume as efficacious because "it increases all the spirits, natural, vital, and animal." This is exactly in the Galenical order.

The point of interest for us in all this about spirits is that thus early we have glimmerings of the notion of innervation, the agent of which is spirits ; for the animal spirits of Galen are the nerve-impulses of to-day. It will be noticed, however, that there is in this ancient doctrine of spirits some sort of latent distinction between powers of absorbing nourishment, of expressing vitality, and of conferring movements. The modern advance on this is that not even the absorption of nourishment is outside of innervation. The growth of the ideas of innervation centred, as might have been expected, round the power to arouse movements in muscles, in fact around motor innervation only.

The problem which so agitated the physiologists of the eighteenth century had not arisen in Galen's time, namely whether muscles contracted of themselves, for instance after all their nerves were cut (doctrine of Inherent Irritability), or whether all their irritability was conferred on them through their nerves, that is from outside, the so-called doctrine of the Neurologists.

For the sake of clearness it may be well to say at once that muscles have irritability of their own, after all their nerves are cut, but that unless nerve-impulses (tonic) are constantly pouring down upon them, and unless stimuli to action are frequently being received by them, they will waste away because there is nothing to call forth the power of contraction which they do possess.

As regards views on the working of the nerves, we find nothing of any consequence from the death of Galen (200 A.D.) to the time of Vesalius (1543), for the interval of more than a thousand years was occupied by the Dark Ages when there was hardly any investigation of living nature, and very little curiosity about the mysteries of life.

Vesalius wrote of muscle that it " also receives branches of arteries, veins, and nerves, and by reason of the presence of the nerve is never destitute of animal spirits so long as the animal is sound and well. . . . Nor do I with Plato and Aristotle (who do not at all understand the nature of muscle) attribute to the flesh so slight a duty as to serve the purpose of lessening the effects of heat in summer and of cold in winter. On the contrary, I am persuaded that the flesh of muscles, which is different from everything else in the whole body, is the chief agent by the aid of which (the nerves, the messengers of the animal spirits, not being wanting) the muscle becomes thicker, shortens and gathers itself together." Thus writes Vesalius, who does not attempt any explanation : he does not know what spirits are, or how they affect the muscle, or why it *shortens* when they do affect it ; he only knows that something in nerves does influence muscle.

G. A. Borelli of the University of Pisa (1608–1679) the mathematician and author of the *De motu animalium*, endeavoured to be more exact in his conception of *how* this activity of muscle came about under the influence of nerve-impulses.

Borelli at the outset fell into the error that a muscle increases in volume when it goes into activity. He then attempted to get some idea of what these animal spirits were which apparently could inflate muscle, and he thought they must resemble air. But when he cut an active muscle across under water no bubbles of air or gas come out of it ; therefore, he concluded, the spirits were not gaseous. Nevertheless,

something real descends the nerves to influence the muscles, and so Borelli finally called this something the "succus nerveus" or nerve-juice. The analogy he had in his mind was that of an incompressible fluid in a flexible tube which can conduct rapidly from one end to the other of it the disturbance produced by a tap or concussion.

The position of the acute and critical Dane, Stensen or Steno (1638–1686), was wholly agnostic : he wrote, "As the substance of this fluid (nerve-juice) is unknown to us, so is its movement undetermined." Although Steno left the problem of the nature of nerve-impulses unsolved, yet he clearly distinguished between neural activity and muscular irritability.

The Englishman Thomas Willis (1621–1675) reverted to Borelli's position, believing that spirits leapt from the nerves into the muscle-fibres and so dilated them.

Francis Glisson (1579–1677), who formally introduced the conception of irritability into physiology in 1662, contributed something to this subject by showing experimentally that a muscle did *not* alter in volume when it went into a state of activity or contraction. By muscular "contraction," therefore, we do not mean shrinking in volume ; the volume and the density of a muscle remain constant whether in rest or in action.

The great investigator Stephen Hales (1677–1761) made an interesting remark about the nerve-impulse, asking "whether it is confined in channels within the nerves or acts along their surfaces like electrical powers." This is probably the earliest suggestion that the nerve-impulse and electricity have anything in common.

By many subsequent writers, nerve-impulses were considered identical with electricity. The discoveries of Galvani seemed to make such a thing probable. Those experiments of his known as "contractions without metals" seemed to prove that muscles would contract when stimulated by electricity of purely animal origin. What, then, more probable than that nerve-impulses and animal electricity were the same thing? Popular writers forthwith assumed this to be the case, although it was not warranted by any of Galvani's experiments. Galvani's experiments really proved that the feeble differences of electrical potential developed by injuring nerves or, for instance, by the activity of the heart, were sufficient to make a muscle (of the

frog) contract. Galvani was right that there was such a thing as animal electricity, but he was wrong in attributing muscular contraction to it in such cases as those where there were contacts of dissimilar metals; Volta was wrong in denying the existence of electricity of animal origin, but right in claiming that some electricity was of metallic origin and was the true stimulus in several cases in which Galvani thought it to be of animal origin.

It is only comparatively recently that the non-electrical nature of nerve-impulses has been established.

Albrecht Haller (1708-1777) brought the subject into the domain of modern thought by distinguishing three things: the inherent irritability of muscle (the *vis insita*), the nerve-impulse (*vis nervosa*), and the stimulus to the muscle which might or might not be the *vis nervosa*. Writing of the *vis nervosa* he said: " It comes from without, and is carried to the muscles from the brain by the nerves; it is the power by which the muscles are called into action." The *vis nervosa*, taking the place of the *succus nerveus*, remained in nerve physiology until about the middle of the nineteenth century.

Robert Whytt, of the University of Edinburgh (1714-1766), though he furthered the study of reflex action, did not understand nerve-impulses as clearly as did Haller with whom he had a long controversy. Whytt denied to muscles inherent irritability, and thought it was conferred on them by the nerves; he held that the stimulus could convey energy—a view now rightly regarded as a neurological heresy. The controversy lingered on until John Reid (1809-1849) demonstrated that muscles severed from their nerves could, under suitable conditions, retain their contractility for months.

The suitable conditions were, (*a*) blood-supply for the muscles and (*b*) their being constantly " exercised by Galvanism." Reid in this way prevented the muscles showing atrophy from disuse. He kept them in good condition by artificial, electrical instead of by normal, neural stimulation; but the irritability must have been inherent in them in order that the stimuli should act on them at all. The artificial stimuli could not have conferred irritability on the muscles, neither, then, did the normal, neural stimuli. The reception of nerve-impulses (neural stimuli) was only the occasion of the muscles exhibiting the contractility which they possessed independently.

This incomplete historical survey affords us one more instance of what is so interesting in the progress of science— the tendency towards concreteness in conception. We begin in Antiquity with "spirits" in the nerves; the science of the Renaissance converts these into *succus nerveus*, an incompressible fluid such as was being investigated by the physicists of that time; the eighteenth century gives us the *vis nervosa*, which later is identified with the electric current then being studied both in Italy and in England. In the nineteenth century we have nerve-impulses not only measured as to the velocity of their travelling, but actually rendered visible through their concomitant electrical effects. Nerve-impulses are not electricity, but they produce it and can be manifested by it. Thus each generation must think and express itself in the language of its own time.

DIFFERENCES IN ANIMAL AND PLANT LIFE.

By F. CARREL.

In biology no essential difference is considered to exist between animal and vegetal life. Resemblances of reproduction, cell-construction and development, nutrition, digestion, and metabolism are observable in the two states. Some organisms partake of the nature of both kingdoms. Some spores and leaves of plants are motile, and a few animals possess characteristics which are common in plant life.

For these reasons the life-principle is held to be identical throughout living nature.

But when the word principle is used in this connection, it is necessary to be clear as to its meaning. What is termed the principle of life is evidently that series of circumstances whereby organised matter is enabled to stand for a space of time in accretional and assimilatory relationship with the environment. The circumstances are common to both plants and animals, but there are differences in the way in which the relationships occur, and these differences are great enough to divide the manifestations of the principle into two parts which may be called the major and the minor according as they are produced in animal or vegetal form.

In the vegetal form, as is well known, the non-parasitic organism derives its nutriment from the soil [1] and air, and not directly from the flora or the fauna (except partly in the case of insectivorous plants) which surround it. It is thus dependent upon the gases which it obtains from the air, and the salts which it derives from the soil as well as upon water. It possesses no real nervous system, no blood to act as a distributor of nutriment, but is indebted to the influence of chlorophyll and sunlight

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[1] Although the bacteria in the soil which convert nitrogen compounds into ammonia are the means of supplying ammonia to plants, they are, of course, not themselves plant-food. Neither are the symbiotic fungi of the roots of forest trees directly alimentary.

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for the assimilation of its principal food, and although a few animals possess chlorophyll the vast majority do not: consequently chlorophyll, is a distinct feature of vegetal existence.

Again, the plant is surrounded by an almost impervious envelope of cellulose, and although a few animals are said to possess this substance, the great majority are destitute of it: therefore it constitutes a special vegetal characteristic. No plant has visual organs, and though what are known as eye-spots have been observed in plants, these probably serve as means whereby greater response to light is obtained. It is needless to say that no plant possesses the semblance of a heart.

Luminosity is a characteristic of plants as well as of animals, but while in the former the effect is mainly produced in swarms of minute organisms, in the latter it appears in higher forms—in worms and fishes.

Electrical conditions differ in intensity in the two kingdoms. In plants, so far as our present knowledge goes, the currents set up by metabolic changes or the movements of water are very faint. No plant yet discovered exhibits the same phenomenon as the electric fishes which are capable of imparting shocks. *Dionœa muscipula*, which among the plants produces the strongest currents, is precisely one that partly feeds on insects. In animals not only do appreciable currents occur, but in man there are rare but well-authenticated cases where the whole or part of the body gives rise to what appear to be magnetic forces.

It is hardly necessary to say that it is impossible to speak of intelligence in plants in the same terms as of intelligence in animals. All that corresponds to an animal intelligence in plants is the well-known sensitiveness to light which causes the plant to turn its leaves to the luminous source—undoubtedly a chemical effect—and the "movement" of petiole and leaves produced in certain plants of which *Mimosa pudica* is the best example in response to stimulus. But this movement is not conscious movement, and it is now known that it is caused by a difference of turgidity in the protoplasm of the cells brought about either under the influence of darkness or by shock.

In the matter of longevity, the passivity of plant life appears to be in its favour, since none of the higher animals have the longevity of many trees. Few animals hibernate. The forces

of the great majority are expended during the whole of their adult life. The greater part of the higher vegetal life in temperate climates can be said to rest for half the year, and it may well be that this annual period of quiescence, during which the tree merely absorbs sufficient nutriment to preserve its vitality, is one of the causes of its long life.

All land plants are anchored to the soil or rock on which they grow and have no free conscious movement. It is true that many vegetal spores are motile. Those of *Vaucheria* rotate with a screw-like motion on their longer axis, but this movement of plant spores is different from the swimming of animals in the water, and it may possibly be accounted for by an absence of symmetry in the molecular arrangement of the protoplasm of which they are composed. Plant spores, for the rest, are only temporarily motile, and are in transition to the plant state to which they essentially belong. If the spermatozoids of certain plants resemble those of animals, the resemblance is no cause for concluding that they are much nearer to animal life than their development shows them to be, and if insectivorous plants have not their internal cavity fully developed, they are none the less rooted to the soil, and derive a portion of their nutrition from it. They have been known to exist for as long as two years without animal food.

Mobility affords irrefragable proof of life, but whereas in animals it is almost always perceptible or easily excitable, in plants (excepting in the spore phases above alluded to) it may be said to be absent—the leafing of trees and the extension of roots being in reality phenomena of growth. If an animal, like a plant, were chained to one spot without the power of movement it would slowly perish although supplied with food and going through physiological exchanges with the outer world. The adult plant, on the contrary, thrives in immobility. On the part of plants there is no conscious search for food unless it be in the faintest manner by the roots. The plant accepts the nutriment which the soil offers in which it is able to grow as well as the moisture which the rains provide. If moisture is withdrawn from the site on which it stands, then death ensues, since the plant, unlike the animal, cannot remove to more favourable pastures, and must share the fortunes of its environment.

In the matter of nutrition there is a considerable difference of process. In plants all food is taken in a soluble form, for the

plant has the power of forming complex substances from simple ones. In animals the food has to be reduced from the solid and complex to the soluble condition before it can be assimilated. On the whole, however, the substances absorbed by the entire plant kingdom are the same as those absorbed by animals considered as a whole. Like the animal, also, a plant feeds partly on nitrogenous substances and constructs proteids. If, however, plants absorb the same elementary substances as animals they absorb them in different forms and combinations. Plants are fed largely by means of the carbon dioxide existing in the atmosphere, which they accumulate and which, though given off by animals, cannot be breathed by animals, except in minute quantities, without producing suffocation owing to the effect it has of diluting down and excluding the necessary oxygen. For although animals can take this gas into their stomachs, they do not feed upon it directly. Notwithstanding the fact that plants do, like animals, absorb and return oxygen, and exhale carbon dioxide (probably what is not needed for the formation of starch and other substances) it is known that the inhalation of an excess of CO_2 does not kill them.

These differences of functions in this important particular constitute a gap between the two kingdoms. What is rejected in the process of expiration by the one is received as an alimentary necessity by the other in the form in which it is rejected, and although in animals carbon is also an alimentary necessity it is received in the food of animals in combination with other substances and is not directly assimilated. Further, although plants take in both carbon and water and reject what they do not want of these substances, they absorb the carbon in the form of gas and eject the surplus water in the form of vapour. Animals, on the other hand, take in carbon in their food and reject what they do not need chiefly in the form of gas, eliminating surplus water mostly in a liquid state and nitrogen in combination. Neither the plant nor the animal, however, can live for any length of time without oxygen. Both need this substance for the purpose of combustion and both eject it. It is, as we know, by reason of the differences in the manner of nutrition that the balance is maintained whereby life is possible on earth, and they are of the highest significance in a comparison of animal and plant life.

The methods of reproduction are not all similar in the two

reigns. Self-fertilisation is largely to be found in plant life, but is only to be met with among animals in some of the lower forms. No doubt the reproductive process is very much the same in plants and animals once fertilisation has taken place. The agency of chance, however, plays a greater part in the one than in the other. Since plants require the help of the wind and of insects to convey the fertilising element and animals have no such need, this fact constitutes a difference, and the difference is accentuated when the selective characteristic in animals is taken into account. The seeds of plants and animals are not interchangeable. The pollen of a plant, it is needless to say, will not develop in the ovary of an animal, and crosses between distinct representatives of the two kingdoms are not obtained, although no doubt it is not impossible to suppose that zoophytes originally resulted from some accidental cellular fusion between algæ and marine animals. It is true that in the manner of cell-division there is not a great apparent difference between plants and animals. The attractive and repulsive forces at work in the cell-field whereby the transformations are effected which result in the splitting of the chromosomes are practically the same, as far as staining reveals their working; but the material on which they work must necessarily be different. If it were not so it seems evident there would not be dissimilarity of growth; there would only be one category of living things.

The tissues differ in the two kingdoms. If a section be cut from the stem of a higher plant and another from a typical organ of the body of a higher animal and both be examined under the microscope, it will be seen at once that a considerable difference of structure exists. The cells in the former are regular and separated by clearly defined cell-walls of definite thickness, whereas in the latter they are irregular and almost continuous. As Claus and other observers have shown, while the plant cells retain their original and independent form, sharply defined, those of animal tissue suffer numerous changes at the cost of their independence and are often scarcely distinguishable in the mass of protoplasmic material, the reason for this being that the plant cell is surrounded by a non-nitrogenous, while the animal possesses a strongly nitrogenous boundary wall of a far less definite character. The resemblance between the two tissues is greatest in the lower forms of life. It becomes gradually fainter as organic complexity increases.

There are thus dissimilarities between plants and animals which taken as a whole appear sufficient to constitute an essential difference between the two phases of existence, a difference that must necessarily extend to the primal substance of which they are composed. If we cannot know whether or not there was unity in the origin of the substance we need not for that reason be deterred from concluding that there is duality in the development, that is to say in the protoplasm at present extant in the world. The fact that there are minute unicellular organisms which appear composed of the same material and yet to be on the border-line between the two categories of life, need not embarrass us. These organisms stop short at the rudimentary condition. They are rough sketches which are not elaborated and are no obstacles to the view that the principle of life has a dual manifestation. Throughout nature, in addition to well-defined activities, there are to be found tendencies, overlappings, rough models and abortive schemes which need not disturb the judgment in the consideration of the finished work. It is the indeterminate protista that have mainly given rise to the theory of the unity of protoplasm, but these protista go no farther than protista and should not give the rule for the well-defined divisions that come after them. Even at the origin of life it seems probable that the two phases must have been separate unless we are to suppose that the one developed from the other at some later period. But this is not a view to which it seems possible to attach much weight. The motile spores of algæ can scarcely have passed out of the plant phase and become the ancestors of animal existence. The amœba which incorporate their food and move by alternate contraction and extension of their edges, together with all motile feeding micro-organisms not undergoing transformation, might conveniently be considered as animal and the few thousand temporary motile spores as vegetal.

Certain authors like Verworn frequently insist on the identity of plant and animal life. It does not seem possible that there should be identity when there are so many differences of habit and of function. There is some reason to believe that the views of the older investigators who saw an absolute division between the two life states will be ultimately found to be less erroneous than they have been held to be.

It is not easy to find an exact parallel for the dualism which

evidently exists in life, though of course dualism is plainly evident in nature. The force of electricity which divides itself perceptibly into static and dynamic, presents a somewhat close analogy. The inter-relations of the two manifestations might not inconceivably be represented thus:
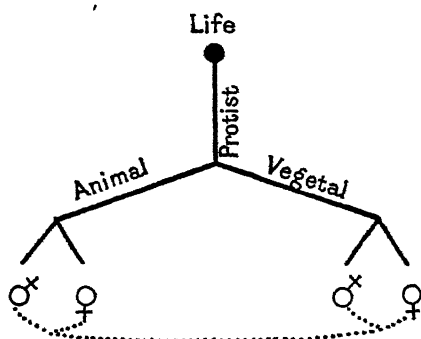
the dotted lines merely indicating the necessary chemical connection.

It is indeed hard to know why there should be so much straining after unity on the part of modern inquirers. Since we are not even sure that the living protoplasm of a horse is absolutely the same as that of a snail or whether there may not be differences in this respect in individuals of the same species, how are we to assume that the protoplasm of plants and animals is one and the same substance?

It is the modern habit of not discriminating between the primal substances of the two kingdoms that has been the cause of errors of interpretation in the application of Mendelian principles. What is true of certain plants in this connection is largely false of many animals. In the absence of any means of analysing living protoplasm, it is difficult to understand how the identity of the primal material of plants and animals can be positively asserted. If protist life in which the two principles tend to merge could be seen to develop into higher forms, there might be some foundation for the unitarian belief; but on the contrary we see, as before observed, that it does not emerge from its lowly state, and what it did at its origin is a subject of conjecture. It is not easy to concede that the plasmic constitution of a tree whose ancestors since the origin of higher plants have led a vegetal existence is the

counterpart of that of a being whose ancestors since the origin of higher animals have led the animal existence. We can hardly admit that without ignoring the importance of agreement and difference, the recognition of which, according to logicians, is the essential part of knowledge.

There are now in nature two definite phases of the life principle, and at the side of these an elementary indeterminate condition in which they merge. In the latter condition organisms are for the most part microscopic, difficult of analysis, and with few direct connecting links with higher life.

Evolution, if it has been operative in the world, has turned away from them. The fact that they resemble the cellular units of which the higher animals and, in a lesser degree, the higher plants consist, is no reason for offering them as proofs of unity, especially as they themselves are composite in character.

Although biology is concerned with both animal and vegetal life, there can be little doubt that its chief interest is with the former, which represents the human phase. At all events there are grounds for thinking that in the pursuit of this science the practice of attaching equal evidential value to examples drawn from both kingdoms is not likely to lead to accurate results.

Undoubtedly the same elementary substances are operative in both divisions to maintain life; but the manner and the form in which they are employed are different, and this difference is sufficient to render it inexpedient to regard the primal substance of which plants and animals consist as one and the same thing.

THE RELATIONS OF SPEECH TO HUMAN PROGRESS

By LOUIS ROBINSON, M.D.

WHILE we are of course quite sure that human speech once had a beginning it is very difficult to guess what that beginning was. We often get some indication of evolutionary history by observing the development of the embryo; but when we study the processes of vocal expression in human beings this method is of very little use, because the imitative faculty seems to account for most such manifestations of mental working in young children.

Did speech originally begin as a mere development of those stereotyped noises which practically take its place amongst most of the lower animals? Or did our ancestors have the capacity which we observe in so many birds, and in the young of our own species, of mimicking other sounds by the voice? In this direction we appear to get no aid from the study of our nearest relatives in the animal world. In a state of semi-domestication they appear ready to imitate our actions in some particulars, but as far as I have been able to learn this does not extend to vocal efforts at all. Indeed the anthropoids best known to us appear to be curiously silent beings whose vocal activity is very much less than that of many creatures far behind them in intelligence. One would think that creatures with such large and versatile brains as the chimpanzee and the other great apes, must have, in their natural state, some habitual method of intercommunication corresponding in some degree to their mental development. If this be so naturalists have altogether failed to discover it.

This inarticulateness certainly is an argument, when we consider what a vociferous being is man, against our near kinship with the great anthropoids. It is said, however, that among those humbler manlike apes, the gibbons, which in many ways seem so far removed from us, there is a far greater use

of varied vocal sounds in the wild state than is observable in
the gorillas, chimpanzees, and orangs. Hence possibly the
suggestions which have been made by comparative anatomists
that we must seek our forefathers rather in the direction of the
gibbons than among, or near, the greater apes, receive some
support from the study of the beginnings of articulate speech.

A very little imagination will show what an enormous
advance was made as soon as artificial verbal counters or
tokens were invented which enabled men to traffic in ideas
by means of the mouth and the ear. Among many non-
speaking creatures there is a system of vocal signalling which
meets most of their needs. Like ourselves many of them also
seem to have a good flow of small-talk, which advertises their
presence and serves certain social purposes, but conveys very
little meaning. Animal cries are for the most part mere
stereotyped signals for awaking the attention of the senses.
They are incapable of expansion or adaptation to give an
elaborate message. That they are effective is almost always
due to the exceedingly keen perceptions of most creatures
whose lives are constantly in peril, and not to any explanation
which they may convey of the exact state of affairs. The senses
of most lower animals are so much more acute than ours,
especially as regards scent, sight, and hearing, that on receiving
ever so small a hint they will get detailed information of the
approach of an enemy when it seems to the human watcher
that the only possible way in which such information could be
obtained must be through some detailed communication from
one of their fellows.

All animals and birds which are either gregarious or are in
the habit of associating habitually with other creatures have
a very alert sense of the behaviour of their comrades round
about them. Let one beast arrive in the herd panting and
frightened from near a neighbouring thicket where an enemy
might lurk, and all the rest do not need to ask a single question
before seeking safety. There can be no doubt that the con-
spicuous marking of many gregarious animals, such as for
instance the white tail in the deer and the rabbit, are specially
adapted for aid in this method of self-preservation.

Often a good deal of system and intelligence is shown in
giving and receiving warnings of this sort. Mr. Stewart White
has given a most amusing account of the behaviour of the

kongoni antelopes on the East African plains. These creatures seem, from a kind of natural officiousness, to have assumed the position of guardians over the zebras, gnus, and other antelopes which habitually graze with them. Not only does the kongoni mount himself upon an anthill to watch for danger—this is common enough—but evidently he is determined that, if any warning is given of the approach of a beast of prey, it should not be ignored by the other beasts which he has set himself to serve. He will attract their attention by various antics when he has a warning to give, and will even start to round them up almost like a sheep-dog if they should persist in ignoring his advice.

Obviously among forest-dwelling animals sight and scent must play a much less important part than in those who live out on the plains; hence we find that here vocal methods of intercommunication take precedence over such safeguards as those employed by the antelopes and their allies. One common habit resorted to among gregarious creatures, who are perforce concealed from one another while seeking food among the herbage, is that of making a continual subdued noise so that their kindred who are not far off can keep in touch with them. This is doubtless the explanation of the continuous automatic grunting of the pig, and the "small-talk" of many other birds and animals. It is certainly an interesting fact that the same widely distributed habit reaches to the lemurs—who are continually grunting; but whether it goes beyond them among the *Primates* proper I have not been able to ascertain.

Now useful as this instinctive or mechanical method shows itself to be, it is easy to see what an enormous stride would at once be made if, when the alarmed animal warned its fellows, it could so adapt its vocal message to a special case as to tell the exact nature of the danger, and the direction from which it might be expected. For instance, let the approaching enemy be either a tiger or a leopard. If the giver of the warning—who we will suppose to be a very primitive man approaching a band of his fellows—could clearly indicate that it was a tiger, obviously the climbing of any stout tree would suffice to procure safety for every one. But if the warning said "leopard" only an immediate flight to certain selected places of safety in the very tree-tops would give security.

As soon as the ground became the new theatre of man's

operations, and hunting by co-operative measures took the place of a solitary search for such vegetable nutriment as the forest afforded, it is plain that a power to express plainly what part each hunter was to play would be most essential. It should be remembered that man must have been for a long time an amateur, a mere blundering novice, rather than a finished professional, as are all the true beasts of prey. A pack of dogs or wolves manage to co-operate and follow the hints of the leaders with extraordinary success; but then they have been bred to the trade for innumerable generations, and their instincts sharpened in this particular to an extraordinary degree. Early man had to find some short cut in attaining the results which the carnivora attained as a result of an apprenticeship through whole epochs of time. His brain was amply sufficient, in all probability, for the task, but it was needful to find a method by which schemes and artifices springing from that already active brain could be communicated with accuracy to his partners in the enterprise. Here I do not think that any elaboration of those natural animal noises, which came to him, like his physical attributes, ready-made from a more brutish generation, would have gone far. But if he possessed a very little of the mimicking faculty and a fair vocal range, the beginnings of human speech become possible. As a matter of fact all languages give proof of the large use made of sounds which were originally the mimicking of the voices of nature.

The question as to the possible remnants still existing in our elaborate methods of speech of the original sounds and cries belonging to a pre-human existence is an intensely interesting one. That they still persist in some degree is fairly obvious in the form of certain semi-articulate exclamations common to practically all the peoples of the earth. What used to be known in our grammar books as *Interjections* are probably their fossil remnants, more or less modified by the pressure of superincumbent ages. The writer paid a good deal of attention to this subject some years ago when investigating the ancestral traits in very young children. Of course the "crowing" and scolding cries in young infants are of this character, as also are many of the "o" sounds of later life indicating distress, wonder, or surprise.

A complete catalogue of such vestigial pre-human parts of speech cannot be attempted here, but the subject is a very

fascinating one, and any anthropologist who could travel the world over and study vocal exclamations among various backward peoples, and the early sounds uttered by very young children before imitative speech was acquired, might, I think, make a good deal of it.

It is evident that *tone* has a great deal to do with the matter, and it seems probable that we have here a much more persistent relic of the pre-human stage of vocal communication than is found among actual words. Tone indicating emotion appeals to our *feelings*—which are primeval—far more than any mere words, and is at once understood, even by the lower animals. Indeed it is largely made use of throughout nature. By it such animals as dogs will give a greatly increased range of expression to a very limited collection of vocal sounds. Possibly the agglutinative languages such as Chinese, where tone plays such a large part, and the same identical word may mean a dozen different things in accordance with the tone in which it is uttered, bear more traces of the original pre-human "speech" than the languages of the western world.

One very obvious advantage of the beginning of true speech is the power it immediately gave of sharing and storing up experiences. Let us imagine our ancient and almost inarticulate forefather arriving at the common lair after an encounter with some wild beast from which he had escaped with difficulty. His scared look and blood-stained skin provoke cries of distress and wonder, and he is led—probably through the sympathetic curiosity of the "women"—to give some sort of a narrative of what has occurred. His words are very few. A growl, roar, or grunt, with a few characteristic movements, represent the specific beast that attacked him. Probably imitated sounds mostly stood for nouns in his "composition," and gestures took the place of verbs, while adjectives giving the degree of his pain and terror would be conveyed by a mimicry of his own animal cries of distress uttered at the time. The total result, however, would be that the young pre-human things sitting on their heels open-mouthed round about him, could not fail to learn, even from such a halting account of an adventure, a great deal that would be of service to them if they ever found themselves in a kindred plight.

From what we know of all the lower savages such narratives of the day's adventures are an almost invariable custom around

the camp-fire, and are not unfrequently repeated in the form of a chant or song. Such was probably the first beginning of every subsequent educational institution from the dame school to the Post-Graduate Course. The very fact that the deeds of the day are still often chanted in a kind of rhythm by the lower savages shows the purpose served by such narrations. Probably we may trace the beginning of all rhythmic utterance to a mnemonic system which prevailed through untold ages before the crudest writing was invented. This was the one way then possible of fixing and preserving experience for general future use. For such a purpose a fairly good vocabulary was needful, though doubtless at first such didactic recitations necessitated a good deal of acting or gesture. Even to this day there are said to be some low tribes in South America whose spoken language is so imperfect that they cannot converse in the dark.

If we learn anything from the relics of the stone ages it is that man dwelt in small separate communities and lived by hunting alone for a period a hundred times as long as that of which we have any historical record. At the end of this period, wonderful to relate, he appears to emerge from primeval darkness practically such a being as ourselves, with a truly human body and a great brain capable, if opportunity offered, of practically all the intellectual pursuits with which we busy ourselves at the present day! If there is anything in the evolutionary doctrine, this was all a product of the normal forces of his savage forest life.

Without a doubt throughout the whole of this period competition was keen between tribe and tribe and between individual and individual—and in every case it was a duel to the death. Many a race like the Neander men proved unfit, and went under in the struggle. Where small communities exist by hunting and fishing alone there is bound to be eternal friction leading to warfare about boundaries and game rights; so that even without any desire for scalps or heads, or tribal glory, or other provocatives of blood-lust only too evident to-day, we may assume that throughout the whole enormous period which preceded history the fateful struggle for existence between man and man and between tribe and tribe never failed or relaxed.

It is very easy to discern the enormous power which speech must have exercised in this struggle. Probably through no

other way were the brain capacities, already existing, made available to determine which tribe or individual should survive. The first man able to persuade others to act with him would at once be victor over a more brutish rival who lacked the vocal wherewithal; while a tribe which could take counsel together and form well-understood plans of action would easily overcome and exterminate its competitors whose powers of speech did not suffice for such an end.

There can be no doubt that throughout the whole course of the development of human speech the brain processes continually outran all powers of organic expression. Even to-day, however great be our knowledge of the contents of our dictionaries, and however cunning we may have become in arranging such material to the very best advantage, we are aware whenever we speak or write that we are translating our thoughts into a very imperfect medium. Although in our minds the conception may stand out with the utmost clearness we are often able to do no more than the artist who with a few suggestive lines leads the imagination to see the thing which he wishes to bring before us and does not attempt the task of representing it in all its photographic detail.

How the brain reached this wonderful power of clear internal expression long before there could have arisen any verbal traffic in ideas is at present a mystery wholly beyond us. It would seem as if there is spoken within each one of us *an unknown tongue* (yet for self-communings known far better than any spoken language) which defies full translation into any artificial assemblage of words. The same thing seems true of mathematical processes which man has laboriously endeavoured to translate into arbitrary symbols based originally, it would seem, upon the number of his fingers. It is a curious thought that if the first pen-dactylic thing of the carboniferous epoch had been differently constructed, if, for instance, his limbs terminated in a few more, or less, developments of the fin rays of his fishy forefathers, our whole world of mathematics would have been an utterly different one.

A little thought will show that in every movement of an animal, such for instance as a goat leaping from rock to rock, certain mathematical and physical problems are continually presenting themselves and being solved by the nervous and muscular mechanism. The exact force required by the muscles

to enable the beast to reach a certain pinnacle is estimated beforehand, and the proper orders given to the various muscles which come into play. Any mistake or miscalculation as to the weight to be moved, the direction of the movement, or the momentum to be reckoned with would often mean instant death. Now we cannot conceive any such mathematical process without certain standard units of value, but how our nervous systems work it out no one can say. Plainly such sums are continually in progress whenever we move, and must be, even in their simpler forms, infinitely more complex than anything attempted by our astronomers in reckoning and foretelling the movements of the heavenly bodies. The whole thing, whilst obvious as our own existence, is so bewildering and mysterious that the theological mazes in which the old School-men loved to lose themselves are mere child's play in comparison.

Our words at the best are a mere scratch pack of artificial noises gathered by hook and by crook from all sorts of sources during our progress from brute to man. The inward expressions that they lamely stand for we know within ourselves perfectly well, but can explain to others only a little better than the dumb things about us. Whether any other method will be ever found of tapping the wondrous mental reservoir by conduits less continually choked by our imperfections of expression one can only guess. Thought transference seems to offer the most promise, if it ever can be better understood and got under control. Should, however, such a consummation ever be reached it seems certain that we should be put *en rapport* with those fellow-creatures which we at present call dumb to an extent which it is difficult to conceive. For the " unknown tongue " is probably one and the same throughout nature. Here is a philosophic possibility which writers of stories such as *The Jungle Book* have often imagined, where the hero, generally a child, learns the language of the beasts and the birds and is able to foregather with them as one of themselves.

So much for a speculation which at present I fear is as profitless as it is fascinating—let us turn again to things more material and within our reach.

Human speech, whatever it was originally based upon, requires certain bodily machinery to give it utterance, and there are not wanting many perfectly clear and tangible evidences which, from the writer's point of view, show how the develop-

ment of speech has marched *pari passu* with human progress. Of the brain machinery involved in articulate speech we can never know much. We have learned that there is a kind of speech centre (or more probably a kind of nervous clearing house in the to-and-fro traffic of reflex action) in the third anterior frontal convolution at the left side of the average brain. The skull interiors of primitive men and apes have been diligently examined to see how they differ in this region, and guesses have been based on what has been found as to whether in this or that being articulate speech was possible. Personally I do not think this line of investigation is likely to lead us very far unless we get a much more accurate knowledge of how the brain works and where are the actual centres for the bewildering multitude of reflexes and other media of co-ordination which are brought into play when we talk.

Moreover, it must be remembered that speech is almost purely artificial, and is an exceedingly modern invention from an evolutionary point of view, and that it is working perforce through certain primeval mechanical media which existed before it began.

We are on much more solid ground when we come to deal with man's outward organs of speech, such as the larynx and the tongue. As regards the larynx I do not think that any very great changes can be pointed out in the way of structural elaborations which are due to our human needs. With the lips and tongue, however, it is very different, especially as regards the muscular attachments of the latter. The writer, after studying the subject for a good many years, has become firmly convinced that a muscle which appears to have been almost totally ignored by the anatomists, except as a mere protruderer and withdrawer of the tongue, is one of the most important factors in articulate speech. This is the *genio-glossus*, which takes its origin by a little tendon from a point inside our lower jawbone about half-way between the roots of the incisor teeth and the point of the chin.

This tendon almost immediately divides into a number of muscular fibres or bundles, which spread out like a fan from before backwards, and run up through the fleshy part of the tongue, from its root to its tip, until they terminate quite near the upper surface. Certain of the lower fibres go almost straight back from the lower jawbone to the hyoid bone which

lies between the tongue and the larynx, and for this reason the muscle is called by many anatomists the *genio-hyo-glossus.* When the tongue is at rest the front fibres of this muscle follow the outline of its under-part as seen from the front, and hence are concave forwards. The central and posterior fasciculi of this fan-like muscle are usually almost straight. The very fact of its spreading from its point of origin like an open fan shows that there is a widening interval between the composing bundles of muscular tissue as they pass to their place of insertion, which interval is filled up by loose connective tissue comparable to that which lies between contiguous muscles elsewhere, in order to allow free movement between the neighbouring parts. There are two of these muscles lying side by side separated by that gristly septum which divides our tongue into two almost distinct halves. One marked peculiarity of the muscle in man it may be as well to describe here. It gets its nerve supply from the ninth pair of cerebral nerves (the *hypo-glossal*) and each *fasciculus* receives a distinct branch, just as if it were a separate muscle.

Now it is plain that whatever the functions of the *genio-glossus* may be (and that they are very important is shown by its greatly increased size in man as compared with other animals) it requires considerable room beneath the tongue in which to exercise those functions. If we examine it in most of the lower animals we find it is merely a feeble slip of flesh lying in a position too cramped to be of any great service, since in dogs, cats, pigs, and most other quadrupeds the tongue lies in almost immediate contact with the inner surface of the jaw.

Now we come to some exceedingly curious and suggestive facts. In the apes this muscle begins to show signs of having important functions. These functions probably are to enable the tongue to move freely about the mouth for the purpose of sorting the food which is already there and rejecting such things as nutshells which are of no use to the animal. If we examine the lower jawbone of any ape we find that there is on its inner side a deep pit or hole specially to accommodate the *genio-glossus* muscle. Outwardly many of the apes, and especially the baboons, bear a considerable resemblance to dogs, but no one could possibly mistake the lower jawbone of a baboon for that of a dog.

Here we have a very remarkable difference of structure between ourselves and all our nearest relations in the animal world. In man the *genio-glossus* muscle springs from the top

of a bony prominence; in all the lower *Primates* it comes out of a pit. Moreover, in the apes it is found not only to be much smaller than in man—which is a sure sign that it meets certain specific human needs—but it is also obviously much less versatile, in that the separate fasciculi of the muscle are bound closely together. In several of the lower monkeys dissected by the writer no trace could be found of that curious splitting of the *hypo-glossal* nerve before it enters the muscle found in the human subject. Further information on this detail of comparative anatomy is very desirable.

Why should the *genio-glossus* muscle appear so much larger in man than in his nearest congeners the great apes? As far as the other, and especially the intrinsic, muscles of the tongue are concerned, I have not been able to discern very much difference between our tongues and those of gorillas and chimpanzees. It cannot be because we want to sort our food with our tongue to a greater degree than do the monkeys. We have no cheek pouches, which among many of the Old World apes form a kind of banking account, of which the tongue plays the part of the cashier. Man's intelligence, inventiveness, and versatile hands free the tongue from many of the discriminating duties it has to exercise lower down the scale.

It is only I think when we consider the functions of the *genio-glossus* muscle as an important aid in articulate speech that we are able to account for new facts. The mechanism of speech is exceedingly complex, and here it must suffice to discuss the part of it which refers more particularly to the question before us. When we speak at the rate of (let us say) 150 words a minute the number of separate tongue movements involved must come to nearly 500. These movements are following one another in ever-varying order, and most of them are composite, *i.e.* several groups of muscles are brought into action at practically the same time and must act in harmony with one another. Moreover, absolute precision in all these movements is necessary, and any failure results in a breakdown of clear articulation. Stammering is undoubtedly due to such failures of co-ordination, for any hitch in the exact timing of the muscle contractions (at the rate of nearly ten per second) causes a clashing of the forces brought into play comparable to the result of commutator troubles in internal combustion engines.

Now it is obvious that to achieve such feats the speech mechanism of the tongue must be simple and unhampered from an engineering standpoint. Let us examine briefly how the *genio-glossus* muscle acts when we articulate certain sounds. When we pronounce the letter T the tip of the tongue is placed against the front part of the palate by the contraction of the upper intrinsic fibres of the *lingualis superior*. In this position the front fasciculi of the fan-like *genio-glossus* are *drawn taut*, so that a simple shortening will instantaneously draw the tip of the tongue down. In pronouncing the hard G and K exactly the same thing takes place with the central bundles, while in the uttering of all vowel sounds and of others where the exact placing of the upper surface of the tongue against or near the palate is required, some or other of the bundles of fibres of the *genio-glossus* would be in a position to exercise exact control with the greatest possible mechanical advantage.

Now all anatomists are agreed that the different parts of the human *genio-glossus* muscle must act independently of one another, because the posterior fibres appear to thrust the tongue out while the anterior ones draw it in. The total action is described in some books of anatomy as that of lowering the central part of the tongue in the mouth as in the action of sucking, and it has been suggested that this is one of the important duties performed by the *genio-glossus*. The writer, by a series of dissections of the muscles in young animals and infants, soon became convinced that this view could not be supported, since in early life the *genio-glossus* is smaller in proportion to the rest of the tongue than it is later. Moreover, the act of sucking is common to all the mammalia, and certainly man is not commonly credited with any unique gifts in this direction.

When the *genio-glossus* muscle came out of a deep pit, as in the monkeys, and was " cabined, cribbed, confined " between the lower jaw of the under-surface of the tongue, it was impossible for the separate fasciculi to exercise the free movements requisite for articulate speech. Hence as soon as this new function was demanded we find that nature discarded the pit and designed another method of obtaining engine-room beneath the tongue.

. This was effected by a tilting forwards of the lower surface

of the under-jaw, and hence the characteristic chin which so distinguishes the human countenance.

By this radical change of structure (for it involved a complete departure from the fixed type of mandible common among all vertebrates) the muscle was at once set free and the separate fasciculi were enabled to act upon the under-surface of the tongue without being hampered by overcrowding. Even now mechanical perfection was not quite reached, for it is obvious that if the fan-like muscle sprang from a prominence the requisite independence of its component parts would be facilitated still more. This would necessitate still further room in what was originally the cramped space between the tongue and the inferior maxilla, which could only be obtained by a still further tilting forward of the lower margin of the bone.

Now when we come to examine by comparative methods the jawbones of apes, prehistoric men, primitive savages with imperfect articulate speech, and finally the more highly developed and civilised races the world over, we find indubitable evidence of such changes having taken place. The writer for many years has been collecting specimens or making plaster casts of this part of the jawbone, and a mere glance at the complete series demonstrates the facts with scarcely any further explanation. First there is the usual type of monkey's jaw with its deep pit, sometimes almost penetrating through to the anterior surface. Then among certain anthropoids where a decided tilted movement has begun, such as the chimpanzee and certain of the gibbons, the pit becomes shallower because it was no longer so much needed. In certain prehistoric jaws such as the Heidelberg and Naulette specimens the pit is still there, but has become shallower still. Among practically all the Bushmen, and many of the Central African Pygmies, Andamanese and Veddahs, there are still signs of the pit, but on the whole the surface is a flat one with only slight roughnesses upon it. In several interesting specimens of Hottentot jaws the *genioglossus* tubercles are seen as tiny prominences coming up from the lower side of the cavity, while in practically all the peoples of the earth who have adapted an elaborate form of articulate speech the whole inner surface of the jaw from above downwards is slightly convex, and in the centre of it are the genial prominences that are described in all current works on anatomy.

An examination of the development of this part in the young

shows that children possess no tubercles at all.   At about fourteen years old the European jaw almost exactly resembles that of the primitive races, while between fifteen and seventeen years of age the prominences assume their fully developed form.

A very interesting piece of evidence comes from the examination of deaf mutes.   The writer has had great difficulties in obtaining trustworthy information in this direction.   The one specimen in his possession of a French deaf mute of adult age seems to show that when speech is absent the tubercles do not develop at all, even in civilised races.   It is interesting, by the way, to note that the evidence seems to show that in French and Italian jaws, and also in Irish, there is a fuller and more uniform development in the genial tubercles than in the average specimens found in our English museums.   Possibly this may be because these peoples speak their language with a more painstaking articulation than is habitual in England.   The evidence tends to show that the tubercles are really not an inevitable part of us, but that they are in each case a sign of the activity of the muscle comparable to those rough ridges and lines found on the bones in all muscular subjects.   Such ridges and roughenings have already been used as pieces of historical evidence, for Rutimeyer in his researches among the remains of prehistoric lake dwellings of Central Europe professed to be able, by examining the bones, to differentiate between those of wild animals which had led an active existence and those of domestic animals which had lived a comparatively lazy life under man's protection.

Hence we possibly have in our genial tubercles an historic record of the extent of which we have made use of articulate speech.   Moreover it seems to the writer quite possible that a close and systematic examination of the arrangement of the varying tubercles (for they do vary in a very strange manner) in different races might give certain information as to the characters of the languages spoken.   We know how exceedingly different are the muscular requirements for different languages, since it is impossible, in many instances, for adults to so work their tongues as to articulate an acquired language with anything like correctness.

The ethnological part of the writer's collection of casts of jaws, although it contains specimens of nearly all families of the

human race, is nothing like complete enough for an inquiry of this kind; but it should be easy, considering the vast amount of material now available in our museums, for any one who has the time at his disposal to make a fairly complete comparative collection of such plaster casts. The process is very simple. The writer's practice has been to carry about him some pieces of wax, preferably the paraffin wax of which ordinary candles are made, which can be softened at a comparatively low temperature. A piece no bigger than a walnut suffices for the purpose of taking an impression of the part of the lower jaw involved. The whole proceeding takes but a few moments, and a permanent record is obtained which can be stored away and easily transformed into a plaster cast at any convenient time.

Incomplete as my material is it already demonstrates some interesting facts bearing upon the relations of articulate speech to human progress. There can be little doubt that the almost universal absence of the tubercles in the Bushman, and their exceedingly imperfect development among other primitive races which we know to speak languages which, from our European point of view, are very imperfect, tend to show that those prehistoric peoples which present a like peculiarity must have been far behind modern men in this respect.

There is a peculiarity about the Heidelberg and certain other prehistoric jaws which I have examined which it may be as well to draw attention to here, as it has already given rise to misunderstandings as to the value of the evidence from the genial tubercles. Beneath the prominence for the attachment for the *genio-glossus* muscle, and nearer the lower rim of the bone, are two smaller prominences which often take the form of slight rough ridges more or less united. These are found not only in man but in the apes and certain of the lower animals. They are the points of attachment for a muscular strip which has nothing to do with the tongue, called the *genio-hyoideus*, because it connects the chin with the hyoid bone. In the Heidelberg jaw there is a prominence representing this tubercle, but if the part above it is examined carefully the region occupied in our jaws by the prominent genial tubercles is represented by a decided depression. Most jaws of the Neanderthal or Spy type seem to indicate a state of development comparable to the Bushmen and Hottentots. The Piltdown jaw unfortunately is broken off at some distance from the symphisis, and hence this most interest-

ing relic is not able to offer evidence bearing upon our present inquiry.

The question as to whether the Piltdown " woman " and other very early men could talk, which has been discussed a good deal in the papers, seems to the writer of very little profit. We have only to go among some of the more backward races of the earth to find that methods of vocal communication sufficient for their needs are obtained by guttural noises, hisses, grunts, and clicks which involve very little use of the machinery for clear articulation employed among ourselves. An examination of the writer's collection shows, however, that wherever one has a race which has risen far enough for those complex social institutions to come into play which are the foundation of all civilised life and which involve storytelling and oratory, a prominent chin has become developed and the genial tubercles are well shown. It seems more than probable that such developments from a state of almost inarticulate savagery have gone on independently in various parts of the world.

It is scarcely necessary to dwell upon the influence of articulate speech on human progress after civilised methods of life had once been adopted. Among all the peoples of the world the capable speaker has won prominence and prosperity beyond his fellows, and hence would be one of the winners in the continual struggle which eliminated the unfit. Parliamentary institutions—using the term in its broadest sense—have left their mark upon the human countenance; for there seems good reason for supposing that not only the lower jaw, but also the nose and the cheek-bones (the hollow chambers of which have a great deal to do with the resonance and quality of the voice), have been shaped amid such evolutionary forces.

A good deal of the matter discussed in the present article seems to be practically virgin soil to the anthropologist, and the present writer is quite prepared to find that many of his pioneer efforts to get at the truth may be corrected when more capable investigators give earnest attention to the subject. It appears to him, however, a line of research of great promise, which may enable us to glean knowledge obtainable in no other way concerning the dark places of early human history.

RECENT ADVANCES IN OUR KNOW-
· LEDGE OF SYPHILIS

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THE origin of the name of the disease called syphilis is still a matter of dispute, and the genesis of the affection is unknown. I am informed by Mr. E. Bennet, Fellow of Hertford College, Oxford, that there is no definite mention of the disease in the classics: and this is the reason, probably, for the belief that syphilis did not begin until the Christian era had well advanced. Hippocrates, the Father of Medicine, does not mention it—even the *Aphorisms* contain no admonitions which certainly apply to venereal diseases; the heroes of the *Iliad* and *Æneid* were either blameless or fortunate; and neither Xenophon, Tacitus, nor even Cæsar himself give it a definite place in history. In *Priapeia et in Diversorum Lusus*, which contains the lewd stories of the Greek and Latin authors, including those of Ovid, syphilis is not described. But it has been suggested that the Biblical prophecy "The sins of the fathers shall be visited upon the children unto the third and fourth generation" refers to the disease; yet, if this is the case, the statement is inaccurate, for, as is well known, syphilis is transmitted from parents to children for one generation only. The religion of the ancient dynasties of Egypt seemed to centre round the worship of generation, as many of the monuments on the banks of the Nile show; yet venereal disease is not mentioned in the papyri nor in the inscriptions at Karnac, Thebes, Memphis, or Philæ. But Dr. Armand Ruffer, C.M.G., and Prof. Elliot Smith have recently examined a number of well-preserved mummies from the tombs of the kings, and the former has informed me that in some instances the bones showed changes which resemble those that are known to us now as being due to syphilis. It is commonly believed, however, that this affection, which is the source of an enormous premature mortality, produces great and lasting disability, is the frequent cause of idiocy, imbecility, and insanity, a predisposing

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cause of cancer, and the fount of great expense to the State, did not exist until the fifteenth century. And, until recently, it has been generally accepted that it is confined to human beings and that it originated among the soldiers engaged in the later crusades or among those who accompanied Columbus and Cortez in the conquest of America. Yet, probably, the reason why it is not mentioned in the classics is the same reason why it is not mentioned in our public literature to-day; syphilis is not described in our public print even now in the twentieth century, and as recently as July 1913 many of the London newspapers declined to publish a calmly and carefully worded appeal from the medical profession for an inquiry into the ravages of the affection owing, apparently, to an inborn dread of the public use of the word "syphilis."

The discovery by Pasteur of the capabilities of bacteria to cause disease and that of Ray Lankester of the powers of the parasitic blood protozoa in producting distinct maladies, induced a young research scholar named Klebs in 1897 (*Archiv. f. exper. Path.*) to suggest that syphilis was due to a microorganism which he supposed is transmitted from one person to another and from parents to children. But very little fruitful work was done on the subject for twenty years, owing to the insufficient methods of microscopy then in vogue, the results of research being ineffectual. In the meantime, Koch had discovered the bacillus of tuberculosis, Eberth and Gaffky that of typhoid fever, Kitasato that of plague and Hansen that of leprosy; and Laveran had found the protozoal blood-parasite of malaria, Lewis that of trypanosomiasis—discoveries which have led to the most important of practical results, namely, the prevention of disease. But, until the last decade, nothing certain or definite was known of the actual causative agent of syphilis; for, although much work was done and there were many conjectures and theories, nothing was proven and no hypothesis would bear critical examination. The methods of microscope examination were inefficient and faulty.

Early in the nineties, Louis Jenner invented his method of staining dead cells by a compound stain, and eight years later this method was improved upon by Romanowsky, whose method was again modified by Nocht, Leishman, and finally Giemsa. Then, in the years 1900-1, Losdorfer (*Wien Klin. Woch.* 1900) and Stassano (*Acad. des Sciences*, 1901) described peculiar

microscopic bodies in the humours of persons suffering from secondary syphilis. The objects they described were very indefinite, and their observations were not regarded very seriously. It is very difficult, even in the light of our present knowledge, to be certain that the objects they pictured are connected with those which we now know to be the cause of the disease.

It was not until the year 1905, when Giemsa's stain was better handled, that something definite appeared. A young German doctor named Siegel had begun work on the subject. He seems to have realised that there is some resemblance between syphilis and the affections known as the zymotic diseases—small-pox, vaccinia, scarlet fever, measles, etc.—inasmuch as they are all accompanied by skin-rashes, though they differ widely in many other respects. He remembered that Guanieri had, in 1892, described peculiar bodies in the cells taken from the vesicles in cases of small-pox and in pustules caused by vaccination—cell-inclusions, Guanieri called them, or *cytoryctes*. Siegel examined syphilitics and found cell-inclusions somewhat resembling those described by Guanieri in small-pox and in vaccine lymph ; they were found in cells taken from syphilitic ulcers. Siegel called these bodies *Cytoryctes lues*, to distinguish them from *Cytoryctes*

Some phases of *Cytoryctes lues* (Siegel).

variolæ and *vacciniæ* of Guanieri. Yet his method consisted largely of staining dead cells, and he had no means of improving his observations or of proving his interpretations. But, among the others, he described a form of his *Cytoryctes*, a many-tailed, free body which we know now as an appearance often taken by the parasite of syphilis, though he was unable to bring forward any evidence that the objects he saw were parasites at all.

Siegel's statements (*Abhandl. d. h. preuss. Akad. Wiss.* 1905) gave rise to considerable discussion at the time. Most scientists were opposed. Many said that the *Cytoryctes* were artefacts made by faulty technique, and that they were due to degenera-

tion of the cells which contained them—a time-honoured criticism against many cell-observations and often as unreasonable as the observations themselves. A few thought that "there might be something in it," while the majority awaited further developments.

Then the German Government sent a rising Berlin University Professor named Schaudinn to report on Siegel's work, which probably was but an elaboration of that done by Losdorfer and Stassano some years before and obviously based on the teaching of Guanieri. Schaudinn discredited Siegel's claim. But a few weeks later he published the existence, as a new factor, of minute, tailed, snake-like bodies found in syphilis; and these he claimed as his own discovery and stated were the cause of syphilis (*Arb. aus d. kaiser. Gesund*, 1905 and *Deut. med. Woch.* 1905). These objects, which, owing to their stained appearance and their capabilities of motion, he named *Spirochæta pallida*,

Spirochæta pallida as seen sometimes by dark-ground illumination.

very closely resembled part of those which Siegel had already described. But Schaudinn ignored Siegel's protests and explained the spirochæte as his own observation to Metchnikoff and Roux—the co-directors of the Pasteur Institute at Paris—who inoculated syphilis into apes and found the same snake-like bodies in the disease produced. Thus it was Metchnikoff and Roux who brought forward proof, and the world accepted Schaudinn's *Spirochæta pallida* as the causative agent of syphilis; Siegel was forgotten.

For four years, the scientific and medical profession examined the *Spirochæta pallida* in all its aspects. Its length was described, its breadth, its curls, its twists, the way in which it multiplies was pictured; and when facts were not forthcoming the imagination was drawn upon. Writers wrote about long forms, short forms, fat forms, thin forms, round forms, oblong forms, oval forms, dividing forms, double forms. It was described vividly how the spirochæte, with venomous malice aforethought, pricks cells with its tail and destroys them, how a single spirochæte could enter the human brain, remain quiescent there for twenty

LYMPHOCYTOZOON COBAYÆ

DESCRIPTION OF PLATE II.

FIG. 1.—A small extracellular amœboid form of the parasite as it occurs in the peritoneal fluid of guinea-pigs. FIG. 10 shows these to be amœboid as seen in the blood on the jelly : in fig. 11 one is stained by Giemsa's method.

FIGS. 2, 3.—The early included parasite found in the lymphocytes of the blood of guinea-pigs—the dot stage.

DEVELOPMENT OF THE FEMALE AND ASEXUAL ELEMENTS

FIG. 4.—Two parasites included within a lymphocyte of the peritoneal fluid of a guinea-pig—the chromatin dots have multiplied; one shows the next phase to fig. 3 in the formation of the female element, the other is an example of the rod formation (male element).

FIGS. 5, 6, 7, 8.—Other examples of the intracellular development of the asexual and female elements. The parasites grow and their chromatin dots increase in numbers.

FIG. 9.—The newly freed parasite as it sometimes appears in the peritoneal fluid of guinea-pigs; occasionally it breaks away from its host-cell before its development is complete.

FIGS. 10, 11.—The completely developed female and asexual elements. One is shown with a pseudopodium protruded, as frequently seen by the jelly method. The only apparent means of distinguishing between the female and asexual elements is to observe the acts of conjugation.

DEVELOPMENT OF THE MALE ELEMENT

FIG. 12.—The chromatin dot in fig. 3 becomes elongated into a dumb-bell, which splits longitudinally into two rods.

FIGS. 13, 14, 15.—The chromatin rods multiply by simple fission within the parasite inclusion in the lymphocytes of the blood of guinea-pigs.

FIG. 16.—Each rod develops a flagellum at each end. This figure shows one rod as seen within the cell-inclusion highly magnified

FIG. 17.—A parasite in a lymphocyte of the blood of guinea-pigs. It contains many rods and many flagella as seen stained by the jelly method.

FIG. 18.—From a central point in each rod longitudinal splitting takes place both ways along the length of the rod and each flagellum, until there is a maze of threads radiating from the central point wound up within the cell-inclusion.

FIG. 19.—A parasitic inclusion prematurely burst on the jelly. The chromatin of the microgametes (spirochætes) is stained ; the central chromatin hub and the spokes are developed from the rod.

FIG. 20.—The completely developed gametes as seen just before the cell-inclusion (the microgametocyte) bursts ; the nucleus of the lymphocyte is squeezed into a corner of the cell. From the blood of a guinea-pig.

FIG. 21.—A maze of gametes just born from a burst parasite, but caught in a clot and stained by the jelly method.

FIG. 22.—The male elements, the microgametes (spirochætes).

DEVELOPMENT OF THE CONJUGATED FORMS

FIG. 23.—Conjugation between male and female elements.

FIG. 24.—The chromatin of the conjugated form divides and subdivides, and the parasite becomes included within a lymphocyte of the blood, peritoneal fluid, etc., of the guinea-pig.

FIGS. 25, 26, 27, 28.—The growth of the conjugated parasite in the cytoplasm of the lymphocyte; this phase consists of a sphere containing a great number of small chromatin masses.

FIGS. 29, 30, 31, 32.—Budding. Each conjugated parasite gives off buds. Each bud contains chromatin, and on the jelly the process of their separation can be watched. Sometimes, as in fig. 32, the conjugated form buds within the host-cell and the buds can then be seen embedded in the cytoplasm. Each bud resembles the free amœboid forms as shown in fig. 1. Thus the cycles of schizogony and sporogony are complete. All figures are as seen on the jellies except fig 11, which is stained by Giemsa's method. The pictures are by Miss E. Barry, E. A. Ross, and J. W. Cropper.

PLATE II.

PARASITES IN GUINEA-PIG SYPHILIS

years, and then suddenly wake up from its long lethargy and cause general paralysis of the insane or locomotor ataxy; and the way mercury affected it and the way mercury did not affect it was pictured in consummate detail. There were discussions as to its true nature, and authorities became heated over the question of its bacterial or protozoal origin, forgetting that these adjectives are of human manufacture only. Then there were described with varying elaboration curious developmental, involution, or degeneration phases of the spirochæte; but there was not a tittle of proof brought forward in support of the statements made. Too often these writers seemed to forget that it is insufficient to describe "bodies" in the lesions of a disease in any one species of animal for them to be accepted as the causative agent of that disease. More evidence is required than the mere finding of "bodies." It is indeed doubtful whether Schaudinn's discovery would have been accepted had not Metchnikoff and Roux reproduced the disease in chimpanzees by inoculation and again found the same spirochætes in the lesions produced. Moreover, it is not sufficient to see different shaped bodies in a disease in any one species of animal and to weave them into a life-cycle. Inoculation experiments are always required. In other words, proof is necessary.

Since 1905, however, medical men have regarded *Spirochæta* (or *Treponema*) *pallida* as the causative agent of syphilis. When this organism is found in sores the disease is at once labelled syphilis; and Schaudinn has the credit, at present, of having discovered the nature of the disease. Yet, lately, there have been some authorities in science who have considered that the spirochæte is not in itself sufficient to account for the manifold manifestations of this malady. They remember that syphilis may remain latent in the human body for long periods and may then reappear in some part, which before was apparently un-affected, years after the disease is seemingly cured. Such thinkers—a small minority—have found it difficult to accept that this organism unchanged can alone cause the varied sores of syphilis, the multiform rashes which "imitate all and originate none"; can remain quiet in the body and can then cause the conditions known as the parasyphilitic affections—general paralysis and locomotor ataxy—years after the disease first appeared; and they find it hard to believe that the *Spirochæta pallida* can by itself cause primary, secondary, and tertiary

syphilis, idiocy, insanity, and hereditary locomotor ataxy. But, except for a few malcontents, the world has accepted that the nature of the disease is known in its entirety.

In the meanwhile, the means of diagnosis and the methods of treatment of the disease have greatly improved. The Wasserman reaction has proved to be a means whereby the existence of the disease can be recognised even after all symptoms have disappeared, and when an immunity has been established. But it is not operative until the disease has progressed, though its value in later diagnosis and in controlling treatment is vast. The treatment of the disease, too, has made a great advance in Ehrlich's discovery of salvarsan, or " 606." It is the outcome of an evolution of knowledge. The history of the treatment of syphilis would fill the pages of a profoundly interesting book. It is not known who first noted the curative powers of mercury, arsenic, antimony, and their compounds in this disease ; nor is it known when the discovery was made. It ranks with that of the effects of quinine and arsenic on the parasite of malaria. Both discoveries were blind shots in the dark which hit the mark. For more than a century, compounds of mercury have been administered in syphilis and the disease cured by them. In the Early Victorian age it became fashionable to give it commonly to children as a cure for all trivial ailments, and mercurial stomatitis and teeth disorders were frequent. But until the last few years the slow method of treating syphilis by mercury, a treatment extending over a period of two years or more in every case, followed by a year's dosing with iodide of potassium, was the rule. Then the known effects of arsenic on the protozoal parasite of sleeping sickness led to pharmacological research and the production of a complex compound of that metal was the result ; it was named Atoxyl, and was tried on animals infected with trypanosomes (the cause of sleeping sickness) ; for it was realised that a drug more rapid in its action was required for those affections which kill more rapidly than syphilis. Step by step these compounds were improved upon until at last Ehrlich found his salvarsan. Perhaps its effects are not all that were claimed at first, but it signals the beginning of a great advance in the treatment of syphilis, for it rapidly curtails the more obvious symptoms of the disease, and, when combined with mercury, leads to a quicker cure than was formerly possible.

But prevention is better than cure. So far as medicine is concerned, the opening of the twentieth century will be recorded in history probably as the beginning of the era of disease-prevention. It was Edward Jenner who pointed out the path a hundred years ago. He found that the inoculation of cow-pox into human beings modified and prevented small-pox; and to-day small-pox does not exist in civilised communities. This is advance indeed, and the beginning of the present century has given us the application of his teaching—malaria, yellow fever, tuberculosis, Malta fever, dengue, are being prevented wholesale, and prevention is replacing the old retail method of individual cures. We are learning to regard disease as an armed enemy standing on the threshold of an unarmed homestead; we must find a means of shutting the door in his face rather than try to attack him when inside. It was with this object in view that in July 1911 Mr. McFadden instituted researches at the Lister Institute of Preventive Medicine into the causation and prevention of certain of the zymotic diseases; he suggested those which produce the greatest death-rate—measles and scarlet fever. His object was to find out a means of preventing them. These researches have resulted in advancing our knowledge, not only of scarlet fever and measles, but also of syphilis.

A start was made with the examination of the blood of cases of acute scarlet fever and measles. For this the newly invented "jelly method" of staining living cells was employed. The jelly method is a considerable improvement on the older techniques by which dead cells distorted by alcohol and other fixatives were examined; it is better than the dark-ground illumination, which only shows the shadows of living things. It consists in placing living cells on a soft jelly where they can be watched under the microscope; they are spread out gently, remain alive for hours, and their component parts are made to stain slowly. Thus their action can be observed and the presence of parasites detected better than by any other known method. The jelly method showed peculiar inclusions within the large mononuclear cells of the blood in all cases of scarlet fever and measles during the acute febrile stages of those diseases.

But, as it was found difficult to ascertain the exact nature of these intracellular bodies (the same difficulty which Siegel had to face) which stain in a peculiar manner by the jelly method, a

halt was made until some similar but simpler cell-inclusions had been examined, and observations were made on the blood of the lower animals; for the finding of bodies is not sufficient in itself to label them as the parasitic causes of disease, even when their appearance is constant.

The discovery of somewhat similar cell-inclusions in the mononuclear cells of the blood of guinea-pigs, and which are known as Kurloff-Demel bodies, led to the establishment of a new genus of parasite called the Lymphocytozoa, because these bodies were found (by the jelly method) to develop into spirochætes. These intracellular parasites stain in a very remarkable manner on the jellies, and the development of their nuclear material, even while within the substance of the cells, can be observed very accurately. It was found that they pass through a certain, constant, definite development into spirochætes while within the cells. It was also found that the affected guinea-pigs frequently show signs of disease which are similar to those seen in syphilis; and the spirochætes were discovered free in the blood of these animals. The parasitic nature of the cell-inclusions therefore was evident.

Then J. W. Cropper of the McFadden Researches discovered similar cell-inclusions in the male generative organs of earth-worms; these also were shown by him to develop into spirochætes and to pass through the same phases of development as the guinea-pig parasite. This train of information was so suggestive that a thorough examination of cases of human syphilis was undertaken to see if similar intracellular parasites were present in that disease also, which resulted in the finding of them in every one of five hundred cases of syphilis examined. They have been seen in all the lesions of the disease—those of the primary, secondary, and tertiary stages; and they have been demonstrated by the jelly method to develop into spirochætes which resemble the *Spirochæta pallida*. But it was noted also that there were other forms of the parasite, namely, free amœboid bodies, and various other phases were recognised in the circulating blood of syphilitics; this led to the suggestion that the spirochætes really represented *gametes*, or male elements of a large and complex parasite. Phases which may well be the female elements have been seen in both guinea-pigs and in human beings, and conjugation has been observed in the former; but this aspect of the question remains for the present

LYMPHOCYTOZOON PALLIDUM

DESCRIPTION OF PLATE III.

FIG. 1.—Free amœboid forms found by the jelly method of chancres, glands, condylomata, and sores of syphilitics. Each contains chromatin.

FIG. 2. —A small amœba included within the cytoplasm of a lymphocyte.

FIG. 3.—The chromatin of the cell-inclusion becomes surrounded by a definite cell-wall, and divides into three circular masses, each containing a central dot.

FIG. 4.—Two parasites included in a lymphocyte. One possesses three deeply staining dots, the other a dot and a rod ; the former is an early stage of the development of the female and asexual form, the latter is an early phase of the development of the male form.

DEVELOPMENT OF THE FEMALE AND ASEXUAL ELEMENTS

FIG. 5.—This cell, from a chancre, contains two female and asexual parasites. In one the chromatin is in the act of division ; in the other it has already divided.

FIG. 6.—A parasite found in a lymphocyte of the blood squeezed from a syphilitic papule. Within the parasite there are eleven separate chromatin masses, one of which is in the act of dividing. Each chromatin mass contains a deeply staining dot—a feature of these parasites.

FIGS. 7, 8, 9, 10 —Free amœboid forms derived from the bursting of forms like fig. 6. On the jelly they are highly amœboid. Each contains a nucleus and granules, some contain vacuoles, and all have a central intranuclear dot. These represent, according to their close analogy to *Lymphocytozoon cobaya*, the female and asexual forms.

DEVELOPMENT OF THE MALE ELEMENTS

FIG. 11.—The chromatin rod and dot in fig. 4. have multiplied within the cell-inclusion into three rods. The cell is from a chancre.

FIG. 12.—A mononuclear cell from a chancre containing three parasites. Two show the formation of the microgametes within the microgametocyte ; the third is an early phase. The pink-coloured sausage-shaped body is probably a diffusion vacuole ; it contains no structure.

FIG. 13.—An epithelial cell from a syphilitic gland. It contains a large parasite in which there is a bunch of microgametes. These are radiating from a common centre or hub like the spokes of a wheel. Some of these gametes have the same optical appearance as *Spirochæta pallida*, but only the chromatin of the spirochætes is stained. From the analogy of *Lymphocytozoon cobaya* the hub is formed from the rod pictured in fig. 11.

FIG. 14.—A large mononuclear cell found in the finger-blood of a case of secondary (macular) syphilis. It shows the mode of the formation of the microgametes within the microgametocyte. The lymphocyte was alive on the jelly, as is demonstrated by its pseudopodia, and the parasitic inclusion burst while under examination ; the spirochætes were ejected into the plasma. Note how the nucleus of the host cell is squeezed.

DEVELOPMENT OF THE CONJUGATED FORMS

FIGS. 15, 16, 17, 18.—Copper-coloured bodies found in the chancres, glands, sores, and peripheral blood of syphilitics. They contain numbers of deeply staining granules. By analogy with *Lymphocytozoon cobaya* these represent the conjugated forms, but they are generally found free and not included within the cells.

FIG. 19.—A cell-inclusion, stained by Leishman's stain, from a syphilitic gland found by Colonel Jennings. The chromatin granules are very similar to those of the conjugated forms of *Lymphocytozoon cobaya*. If, as is the case of the guinea-pig parasite, these give rise to buds containing the granules, the cycles of schizogony and sporogony of this parasite are complete. With the exception of fig. 19, all the drawings are as seen on the jellies. They are by E. H. Ross.

The parasites seen in rabbit syphilis (*Lymphocytozoon leporis*) are smaller than the above, otherwise they are identical.

PLATE III
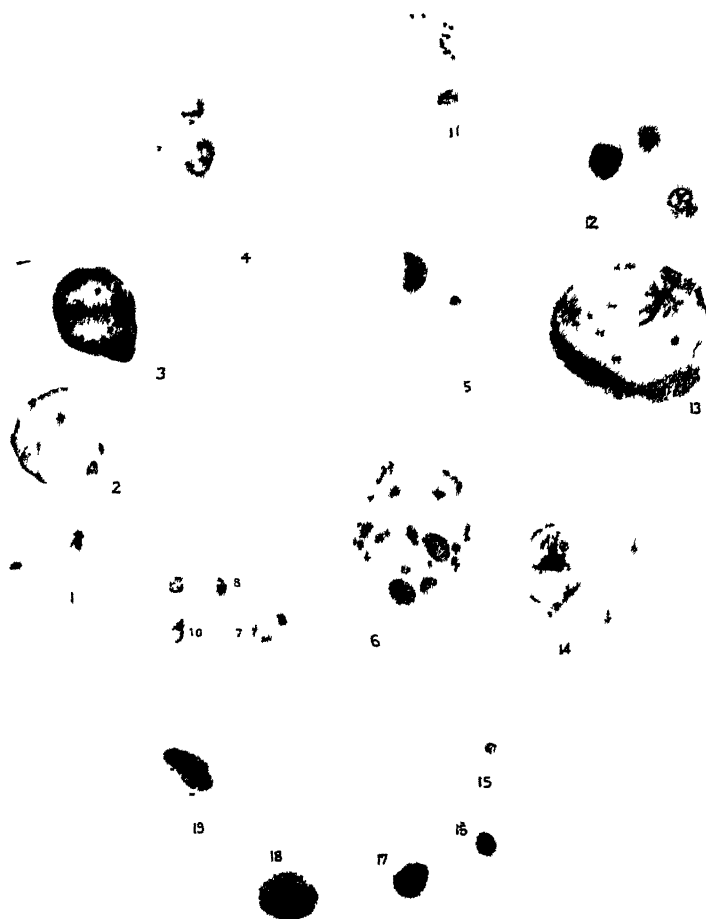
THE PARASITES IN HUMAN AND RABBIT SYPHILIS

unsettled, though the evidence in favour of the theory is very strong.

Yet, although the spirochætes seen developing within the cells of syphilitics very closely resembled the *Spirochæta pallida* in appearance, and although there was the evidence of the disease in guinea-pigs, and the corollary of the similar intra-cellular parasite developing into spirochætes in these animals also, proof by inoculation was wanting; for it is impossible to experiment with syphilis in human beings, and the manifesta-tions of the guinea-pig disease are inconveniently confined to the blood-cells and internal organs. Nevertheless, these inocu-lation experiments soon became possible, and in a very curious way. I was told by Lord Kimberley that the wild rabbits and hares in the county of Norfolk were suffering from a disease named by gamekeepers "rabbit-pox." Very soon afterwards, a paragraph appeared in *Country Life* (October 6, 1912) in which it was stated that the wild rabbits on the east coast of Scotland were infected with a peculiar disease. Some of these infected animals were obtained and the disease examined at the Lister Institute. It was soon found by me that these rabbits had a naturally contracted affection similar to human syphilis, though probably not identical with it. And examination showed the presence of similar but smaller intracellular parasites like those which had been already seen in cases of human syphilis, in the guinea-pig disease, and in earthworms. These animals were watched, and the progress of this affection observed. It coin-cided with the progress of syphilis in human beings, except that it is more severe under the natural conditions of rabbit-life, and the animals frequently die when uncared for. Some were treated with salvarsan and mercury, and improvement began at once; several infected animals have now been apparently cured.

But, as stated before, it was necessary to prove the deduction that these intracellular parasites are the real causative agents of these diseases. Therefore, a young healthy rabbit was inocu-lated from a diseased rabbit. The inoculation was accomplished by scratching with the point of a contaminated sewing-needle, as a calf-lymph vaccination is performed. In twenty-five days a small sore appeared at the seat of inoculation, and the disease began. In this sore the same intracellular parasites were found, and, finally, free swimming spirochætes, which appear to be exactly similar to the *Spirochæta pallida*, were seen by

Bayon, Noguchi, Martin, the writer, and others (see the *British Medical Journal*, November 1, 1913, p. 1159).

Therefore, the train of evidence that these intracellular parasites which develop into spirochætes are the causative agents of syphilis is complete, because similar parasites which develop into spirochætes have been found in several species of animals, accompanied by diseases in those animals which resemble syphilis; and inoculation experiments based on this belief have been successful, for the disease and the same parasites have been reproduced artificially; spirochætes are found always accompanying the intracellular parasites in all the lesions in all the animals. But this belief has received still further proof. Noguchi has succeeded in cultivating several of the spirochætes, including *Spirochæta pallida*, in test-tubes. But he has found that the latter will only grow in the presence of living tissue cells. Some of these cultures were obtained (they were subcultures taken from those sent from the Rockefeller Institute, New York), and in the living cells the intracellular parasites were found. Noguchi himself noticed some peculiar bodies which are very similar to those seen by the jelly method in human, guinea-pig, and rabbit syphilis. These he described at a recent meeting of the Royal Society of Medicine.

The discovery of syphilis—or a disease closely allied to it—in the lower animals heralds a most important advance in our knowledge of the disease. It throws a new light on its origin. For, although the parasites of human, rabbit, and guinea-pig syphilis differ slightly from each other, the difference is no greater than the difference between the animals which contain them. From their appearance one is struck by the probability that they were derived from the same original source. The old idea which placed the origin of syphilis in Divine wrath is no longer tenable, for surely rabbits have incurred no such displeasure; nor did rabbits or guinea-pigs play any part in the Crusades, nor in the conquest of America. It seems more likely that syphilis has existed in the human race as long as that race has existed, and even longer; and that perhaps it has taken its place in the evolution of the animal kingdom, and with the evolution of the species has come the evolution of their parasites and their diseases. The reason why the ancients did not mention syphilis is probably the reason why we do not mention it now—we are ashamed of it.

The finding of syphilis in rabbits has opened up a new road for research which should lead to the prevention of the disease. It may be possible to apply Edward Jenner's discovery of the means of preventing small-pox to syphilis. He inoculated human beings with cow-pox, and this modified and prevented small-pox. The parasites of small-pox, cow-pox, human and animal syphilis, seem to belong to the same family; and therefore it appears reasonable to suppose that rabbit syphilis, if vaccinated into human beings, would modify or prevent the human disease. If this proves to be the case, there will be an enormous saving of health and money. In our naval and military forces alone an immense boon would be gained, and in the civil population idiocy and insanity, with the expenses they incur, would be enormously reduced. There will be difficulties to encounter, but with patience and careful experiment these should be overcome in time. Experiments to this end with monkeys are being instituted forthwith.

Such a method of preventing syphilis appears to hold out the best hope of solving the problem. Up to the present time other methods have proved most unsatisfactory.

Attempts to prevent disease by treatment are not generally efficacious either. There is the example of the old attempts to prevent malaria by enforcing the administration of quinine. At Ismailia, in the old days before mosquito reduction, the fever continued notwithstanding the quinine. People will not do it. So in the British Navy and in the Army attempts to enforce the hospital treatment of syphilitics have not been entirely successful. I can remember how the infected sailors were sent to hospital and treated until their obvious symptoms disappeared. Then they returned to their ships, and, although under nominal observation, they continued to spread the disease as soon as they were given general leave, for syphilis remains infective for two years or more. The Army during the South African War was the same.

Without doubt some form of protective vaccine such as I have suggested holds out the best hope of prevention. But there is one important factor which must be grasped. So long as the name of syphilis is hidden in a halo of hush, so long as all research into the problem is fettered by the shackles of silence, the greater will the difficulties be. If only the public generally could be taught the facts of the disease, if old-world

prejudices could be laid on one side, and if every one could realise that this disease is curable easily if taken at once, an enormous step would be gained. Let us preach prevention, or, failing this, cure. All important health-work requires publicity for its accomplishment. Freedom of publicity and the right of sincere discussion are essential. It is time the public grasped the true nature of this disease. Silence can do no good. We cannot prevent mosquitoes and abolish malaria and yellow fever by remaining silent. So it is with syphilis. Surely every one should be taught the dangers of life? At present each man tells himself that it will not be his lot until he is stricken—then he is dumb. Let us bring the whole matter to light, place our cards upon the table face uppermost, and examine critically our position. And, finally, let us remember the last words of Pasteur, "Il faut travailler."

WHY ARE PEOPLE SO CONFINED, WHEN FREEDOM CAN BE ENJOYED

By Mr. T. BROWNBRIDGE
North Shields

WHY are people so confined when freedom can be had in the open where the air is pure. The lifes of men women and children are over estimated by the sulphur and smoke that surrounds them. The Universe and the Natures of the Universe desire every individual life to attend to the Natures that are in want of Maturity of their Natures, as every life is deffecient of that purity Nature offers. Every life of the human, animal, and vegetable kind is suffering for for the want of purity where sulphurs and smoke are. The world desires purity as a right in all its spheres. The extensions of waves of sulphur and smoke are wavering the Atmospheres lifes of molecules, bacteria, and germs, and these Atoms are our lifes and to injure them hinders our own progression. To fill a life with contaminated atoms is to destroy any life of the above mentioned spheres.

Are we not getting further further away from purity? While the earth is raidiatively acting the worlds life there is shooting out volumes of poisonous sulphurs and smoke out and up into the Atmospheres from furnaces and chimneys to destroy lifes of the invisible to the naked eye. Also visible lifes.

Mankind is truly making a trap for himself to shorten the worlds life, and there is increasing sulphur and smoke as lifes increase, to be insulted by being enforced into unnatural works. And more increasing of Armaments causes more lifes to suffer, who work at the Shot and Shell. Also the manufacturing of the Guns, and Ships, and Armants.

The solidarity of these plans of works, of all sources, as shell, steel shipping causes Vibrative of the Atmospheres to be attracted from their naturalism as forces of Units acting for the good of lifes of our World, of all sources.

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[1] We make no apology for printing this utterance of a voice from the smoke.—EDITOR.

The lifes of these Atomic Units are despondent through the advantage took of their Natures, and are rebellious against the different lifes in which they represent as natural forces of Nature. Although invisible they must be allowed to act their part as pure as the Atmospheres allows, but it is the duty of Mankind to protect the Atmospheres in every environment.

No Sulphur, and no Smoke should never have been allowed to contaminate the Atmospheres.

But I think I hear the words how are we to do this. The actions are now done and cannot be undone.

But I maintain that the working condittions of our World can and must be altered if we are to prolong our Worlds life.

The vacant earth will supply our needs, the beautiful surface of the earth so neglected whilst we are poisoning the Atmosphers and shortening our lifes of the World we live by. We are suffering by our units, suffering by the Sulphur, and Smoke.

And our attributes as Attoms are to be higher up in the Altitudes instead of being nearer to us. They are the life of our being and are our existence but we are robbed from them doing us good.

And without we learn how to exist Naturally in pure Atmopheres we shall be ignorant and defective. and deffecient of pure life.

Time is arriving when man will suggest but will not fullfill or even try to help to prolong his World. But it must come to pass that there is a respdnsibility rests on the consciousness of some-one. But do we not hold a responseability individually, since the Area of the World up to its present position ?

Our World's position and its life are not so rife with stability and endurance as of yore. The connections of our World is weakening through our weaknesses, and without support of every individual life of the World, the weakining must increase as the supports of our World, because of the weakening Connections of the Worlds life. And, stratus, that is in communication with our Our Atoms and all lifes between the Stratus and the Atoms must either increase purity or impurity. but impurity is on the increase through the Contamination of the invisible agents of the Atmosphere and Stratus.

We must allow our agencies to be pure, as it is of the greatest necessity to be pure ourselfs. In the vicinities of Sulphur

and Smoke it is most disstracting to these lifes of minute forces, of our lifes. These germs are most welcome in their Atomic state but they must be encouraged purity by keeping the Atmospheres pure.

Although we have to take them as they come through difficulties of weaknesses through the poisonous sulphur and smoke that so corrupts the pure Atmospheres.

We are to live with, and by them, as we inhale them into our bodies, they are our lifes, and we are theirs and we must encourage them to purity, if not something is wrong with us as they through the impurity of the Atmospheres, and we must encourage a life for a life, as the lifes of the invisible forces so encourages us, more so when kept in a pure state.

We are brought to great difficulties and even stagnation of the body in environments where impure Atmospheres exists, and the suffering ones in lead works and other works whereever ores Stratus are extracted from their beds of devolopment and experiments upon and manufactured by the workers in the different particular spheres of life where the different units of Atoms are put asunder through the ignorance of man.

Then the Atoms of his life cannot get near him, and his life is awaiting his attributes, Atoms are waiting to be near their parent.

These Atoms have derived their existence by inalations by breathing in from pure Atmospheres and passed out of the ' being ' by passages after they have passed through the system, ' they our Atoms ' Naturally and instinctively claim us as their parent and we have to give our lifes for them, as we increase them as long as we breathe either by impure auras, or pure auras.

Now the time is with us to speak out and express our selfs as we know the health is not with our world and its lifes. The lifes of all are so weakened by the weakening of the World, by taking away the stratus, the structures and relaxing the invisible powers that upholds them. These invisible forces and units of the stratus are in conjunction with our units. ' Atoms ' and these correspondencies are by their Natures, Natural Conscious lifes of their own spheres, assisting our lifes to maturity.

Now how are we to act to Justify these minute lifes, of our lifes, to be constantly with us. for all lifes, for all time. They must have pure Atmospheres to give us the pure Quickening

help. The lifes of these Conscious minute forces are wronged by the wronging of the stratus and the Atmospheres and to make life complete, the stratus the life and body, of the bowels of our World must be left to mature, as the Worlds individual lifes and and their Attributes.

The surface of the earth will accept all life, to labour and live, to mature by having plenty of fruits of the earth and Natural labour, to help our Attributes to help us.

THE PROTECTION OF SCIENCE BY PATENT

BY AN AUTHORITY ON PATENT LAW

IN *The Times* of February 13, 1913, a letter from Sir Ronald Ross appeared concerning the Patents Act and Medical Research. In the course of the letter it was shown how the present Patents Act excluded certain scientific workers from the benefit of the protection which is given to other inventors, and the opinion was expressed that the time had arrived for reform in the British method of dealing with science.

The subject of *The Times* letter deserves more than the passing reference to which the columns of a great newspaper necessarily confined it, while its importance to workers in the higher branches of science is so great that detailed examination of the complaint against existing conditions is desirable, while a discussion of the means which may be proposed for removing the disabilities under which scientists labour may assist in the removal of the disabilities in question.

There is scarcely a department of life but has been influenced by the researches of the scientist, researches which do not necessarily result in manufactures. His operations touch us on every hand, while his labour is fraught with momentous consequences. In the realm of electricity, such matters as telegraphy, telephony, and the transmission of power are directly referable to his discoveries, while in the chemical industry, the development of dyes, the production of alkali, and the formation of sulphuric acid are immediately attributable to his foresight. And what is to be said concerning discoveries of bacteria whereby soil may be enriched, and concerning astronomical investigation, which enables navigation to be more safely pursued? What of the discoveries of Pasteur and his followers as bearing on the preservation of food? And what of medical investigations which resulted in vaccination and those which, eliminating yellow fever and other ailments, have secured the cutting of the Panama Canal?

Yet however highly meritorious or beneficial to the State or

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to the world at large the practical applications of the scientist's researches may be, unless a manufacture which exhibits inventive ingenuity over and above the merit of his discovery is the result, no patent protection is obtainable by him. On the ground of fairness alone some variation of the existing method of distributing recompense is urgently called for; and when in addition it is remembered that the chief justification of an elaborate patent system is the stimulating effect of the hope of the reward held out to those who create an enterprise beneficial to the community, the demand for extension of the patent law appears irresistible.

At the present time, if an invention or discovery is to receive the protection of a patent, it must result in what is styled in the Statute of Monopolies of the time of James I. a " new manufacture." The invention must be new and it must also be a manufacture. The meaning of each of these terms has many times been expounded by the judiciary, so that their application is clear, and as *The Times* correspondent has pointed out, newness or novelty of an invention has been interpreted so as to preclude, in particular instances, highly deserving discoverers from the benefits of the patent law. If by any chance an inventor has published his invention before the date upon which he has applied for a patent, no patent which could withstand the ordeal of the Courts is obtainable. Thus, in the case referred to in *The Times* letter, Mr. X. had for years been engaged on certain research work, with the result that pernicious samples of a natural product could be distinguished from those which were innocuous and the illness of the workman engaged in converting the natural product into serviceable form consequently minimised. Since, however, the research had extended over so long a period, Mr. X., before the date of his application for a patent, had published his discovery, " an absolutely necessary procedure for genuine scientific work." Consequently, on the ground of want of newness in his invention, this research-worker was denied the reward of patent protection. The details of his practical method of eliminating the deleterious element of the natural product might have been patented, provided those details were new and exhibited what has been termed inventive ingenuity; but in such a case, as Sir Ronald Ross points out, a rule of medical ethics would forbid. As a result, although workmen, employers, and the State would probably derive

great advantage, the originator of the discovery could obtain no such benefit as he might hope to have obtained from the granting of a patent.

Apart from the particular example, there is a further reason why a discoverer, however meritorious he may be, cannot become a patentee. The Statute of Monopolies, as already alluded to, restricts protection to a "manufacture," and although the word manufacture has gradually been moulded by the judges so as to include manufacturing operations, processes and articles, it has not been held to cover the very highly ingenious, original, and meritorious operations of the purely scientific man which do not result in a manufacture. When we approach the matter more closely with the view to ascertaining what practical steps ought to be taken to remedy the grievances in question, we find two distinct issues in connection with the present system of granting patent protection, viz. :

(1) Whether the originator of a scientific discovery by communicating the results of his research to a learned society ought thereby to lose the right to apply subsequently for a patent; and

(2) Whether the protection awarded to new manufactures ought not to be extended to other applications of scientific discoveries which may be of utility to the public.

The question touched upon in *The Times* whether medical etiquette should so far be relaxed as to permit a practitioner to obtain a patent concerns the medical profession alone and falls outside the present inquiry.

(1) *Communications to Learned Societies.*—As previously stated, if an individual before the date of applying for a patent communicates his invention to the public or to any section of it, the invention is henceforth devoid of the element of novelty which the law demands in an invention which is to be protected. It is immaterial whether the invention is published piece-meal or at a stroke. Provided a divulgence takes place in any way whatever, the invention is no longer new in the eye of patent law and, with one or two exceptions which need not be entered upon, is incapable of being protected by a patent grant. The scientific investigator who reads a paper or series of papers before a learned society, for instance, and gives an account of his discoveries will be precluded from receiving a patent which is unexceptionable. Not only is this the case, but he is even denied the exclusive enjoyment of obvious novel applications of

his discovery.   These are open to others equally with himself, while as regards those applications which are not obvious, any one who can exercise inventive ingenuity may obtain protection for the exhibition of this ingenuity whether he is or is not the originator of the basic idea.   No matter what may be the amount of invention present, whether large or small, which may be involved in furthering the original discovery, provided invention can be proved, the originator of the foundation discovery is refused the right to use the subsequent invention without permission of its patentee, and this although the originator could easily have produced the invention had he known what was required.

Inroad into the sacrosanct requirement of novelty in a patented invention has already been effected ; for the Patents Act, which now governs the grantings of patents, stipulates that a patent shall not be rendered invalid by a prior publication which is made without the consent of the inventor, if the inventor applies quickly for a patent after learning of the unauthorised publication.

Here then nefarious publication is not prejudicial to the inventor ; but what is to be said of commendable publication by the inventor himself before he decides to exclude the public from the free use of the result of his researches ?   Surely, the right to receive protection after an invention has been published by one who is not the inventor ought to be conceded to him who, being the inventor, meritoriously publishes his invention.   It can be no great step to accord him a similar measure of redress when, say, before the Royal Society, he himself has promulgated the result of his researches before having lodged his application for a patent.   But the principle of granting protection as against publication by the inventor, as opposed to the publication by a stranger, has already been affirmed.   By a series of Patents Acts spread over the last sixty years, the publication of an invention at selected exhibitions does not prejudice the inventor against applying for and receiving a grant at a subsequent date, a grant which otherwise would be invalid on the score of want of novelty.   In this instance, the publication is not unauthorised by the inventor as in the other case where the legislature has protected him, but is the direct result of his own action and desire.

We see then two exceptions to the rule that the publication of an invention prior to the date of the application for a patent

is fatal to the validity of the patent which may be issued as a consequence of the subsequent application. The first exception is where publication is unauthorised, and the second where the publication is due directly to the inventor. The suggested reform, which would permit an inventor who had published the result of his investigations before a learned society to apply at a later date for a patent and receive a valid grant, would not only do no violence to the law as it now stands, but would be the natural complement to the steps which have already been taken. This demand for alteration of the law is, however, no new thing. Prof. Sylvanus Thompson, F.R.S., lent his powerful advocacy, but without avail, towards securing amendment on these lines while the Patents Bill was before Parliament; while in the United States the patent law from the commencement has allowed an inventor, during a period of two years before making a formal application for a patent, to publish his invention freely without detriment to himself.

Doubtless there are several methods by which this improvement in the law might be brought about. One method of so doing would be to grant the inventor in the case under discussion what in the Patents Act is technically termed " Provisional protection." By the reading of a paper before a learned society and an application for a patent being made within a specified time, say two or five years, together with the simultaneous deposit of a " complete specification," provisional protection might be conferred and antedated to the date of the reading of the paper. Complementary provisions of a simple and practical nature would also be required, so as to restrict the benefits to those for whom they were intended. By the conferring of provisional protection upon the inventor *ipso facto* by the reading of his paper, the patent to be subsequently received would bear the date of the reading and nominally there would be no publication before the date which the subsequently acquired patent bore. The inventor would hold the field during the period of two or five years, or whatever time might be provided, against everybody, and in particular against the mere snapper-up of a good idea who conceived some slight improvement or further step in advance and patented it, an advance which after all might be but little removed from the obvious and which was naturally within the ability of the originator of the main idea to produce.

(2) *Extension of the Area covered by Patent Protection.*—The originator of a scientific discovery cannot obtain patent protection for the practical applications of his discovery, whatever may be their importance to the welfare of the community, unless they are by their nature " manufactures " within the meaning of the statute of James I. This statute was the direct outcome of the economic conditions which prevailed at the time it was enacted, viz. in the year 1624, and of the economic theories which then obtained. It was designed to incite individuals to provide means whereby workless men might be put to profitable labour. The restrictions of the various trade guilds, particularly in the direction of what virtually amounted to limitation of output, and of their inelasticity as regards extension of the scope of the energies of their craftsmen, had been seen for a century or more to be affecting detrimentally the conditions of the labour market. At the time when the Statute of Monopolies was drafted what more likely means for coping with the prevailing distress could have been thought of than the bringing into this country a knowledge of new manufacturing operations or incidentally by the creation of manufactures by inventive ingenuity ?

The more the subject is examined the more certain it appears that the restriction of patent protection to mere " manufactures " was an historical accident. But the times have changed, economic conditions and thought have advanced, and the judiciary has deemed itself capable of extending the meaning of the word "manufacture" but little beyond that which it originally bore, at any rate not to the extent which modern requirements suggest. Within the rigid boundaries to which the Courts have held themselves to be confined, the judges have tried to deal with the matter on an equitable basis and to differentiate between the pioneer inventor and the follower, the discoverer of a master idea and the mere improver. Where they have found that a new discovery, purpose, or end has been brought to light and some ingenious means have been patented whereby the new discovery might profitably be employed, the judiciary has extended the scope of the protection given by the patent beyond that invention which the words of the patentee as they occur in the complete description of the invention might at first sight appear to describe. The judiciary has not been niggardly in its interpretation of the pioneer's own specification. But what is now wanted is legisla-

tive enlargement of the scope of our patent law infused with a similar spirit of equity. As regards Parliamentary action, although nearly three centuries have elapsed since the Statute of Monopolies was passed, no statute has been brought to bear whereby patent protection has been conferred on aught but " manufactures." A discovery by its very nature is not, it is true, capable of protection. Thought is free, and there is no monopoly in knowledge. A monopoly could be granted only for improved arts or practices the outcome of discovery.

Subject to this natural limitation the question may now well be asked whether there is any valid reason why a more generous measure of protection should not be accorded to the scientific worker. Why, for instance, should not an individual receive patent protection who discovers a method of breeding a rot-proof sheep, who originates new varieties of plants and cereals, or who invents new methods of fruit-culture, matters of no less moment in view of the ever-increasing demands of the community than are the more orthodox subjects of patents. Is not the man who advances public health by the application of some scientific or medical discovery as much entitled to a monopoly as, let us say, one who improves a mustard-pot ? Surely the question only requires to be formulated. Justice and expediency concur in requiring extension of the law whereby originators may receive a reward adequate to the importance of their discoveries, or, at any rate, proportioned to their public use. If deserving scientists are to be protected as regards researches which result in amelioration of the conditions of living, the " new manufacture " of the statute of James I. should be amplified and protection granted to any other new practice which is originated by the scientific mind and is of utility to the public. In other words, letters patent ought not to be confined to manufactures, but ought to be granted in respect of every invention of any new and useful art founded upon scientific discovery.

(3) *Suggested Provisions for Amending the Law.* — The following provisions, which would require the authority of an Act of Parliament, appear to be the simplest means by which the existing patent system could be modified so as to remove the disabilities which are dealt with in the preceding pages. Their effect would be (1) for a limited period to attach to the reading of a paper before a learned society the measure of

protection which is technically known as "provisional protection," with its beneficial consequences, and (2) to extend the scope of patent protection from "new manufactures" to every invention of any new and useful art founded upon scientific discovery. The amendments, moreover, are of such a character that, with no disturbance of current practice, all the elaborate machinery which has been erected to effectuate the patent system would be applicable.

New provisions to be read with the Patents, etc., Act, 1907 :

I. (1) If application for a patent in respect of an invention accompanied by a complete specification is made by the reader of a paper before a learned society within a period of two (or five) years from the reading of the paper, the reading of the paper shall, on request being made by the applicant to the Comptroller, be deemed to be an application for provisional protection of the invention, and the paper so read shall be deemed to be the provisional specification accompanying such application and the application shall bear date accordingly :

Provided that—

(a) the learned society shall, for the purposes of this section, have been certified as such by the Board of Trade;

(b) the paper read before the learned society has been printed and published within a year from the reading of such paper ; and

(c) the application, which is accompanied by the complete specification, shall also be accompanied by a copy of the paper or papers or extracts therefrom as read.

(2) The application shall be subject to examination and investigation in like manner as though it had not been made under the provisions of this section.

(3) The times within which all proceedings in connection with the application must be made shall be extended by a period equal to that between the reading of the paper and the lodging of the application, and, save as aforesaid, all proceedings shall be taken within the time and in the manner prescribed by the Patents and Designs Acts, 1907, or by rules made thereunder.

II. (1) The meaning of the word "invention," shall include, in addition to its content as defined in Section 93, any new and useful art founded on scientific discovery.

(2) A patent granted for any new and useful art founded on scientific discovery shall not be held to be invalid by reason only that the new and useful art is not a manufacture within the meaning of Section 6 of the Statute of Monopolies.

REVIEWS

Formal Logic: a Scientific and Social Problem. By F. C. S. SCHILLER, M.A., D.Sc., Fellow and Senior Tutor of Corpus Christi College, Oxford. [Pp. xviii + 423.] (London: Macmillan & Co., 1912. Price 10s. net.)

THE ordinary treatise on Formal Logic neither claims, nor in fact has, direct bearing on scientific fact or special interest for men of science. Of recent years there has arisen an extension known as methodology, which has, unfortunately, consisted of verbal and abstract discussion and has had small bearing on scientific work. Attempts to make the science practical and to criticise the methods used by scientific men are refused the recognition due to them because academic philosophers do not understand science, and men of science know little of philosophy and have made no careful and systematic study of scientific method. Therefore the blunders of one generation of scientific men, when some uncomfortable new series of fact reveals them, are silently glossed over and their successors proceed to repeat them in accentuated forms. Nothing is more needed than an extension of logic having some relation to science. We therefore turn expectantly to one of the prominent exponents of the pragmatist school of thought ; for pragmatism, if it is nothing else, is at least an attempt to bring philosophy closer to practical life.

Regretfully we are obliged to note that the positive contributions to a logic of science are meagre. The greater part of the volume consists of an attack on formal logic as commonly accepted and taught. The author describes it as an attempt to put the logicians' house in order and to clear the ground for a new logic that has yet to be written. It is, however, hopeless to attempt to deduce from the present book what the new logic would be if the author had time to write it. But such contributions to the advancement of the study of scientific method as are put forward it will be well to note.

In a way, the whole book may be regarded as a defence of science in a sense not very intelligible to any one unacquainted with the Oxford atmosphere. The intellectualist and academic school are disposed to depreciate the study of science, and in so doing they will not omit to mention the obvious fact that the typical inductive proof of scientific principles is not formally valid. The burden of Dr. Schiller's book is that formal validity is of no value or importance. It would perhaps be unwise to underrate the significance of the Oxford atmosphere, and the bearing of Dr. Schiller's attack should be duly noted.

More specific points will be found in the treatment of induction. Mill put forward five classic methods of inferring from effect to cause, and these methods have been subjected to interminable criticism ever since. Dr. Schiller demurs that the essential point is relevance. "Instead of talking about facts at large, let us say *relevant* facts" (p. 268). But Dr. Schiller does not think that the validity of the methods is thus saved. He thinks, on the other hand, that the introduction of the idea would make out a case for a "third branch of logic, underlying both deduction and induction, which would determine the relevance of fact and be more

important than either" (p. 270). Dr. Schiller's view is that relevance consists in what is *selected* by a knower as *helpful* for his purpose, and, consequently, purpose and personal psychology are introduced into the very foundation of scientific investigation. By this road an opening is made for the distinctive catchwords of the pragmatist philosophy. All this would have been made so much clearer, in the way Mill so admirably expressed himself, by a few well-chosen examples. It is so easy to infer anything you please so long as you confine discussion merely to general terms. We can only reply in general terms that it is the universal experience of men of science that valid results are only obtainable in so far as personal psychology and special conscious purpose are eliminated from the process of inference. It is probable that Mill would have had little difficulty in disposing of Dr. Schiller's criticisms. There are also a number of points in the chapter on causation which it is not possible to discuss in the space at our disposal.

Needless to say, there is much cogent criticism in Dr. Schiller's diatribes. The currently taught methodology is certainly somewhat futile from the standpoint of scientific investigation. It is, as the author points out, strangely paradoxical that the theory of science is in Oxford (at London it is admitted as a science subject) taught only to those who know nothing of its practice. But surely not even the current methodology is such as to delay the progress of the science student. Certainly it is far from adequate. But there have been valuable works on the logic of science. Dr. Schiller has forgotten Jevons.

To turn to the more strictly logical part of the book, it should be remarked that the term logic is commonly used in two senses. It may mean purely formal logic (represented by Jevons and Keynes), it may mean metaphysical logic (represented by Bradley and Bosanquet), which is partly logic, partly methodology, partly metaphysics, and partly, to some extent, psychology. We shall confine ourselves almost entirely to the formal side. That curious medley, metaphysical logic, will, no doubt, in time, sort itself out. Dr. Schiller is entirely antagonistic to both, and his attempt to set the logicians' house in order greatly resembles the Chinese method of burning it down. It will be advisable, therefore, to devote some space to the consideration of one or two fundamentals, the full bearing of which Dr. Schiller seems to have disregarded.

The first concerns axioms. Reasoning need not be based on axioms, but it often is, and a careful study of their import is essential to any attempt to clarify logic. Dr. Schiller objects to the use of the term *a priori*. There are, certainly, several senses in which it can be used. But the term, whatever its demerits, does, at least, show the fundamental difference between such truths as the axiom of quantity and the everyday facts of observation and experience. On this matter, Aristotle, Kant, and Spencer, notwithstanding differences, all agree. Dr. Schiller disagrees, or appears to do so. He regards postulation as the source of universal propositions, and makes no clear distinction between the method of arriving at universal truths, in which, no doubt, postulation plays a part, and the certainty which accrues to such truths when enunciated. Dr. Schiller's views on this matter are more fully expressed in *Axioms as Postulates*.[1] These views I have already criticised at some length, and do not care now to repeat the criticisms. In the volume under review, it is not at all clear whether Dr. Schiller has modified the views he previously expressed. His exposition requires clearer and fuller statement with special

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[1] The essay in question is published in a volume entitled *Personal Idealism*, edited by Mr. Henry Sturt, and published by Macmillan. My criticisms will be found in an article entitled "Evolutionary Empiricism" (*Mind*, No. 73).

reference to scientific principles. The statement (p. 244) that the scientific status of the indestructibility of matter has been impaired by the discovery of radioactivity is highly disputable. It depends on definition and point of view. The sentence on the conservation and the dissipation of energy is liable to give the impression that the author does not understand the meaning of the latter principle. Moreover, any one speaking of "gravitation" as axiomatic is using the term in a sense very different from that commonly understood. The importance of the treatment of axioms in all discussions on the foundations of logic can hardly be over-estimated, and the critic approaching Dr. Schiller's volume with the desire to find a clear and coherent view will be left with the impression that an adequate discussion of this point would exhibit inconsistencies with the main trend of the argument.

The second fundamental is that ancient problem, the nature of formally valid inference. The old logical query whether syllogistic reasoning ever elucidates new truth is a particular case of the larger general question. Those who maintained that the syllogism was a *petitio principii* were confronted with the inference that any one acquainted with the axioms and postulates of Euclid, therefore, knew and understood every truth of geometry. It was evident, in this case, that something new *was* elucidated. Dr. Schiller's solution is interesting and plausible. He says that, in all real reasoning, we reason with regard to a doubt. Every syllogism applied to a particular case is an experiment to discover whether an individual who belongs to a class for most purposes can have attributed to him some specific character possessed by other members of the class. This is true enough in its way. But it hardly elucidates the relation between the properties of a parallelogram and the axioms and postulates of Euclid. The meaning of the iron rigidity of logical inference is a problem which none who seeks to penetrate to the foundations of logic can ignore. Dr. Schiller, unfortunately, talks round the problem. I am, no doubt, free to infer that the angles of a triangle are together equal to two right angles, or that the diagonals of a rhombus bisect each other at right angles, or neither. But this does not explain why both are absolutely certain formally valid truths implicit in Euclidean geometry. What is the meaning of this certainty? Why does each step of the reasoning follow from the last? The problem is very similar to the one concerning the nature of axioms.

Considerations of this kind will show a sphere for formal logic much greater than Dr. Schiller is willing to admit. Dr. Schiller wishes to displace logic by some as yet unformulated science of "psychologic." In a later chapter he, semi-humorously, commends formal logic as a good game. The passage is worth quoting :

"Friends, your judgment is too harsh. You must not judge logic by your own feelings nor condemn it because you have no use for it. You should live and let logic live. Moreover, it really has a use. Its use is to keep logicians employed and amused. The study of Formal Logic makes a highly intellectual game. . . . You think it a silly game ; well, in a sense, all games are silly. . . . " and so on (p. 388).

This is interesting and amusing. But Dr. Schiller does not appear to realise that every time we make any inference whatever, practical or theoretical, *a part of the process*, the conceptual part, the formation of the conceptual systems which we apply to reality, whether in mathematics, in science, or elsewhere, comes within the sphere of influence of this game. Granted that it is not the whole process. It is sufficiently important if it is only a part. Also, with regard to the extension of logic which all philosophers contemplate, it is very doubtful whether

"psychologic" would be a correct description. The nearer we get to exact knowledge of anything the more the "psychology" disappears.

The dominant note of this brief review is criticism. It is bound to be so with Dr. Schiller. His whole volume is so critical. But he is always interesting. His style is vigorous. His remarks are always relevant to the state of knowledge of the time. His criticisms on the details of formal logic as actually taught we must leave to the strictly formal logicians to answer. They are very cogent, and require an answer. The book will be of considerable interest to any one to whom the subject appeals. But it is to be regretted, in so large and bulky a volume, that there has been no attempt at definite and positive construction. Is there anything constructive in pragmatist philosophy? Or is pragmatism merely a revolt from the current academic intellectualism?

H. S. SHELTON.

A Systematic Course of Practical Science. For Secondary and Other Schools. By ARTHUR W. MASON, B.Sc., B.A. (Lond.) Book I.—Introductory Physical Measurements. 1s. 6d. net. [Pp. 126.] Book II.—Experimental Heat. 2s. 6d. net. [Pp. 161.] (London: Rivingtons, 1912 and 1913.)

THE two volumes contain the outlines of the first two years of a course of practical science. The directions are given clearly, as are also the methods of entering and of tabulating the results. The book should be of great assistance to the teacher in charge of a practical class. It is certainly one of the best and one of the most thorough of the many class-books at present on the market.

It should be said, by way of criticism, that it is one which a teacher would need to use with considerable discretion. Some of the experiments, especially in the book on heat, seem much more suitable for the lecture-table than for the laboratory. A secondary school laboratory would need to be exceedingly well equipped in order to allow some of the experiments to be performed by a class of pupils. The one on the variation of boiling points with pressure (42) and the use of Bunsen's ice calorimeter are cases in point. Some of them point to the probability of the smashing of apparatus and the loss of mercury. Nothing is said of the age of the pupils for whom the experiments are intended.

On the other hand, the teacher using the book with discretion will find that most of the ordinary easy experiments illustrating elementary physics are included and are described in a thoroughly practical manner. He is in no way bound to follow the order of the book or to include all the experiments in the course. Indeed, he would be foolish to attempt to do so. So used, a better book could hardly be obtained.

It is a small point, but one that the teacher will appreciate. The dimensions of the book are such as to allow it readily to remain open at any page, a great convenience for laboratory use.

H. S. S.

The Science of the Sciences. Constituting a New System of the Universe which Solves Great Ultimate Problems. By H. JAMYN BROOKS, author of *The Elements of Mind.* [Pp. 312.] (London: David Nutt. Price 5s.)

AS indicated by the title, the author's System claims to "explain, or to form the nucleus of explaining every mystery in the universe excepting—(1) The Mystery of Beginning and End ; (2) The Subjectivity of Substance."

The System is stated in outline in eleven propositions, of which the first three are quoted below :

" (1) That the principal basis of mind is a quasi-chemical substance (to which the term 'mental ether' is given), and that it can be analysed into quasi-chemical elements.

" (2) That the principal basis of physical force is also a quasi-chemical substance (to which the term 'physical ether' is given), and that it can be analysed into quasi-chemical elements.

" (3) That these elements, together with all universal elements, are fundamentally the same as the chemical elements."

There are also four hypotheses which constitute an important part of the System, entitled respectively—"The Universal and Monistic Hypothesis, The Chemical Hypothesis, The Physical Hypothesis, The Mental Hypothesis." Once again, perhaps, it will be well to allow the author to speak for himself. The first two hypotheses are as follows :

" 1. That the universe is a compound of all the universal elements, and that each element is coextensive with space and can have no independent existence.

" 2. That all matter contains the whole of the chemical elements and that each element is universally diffused throughout the whole space occupied by the elements."

It should also be mentioned that the author has received congratulatory letters from such men as the late Prof. William James and Prof. James Sully. These refer strictly to an earlier work, but, as the essential ideas are the same, the recommendation should be mentioned for what it is worth. The present volume has been critically investigated by two of the foremost Fellows of the Royal Society, who said that it ought to be published.

No doubt it ought, and the author is to be congratulated on at last being able to place his views before the public. Any one who is interested in hypotheses of this kind, and in the study of systems of the universe, will find much to interest and amuse. It is to be hoped that the work will have a sale sufficient to encourage publishers of serious works, in case of doubt, to take the risk.

In the present case it is necessary to make one decisive criticism. The author plainly and obviously does not understand the elementary facts and theories of the sciences with which he deals. A large portion of the book would have to be rewritten if he had troubled to acquire the most elementary knowledge of chemistry and physics. The assistance of men of science has not enabled him to remedy the defect. As an example, on p. 49, air is referred to as a compound, and it is absolutely impossible, from the trend of the argument, to decide whether the statement is a slip or sheer ignorance. Because atmospheric nitrogen—which, by the way, is considerably heavier than nitrogen obtained from compounds— is found to contain considerable quantities of other elements than nitrogen, therefore chemically pure nitrogen contains infinitesimal traces of every known and unknown element—so runs the trend of the argument. There is absolutely no connection between fact and inference. Again, in the chapter on the tides, the author shows plainly that he does not understand the simplest elements of the current tidal theory that he is attempting to displace. He apparently does not know that the tide-raising force is, approximately, inversely proportional to the cube of the distance between the attracting bodies, and inquires (with the proviso that the question may be answerable) why dry leaves and other loose materials are not sucked up by the attraction of the Moon.

The author says somewhere that it is a pity that his system did not originate

with a Huxley or a Kelvin. He means, I suppose, a Hegel or a Spencer, for Kelvin and Huxley were essentially specialists. But the value of the works of philosophers such as these lies not so much in their systems as in their profound knowledge and insight, in their grip of the knowledge of their time, in the fact that they understood the principles of science more clearly than the men of science themselves, and were sufficiently well acquainted with the details. Kant was a physicist before he became a philosopher. Spencer would have achieved eminence on biological work alone. It is not much use putting forward systems unless one knows enough to know when one is not talking sense.

Mr. Jamyn Brooks has written a work on psychology which, it seems, has been well received. If his work in that department is unsound, he is covered by the fact that the whole science is a little vague and shadowy. The reviewer would suggest that it would be better if he concentrated on the psychological side and if he did not attempt to deal with problems of natural science until he has acquired a sound elementary knowledge of the sciences with which he deals. Let us assume that the author's hypotheses are all true and valuable (to the reviewer they scarcely appear so), it would still require the ability and the knowledge of a Hegel or a Spencer to set them forth in detail. The present volume provides no evidence that Mr. Brooks possesses either, and, if he does not, the very existence of his book is a weapon in the hands of the "stodgy" man of science who is impervious to new ideas. Who was it called van 't Hoff's chemistry in space the vapouring of an unsound mind? The type always exists, and a book like that of Mr. Brooks is so much grist to his mill. Those who think they are the originators of new ideas may look at this volume and decide that, after all, it is safer to leave it alone.

H. S. S.

Vectorial Mechanics. By L. SILBERSTEIN. [Pp. vi + 197.] (Macmillan & Co., 1913. Price 7s. 6d. net.)

THE work of Heaviside, which has demonstrated so clearly the power and relative simplicity of vector methods for dealing with quantities essentially vectorial, is bearing good fruit. There are already in Germany good textbooks, such as those of Bucherer and Gans, which give an introduction to the methods of vector analysis. In the book under review we welcome at last an English book in which a systematic account is given of the vector analysis in use among the physicists of the present day, and its applications to mechanics. Even to those with a good knowledge of the subject it will prove very interesting, as there is a distinct originality of treatment and a unity and sequence which make a strong appeal.

The work is divided into six chapters. We have first a general introduction to vector analysis, which is probably the best in any English textbook. In this the vector and scalar products, the curl, divergence, and the important theorems connected with them are exposed. There follow three chapters, under the headings of General Principles, Special Principles, and Rigid Dynamic, dealing with the application of the analysis to dynamics, those parts, such as the motion of a body under no forces, which best lend themselves to vector methods being naturally treated in the greatest detail. The final two chapters deal with the theory of elasticity and hydrodynamics. There is a useful and instructive appendix, giving the most important equations of the book, together with their Cartesian equivalents. The linear vector operator, of which, outside Heaviside's work, it is hard to find an account, is introduced in connection with moments of inertia, and developed

at greater length in the treatment of the fundamental equations of strain : its properties are demonstrated in a very lucid manner. The section on vortex motion affords an excellent example of the advantages of vector methods in dealing with problems of this kind.

The author has evidently spared himself no pains to make the book clear and consequent, and the care with which the difficulties likely to present themselves to the student have been foreseen bears witness to his discrimination. The abstract dynamical principles are illustrated by simple and direct examples, which make their scope and meaning clearer than could be done in a discussion occupying many times their space. One of the most striking features of the book is the brevity which has been achieved without sacrifice of either clearness or accuracy.

The student will find here an excellent introduction to the whole field of vector mathematics, and especially, although electrical problems are not directly treated, a very good preliminary to the study of modern electrodynamics. The collection of examples, with hints for the solution of the harder ones, is likely to prove exceedingly useful.

<div align="right">E. N. DA C. A.</div>

Researches in Magneto-Optics. By P. ZEEMAN. [Pp. xi + 219.] (Macmillan & Co., 1913. Price 6s. net.)

IN this book, which forms one of Messrs. Macmillan's excellent series of Science Monographs, the famous author gives an account of the researches carried out on the phenomenon associated with his name—the modification of the nature of the emitted light which takes place when the source of light is placed in a magnetic field—and the closely allied magnetic rotation and magnetic double refraction of light. While dealing largely with the author's own experiments, as is necessarily and desiredly the case, the book gives an account of all work done on the subject from the fundamental discovery in 1896 down to the middle of the year 1913. Starting with a chapter on modern spectroscopes—in which he discusses Rayleigh's theory of resolving power and the performances of the Rowland grating, the échelon, étalon, and other recent devices for the finer analysis of light—the author passes on to the fundamental experiment, the magnetic resolution of emission lines, giving his original paper and Lorentz's simple and brilliant theory of the effect. It is interesting to note that this supplied the first proof that the centres of light emission are negative electrons, and that the value of e, m deduced was one of the very earliest determinations. In other chapters he treats of the inverse effect —i.e. the multiplication of the absorption line when the absorbing body, such as a salted flame, is placed in a magnetic field, the types of resolution more complicated than that indicated by Lorentz's simple theory and found in the first experiments, and the magnetic rotation of the plane of polarisation. This phenomenon follows theoretically from the unequal velocities of propagation of the right-handed and left-handed circularly polarised components, demonstrated by Zeeman to exist for rays propagated paralled to the force when the absorbing body is placed in a magnetic field. The importance of the Zeeman effect for astrophysics is brought out in the chapter on Hale's researches, which revealed the effect in the light coming from the sun in the neighbourhood of spots. This is in striking accord with the theory that the sun-spots are solar vortices, for the electrons whirled round in these vortices would produce the magnetic field required for the phenomenon.

The theoretical work which has been done on the subject is most attractively exposed, the essential assumptions of each theory and its main consequences being set out with brevity and great clearness. In the last chapter the origin of the spectral series, and the complicated types of resolution of the lines in the magnetic field, are considered from the theoretical standpoint, and an account given of Ritz's theory of the series, and Lorentz's theories of the coupling of the emission centres by the magnetic field, which, with Voigt's modifications, is capable of accounting for the various types of resolution. The necessary assumptions, however, appear most artificial, and it cannot be said that the present state of the theory is altogether satisfactory. Ritz's theory of the Zeeman effect has been shown to be unworkable by Voigt. Dr. Bohr's papers on the constitution of the atom, which have appeared during the last few months, seem to supply a more simple and inclusive theory of the spectral series, though it remains to be seen if his atom can be induced to give the experimental Zeeman resolutions.

The style and arrangement of the book are throughout admirable, and the author has contrived to give a very clear account of mathematical theories with a minimum of symbolic working. The many personal touches, such as the rather pathetic account of the postponement of the work on the types of resolution of lines of the same series for lack of suitable equipment, give an added attractiveness. It would be difficult to speak too highly of the general production of the book, which contains a large number of the most beautiful photographs illustrating the various effects.

E. N. DA C. A.

Principles and Methods of Geometrical Optics. By J. P. C. SOUTHALL. [Pp. xxiii + 626.] Second Edition. (Macmillan & Co., 1913. Price 25*s*. net.)

THE fact that Professor Southall's work on Geometrical Optics, first published in 1910, has already appeared in a second edition, shows that it has speedily won the recognition it deserves as the best book on the subject in English. The changes in the new edition are small ; the arrangement and pagination have been left exactly as in the first edition, two appendices to the chapters on Refraction of a Narrow Bundle of Rays through a System and on the Theory of Spherical Aberration respectively having been bodily inserted on lettered, not numbered, pages. These appendices are in the nature of further notes to the chapters ; the second contains a detailed account of the calculation of the spherical errors of a centred system by means of the Seidel formulæ.

To the English reader who wishes a full and clear account of the recent work on geometrical optics, especially that of the Germans—we need only mention the names of Abbe, Petzval, Seidel, and Czapski—to whom most of the recent advances are due, the book can be unreservedly recommended. It covers the whole field in a complete and satisfactory way, making full use of the most modern methods, many of which are not to be found in any other text-book of which we know. Especial attention is throughout devoted to the applications to the design of modern optical instruments.

It is to be hoped and expected that the book will do much to revive the study of geometrical optics in England, which, although at present the Germans are its undisputed masters, was the first home of the science.

E. N. DA C. A.

Stellar Motions—with special reference to motions determined by the Spectrograph. By WILLIAM WALLACE CAMPBELL, Sc.D., LL.D., Director of the Lick Observatory, Univ. of California. [328 pp., 8vo, 14 figures and 34 tables in text.] (Henry Frowde, University Press, Oxford, 1913. Price 17s.)

SCIENCE, in all its branches, has during the last century advanced with enormous rapidity. What to one generation seems impossible may, to the succeeding generation, be merely a commonplace. Thus it was but about seventy-five years ago that Auguste Comte wrote that "we shall *never* be able to study the chemical composition of the celestial bodies; . . . our positive knowledge with regard to them will necessarily be limited to their geometrical and mechanical phenomena. It will be impossible, by any means, to include investigations of their physical, chemical (and other) properties." So much for the dogmatism of the philosopher, for within twenty-five years from the time of writing the fundamental principles of spectroscopy were formulated by Kirchoff. Sixty-five years ago the possibility of determining the radial motions of the stars was undreamt of; but with the enunciation by Doppler in 1842 of the effect of the motion of the source upon the wave-length of the emitted disturbance, and with its application to optical problems by Fizeau in 1848, the way was paved for the solution of the problem. Little progress was made, however, for some time. Visual methods of determination are difficult, even in the hands of skilled observers, and liable to errors which may greatly exceed the velocity to be found: thus the star "α Cassiopeiæ" which, according to the best modern determinations, has a radial velocity towards the sun of 3·9 ± 0·15 km. per sec. was found by visual observations at Greenwich in 1885 and 1887 to have velocities of 90 km. and 58 km. per sec. respectively, away from the sun. Twenty-five years ago the radial motion of not a single star was known, even approximately. The rapid development of this branch of astronomy during recent years has resulted from the introduction of photographic methods. Even with this great advance, the displacements of the spectral lines to be measured are so small and systematic, and other errors may so easily enter, that great precautions have to be taken if an accurate result is to be obtained: in 1890 the radial velocity of the star cited above was determined photographically at Potsdam as 15·2 km. per sec. towards the sun, almost four times as large as the correct value. It is to the improvements in the spectrograph and in the methods of measurement introduced by Dr. Campbell, that the present degree of accuracy is largely due, and it is to the untiring labours of him and his colleagues at the Lick Observatory, and at its southern dependance at Cerro San Cristóbal, Santiago, Chile, that we owe almost the whole of our knowledge of the radial velocities of stars; it is, therefore, very fitting that an account of the theory and methods used, and discussions of some of the problems which have arisen in connection with the results obtained, should be for the first time collected together into one volume by Dr. Campbell himself.

The eight chapters of this volume formed the Silliman lectures delivered by the author in Yale University in 1910. To bring the volume up to date, a series of footnotes have been added incorporating the chief results obtained since their delivery. An important feature of the book is the valuable series of tables, thirty-four in number, which illustrate the text, and contain a mine of information. Each chapter of the book is essentially complete in itself, and more or less independent of the others. The first two are introductory, and include a brief account of the development of the subject, and a description of the D. O. Mills

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spectrograph, and of the precautions which are taken in order to secure accurate results. There follow in the third chapter the applications of these principles to various problems of the solar system, such as the period of rotation of the sun, and of Saturn's rings, and also to individual stars. The fourth chapter deals with the various methods of determining the solar motion from statistical considerations of the proper motions of stars, whilst in the following chapter the same problem is discussed from the radial velocity determinations. This method has considerable advantages as compared with the older one. In it the actual radial velocities are used, whereas proper motions are determined in arc, and for given linear motions they vary inversely as the (unknown) stellar distances: moreover, radial velocities can be measured very accurately in a short period of time, whereas the accurate determination of a proper motion requires a series of observations extending over a long interval. The one disadvantage of the method is that, at present, the velocities of the faint stars cannot be found, but with increase in the power of the instruments used this difficulty will be largely overcome. It is probable the velocity of the solar system in space as determined by Campbell is the most accurate yet made.

The sixth chapter contains several applications of the results obtained to the stellar system. By eliminating the solar motion so as to leave the peculiar motions of the stars, it is found that the number of positive velocities is considerably larger than the number of negative, whereas the numbers should be very nearly equal. The residual average velocity belongs almost entirely to the stars of class B, and can be explained if there is an average increase in the wave-lengths of all lines utilised of ·07 A: such an increase could be caused by a pressure of from twenty to thirty atmospheres; and this may be the correct explanation, because in these stars the absorption bands are of considerable breadth, as if widened by pressure. Such a pressure effect appearing in so unexpected a manner is of peculiar interest, and may throw some light on our knowledge of class B stars. In this chapter also is a conclusive proof that stars of early spectral types are travelling slower than those of later classes, as the following table vividly shows :

Spectral class.	No. of stars.	Av. Rad. veloc.
O and B	141	8·99 km.
A	133	9·94 km.
F	159	13·90 km.
G and K	529	15·15 km.
M	72	16·55 km.

On the other hand, stellar velocities are not functions of the visual magnitudes. There is no indication that the fainter stars are travelling more rapidly than the brighter. Another important result is drawn by the author from statistical considerations, and by comparison with results previously deduced from Proper Motion data, viz. that the stars of various magnitudes are more thoroughly mixed in space than had been previously supposed. These are but a few of the important problems discussed in this fascinating chapter.

The last two chapters are concerned respectively with visual and spectroscopic binary stars, and with variable stars, and give a conspectus of our present knowledge on these subjects, which the study of radial velocities has helped to increase in no small measure.

The treatment of such a vast subject is necessarily incomplete; but the author has well succeeded in showing how rich a field of investigation is being opened by the study of radial velocities, which, taken in conjunction with proper motion

determinations, promise, in time, to materially assist in forwarding the solution of the fundamental problem of astronomy—the problem of the evolution of the Universe.

<div align="right">H. S. J.</div>

Quantitative Chemical Analysis. By A. C. CUMMING, D.Sc., Lecturer in Chemistry, University of Edinburgh, and S. A. KAY, D.Sc., Assistant in Chemistry, University of Edinburgh. [Pp. xi + 382.] (London: Gurney & Jackson, 1913. Price 7s. 6d.)

THE average student who attacks for the first time a particular analytical determination usually finds that, although he may follow the instructions of his text-book, his results are erroneous; and unless he has been shown the process in full detail by the teacher, this is almost bound to be the case. There has hitherto been, in fact, no book on analytical chemistry which properly instructed the student in the complicated technique which even the simpler analytical processes demand. It may be said that such instruction should be conveyed by example and not by precept; but whilst this is undeniable up to a certain point, it remains true that fully one-third of the time given by laboratory teachers in demonstrating all the small but essential points of technique to each separate student could be saved by a little more attention to detail on the part of authors of text-books on analysis. Nor can it be said that these remarks smack unduly of "spoon-feeding"; for in these days no student has time to re-discover for himself the "tips" which are our heritage from generations of analysts, let alone the broader details of practice; he *must* be "spoon-fed" to some extent, and the successful relegation of any part of this process to the pages of a text-book confers a boon upon all teachers in large laboratories.

By this standard, as well as by others possibly higher, the work now under review is emphatically a success. The descriptions of procedure are so carefully done, and the gradation of the difficulties is so thoughtfully carried out, that any student who works along the suggested lines could hardly fail to become thoroughly competent. It would have been beyond the scope of a book such as this to have dealt with the theories underlying analysis, and the authors have wisely confined themselves to practice pure and simple. A multiplicity of methods has been avoided, with the result of freeing the student from the feeling of *embarras de richesse* which some of the larger works in vogue tend to induce; at the same time, the methods given are numerous enough to be quite representative, they are up-to-date, and in frequent instances they include many most useful novelties; and all bear the mark of having been proved by the author's own experience.

After the first general chapter, volumetric analyses are first dealt with; then follows a series of typical gravimetric determinations, then a chapter on electrolytic methods. This and the succeeding section on colorimetric analysis are especially valuable. (In passing, it may be suggested that the bismuthate method, now so widely used for determining manganese, might be included in later editions.) Part V. contains a systematic account of the separation and determination of each of the common radicles, conveniently arranged alphabetically. All that is in this section is good, but the omission of cobalt from the list is somewhat strange. The treatment of alloys and of ores is dealt with in Part VI.; then comes gas-analysis, followed by an admirable section on water-analysis. Part IX. treats of organic analysis, and is sure to be found valuable, since it includes an

account of the method of combustion, introduced by Walker and Blackadder, which has generally replaced the older methods wherever it has been tried. The last section is a clear account of the determination of molecular weights. The appendix contains data and useful tables concerning reagents, also a table of logarithms. Finally, the book is judiciously illustrated in a helpful manner.

Altogether, the authors are to be congratulated on having produced a book which cannot be too highly recommended for its purpose, and whose worth has already been discovered both by teachers and by students in more than one laboratory.

I. M.

Organic Chemistry for Advanced Students. Vol. II. By JULIUS B. COHEN, F.R.S., etc. [Pp. vii + 427.] (London : Arnold, 1913. Price 16s. net.)

TO write a book dealing generally with any one of the three main branches of chemistry is a task which becomes yearly more difficult; and this is particularly true in the case of the organic branch. Physical chemistry has reached the quantitative stage, and, guided by mathematics, it keeps on a fairly straight path; inorganic chemistry is now semi-quantitative as a result of the affiliation with physical chemistry of which Abegg's *Handbuch* is a visible sign. In the eyes of many followers of these two branches, their organic colleagues are simply wallowing in the mire of qualitative thought; and yet it was from the study of organic compounds that some of the fundamental principles of general chemistry arose, and the inorganic worker is often apt to overlook the very important contributions which his own branch is continually receiving from the organic side.

Nevertheless, organic chemistry is certainly in a less advanced state, and it is deficient as yet in quantitative laws. Failing these, classification of the vast masses of fact must be resorted to; and after classification comes theorising. The regrettable fact is that frequently theories are propounded before classification is properly begun; and, in addition, what are in reality tentative schemes of classification are often mistaken for explanatory theories.

It is here that the teacher enters the field; and the chief purposes of an advanced course of organic chemistry should be to direct the student's steps away from these two pitfalls, and at the same time to criticise, both destructively and constructively, actual theories. Viewed from this standpoint, Prof. Cohen's second volume is curiously patchy. Impartiality in a teacher is a very necessary virtue, but it can be practised to excess; and this is the chief fault of an otherwise interesting book.

The five chapters which compose the volume under review naturally dovetail into the earlier parts of the first volume, which has been widely used during the last five years. With the author, we may hope that in later editions a re-arrangement may be effected; but in the meantime, these five chapters in themselves form on the whole a fairly natural sequence. The theme around which they chiefly centre may be said to be the perturbations of atoms and of interatomic forces within the molecule; in other words, the central problems of present-day organic chemistry. It follows that many of the subjects dealt with are of a physico-chemical character, and thus the value of the completed work is much enhanced.

The first two chapters ("The Valency of Carbon" and "The Nature of Organic Reactions"), which occupy some 200 pages, are undoubtedly the most interesting of the five. Theories of valency may be said to deal with one or

more of the topics of intermolecular linkage, interatomic linkage, and the intra-atomic origins of these. The last variety is best left to the physicist to elaborate ; inorganic chemists are striving with the first, but it is from organic chemistry that we derive most light both on this and on interatomic linkage.

Most of the first chapters of the book, and the first fifty pages of the second, deal with unsaturated carbon. The student who can think for himself will be both stimulated and provoked into doing so as he reads this part of the work ; he will be stimulated by several very lucid accounts such as that of Thiele's views on double linking, and he will be provoked into thinking for himself by the absence of more than a slight correlation between the divers theories. This is due rather to the present condition of the subject itself than to faults of treatment, and although one could wish for more blending of the theories than the author has shown, the fact stands out that the day of the all-embracing theory is not yet.

Broadly speaking, there are two views respecting the nature of unsaturated carbon atoms, of which one is that the atoms are in a "carbonous" state of di- or of trivalency. In Chapter I. is to be found a survey of the work of Nef and others on divalent carbon, which is clear and concise. The controversial topic of triphenylmethyl is impartially reviewed ; but it is not clear why the existence of ions of triphenylmethyl in solution should imply the existence of molecules of the single radicle, nor is the probability of union with the solvent pointed out. In passing, attention may be directed to a misprinted equation on p. 7. Hinrichsen's and Thorpe's extensions of the "carbonous" idea to ethylenic compounds are well treated. In the first portion of Chapter II. the nature of addition and substitution processes is discussed ; and here we meet the second kind of idea of unsaturation, that of partial valencies, which lies at the root of many hypotheses besides Thiele's. The whole section is an interesting one, and the author's criticisms are valuable.

Then follows a section devoted to "dry" catalytic actions such as those of Sabatier and Senderens. This is perhaps a digression, dealing, as it does, for the most part with practical methods and results ; but it is followed by the best part of the book, namely, that on chain and ring formation. This is really a revision and amplification of the chapter on Condensation in Vol. I., and without doubt the improvements justify its inclusion here. The simple classi-fication of condensations which the author introduces on pp. 109-10 will greatly help the student to cope with this huge subject, and the historical mode of treatment adopted at the same time is of much educational value. The only criticism which is called for is that, in discussing "Grignard" reactions, the author might have taken the opportunity to abandon the usual text-book basis of treatment, which regards Grignard reagents merely as useful aids in preparing otherwise difficultly obtainable compounds. All applications of these reagents fall under one or other of two headings—double decomposition, or addition followed by hydrolysis or other suitable double decomposition ; and some such treatment as this, perhaps in connection with Lapworth's views, would have been welcome.

Coming to the third chapter ("The Dynamics of Organic Reactions"), written by Dr. H. M. Dawson, the reader must readjust his standpoint; in fact, excellent though the chapter is, it is not quite in harmony with the rest of the book. Here again, however, the fault lies with the subject and less with the author; for dynamical studies in organic chemistry have not yet fulfilled their early promise, except in comparatively few cases of systematic researches. This being so, the

subject is hardly susceptible to arrangement according to broad results and general conclusions, and the chapter hence takes the form of a clearly written summary of the various types of reactions—unimolecular, bimolecular, and so on—with well-chosen illustrations from organic chemistry.

In the chapter dealing with the relationships between physical properties and structure, Prof. Cohen has given a particularly lucid account of the development and present state of most of the subject. If any criticism must be made, it is that photographs and descriptions of apparatus seem to be superfluous in a work of this kind, which is already sufficiently bulky.

One cannot help thinking that the author has been less happy in discussing colour and the absorption of light. The section on absorption-spectra is relegated to the chapter on physical properties, which is inconsistent with the identity of visible and of invisible colour to which the author himself calls attention. The present stage of our knowledge of the influences of structure on light-absorption is one of classification chiefly; and many of the so-called "theories" of colour are hardly more than summaries of the compounds which possess the faculty of absorbing light rays. Greater discrimination in this direction would have been acceptable, and the sifting and blending processes before referred to could have been utilised in this chapter to great advantage.

The volume contains few noteworthy misprints; the reproduction of the photographs of absorption-spectra, however, might well be improved. There are full indexes, and references are given throughout both to original papers and to larger monographs.

On the whole, then, the virtues of the book, which are many, are to be found in the clear presentment of facts and of separate theories; and its chief fault is one of omission, consisting of insufficient correlation of these theories. Nevertheless, the two volumes together form a valuable acquisition, and will earn a wide circulation.

IRVINE MASSON.

Organische Arsenverbindungen und ihre chemotherapeutische Bedeutung. By DR. M. NIERENSTEIN. (Stuttgart: Sammlung Chemischer und Chemisch-technischer Vorträge, vol. ix, 1912.)

AFTER a short historical sketch of the employment of arsenic and its compounds in medicine from the earliest times, the author gives a complete list of the arsenical compounds prepared by Bunsen in his investigation of the Kakodyl series, and also a selection of the more important substances synthesised by Michaelis; then follows an account of Ehrlich's so-called Reduction Theory of the mechanism of the action of the azo-dyes Trypan Red and Trypan Blue and of Atoxyl on trypanosomes, a theory which led ultimately to the discovery of Salvarsan. Some space is next devoted to the discussion of two alternative theories described as the Oxidation Theory of Breinl and Nierenstein and the Partial Cell-function Theory of Uhlenhuth, in connection with which the pharmacological action of a number of synthetic arsenic compounds is described. The monograph may be welcomed as a useful summary of a somewhat extensive subject.

The Continent of Europe. By LIONEL W. LYDE, M.A., F.R.G.S. [Pp. xvi + 446. With 12 coloured maps, and numerous smaller maps in the text.] (London: Macmillan & Co., 1913. Price 7s. 6d.)

IT is to be hoped that the users of books read the prefaces that are prefixed by conscientious authors. Mr. Lyde's preface is unusually helpful, and is an intro-

duction to a series of books on the continents of the world. Physical features are here described rather than explained; geography is thus wisely delimited from geology. Mr. Lyde regards the "political control" as providing "the dominant note in the most important areas." The fact that the three parts of Poland contradict his first page in every essential only makes the position of Poland appear more melancholy and more exceptional.

In the next page, the tetrahedral theory of earth-structure is adopted, as explaining the grouping of land-areas in fairly high northern latitudes. On the third page we have a discussion on technical nomenclature, in which it is wisely urged that it is better to introduce a specialised term for a special thing, rather than an existing word in a narrowed sense. Mr. Lyde justly objects to "a high" and "a low" in meteorology; but he introduces us to "a wyr," instead of "an anti-cyclone," which is only replacing a well-established technical term by another that we tremble to pronounce. How did Robert Bruce's military engineers pronounce "wyr"? And was this pronunciation the same on both sides of the border? Lies there yet at Lanercost or Douce Cœur a craftsman who can rise to tell us?

Mr. Lyde has a love of the picturesque in history, and this prefatory episode is a foretaste. All through his book we are led off into delightful trains of thought; to complete the scene that he conjures up we must go from one of our bookshelves to another, from Gibbon to Motley, from De Comines to Miller on the Balkans. He holds himself, however, in great restraint, and the compression of some of his sentences is almost too severe. "What we miscall Macedonia" (p. 149) is just thrown out to make us think; but the connection suggested between "the western end of this old folded highland" and the formation of coal and salt (pp. 4 and 5) remains, we must confess, entirely obscure. Mr. Lyde's facts are, as a rule, well incorporated in paragraphs that reveal their interest and relationship. We are sure that it revolts him to put in a footnote that "onions are exported to the value of over £530,000 per annum."

It seems ungrateful to question certain passages; but what are we to understand (p. 31) by "the normal activity of glaciers intermediate in character between the dry rigidity of the tropics and the constant fluidity of the Polar regions"? Why are the Polar regions fluid; or can it be the glaciers or their activity that show this character? Surely a glacier moves from other causes than fluidity? And are glaciers more rigid in the tropics? We still wonder.

What, again, are the "volcanic upheavals" (p. 225) of Scafell, Helvellyn, Snowdon, and Cader Idris, which have had an effect upon the scenery? Surely it is the hardness of the igneous rocks that has given us their peaks and precipices. The sentence that follows should probably not be laid to the author's score. Something has certainly gone wrong with it, for we read that "the volcanic action, which was probably due to the amount of water embedded in the sedimentary 'Silurian' rock, seems to have played some part in the damming of the glacial valleys, as in the case of Derwentwater and Windermere."

In a book so full of condensed but always suggestive information, it may be easy to find small points that one would question. The fact that *Homo heidelbergensis* was found "in the Danube valley" (p. 45) would not show, as the author implies, that man originated in valley-lands. He was really found, however, high up at Mauer on the Rhine-wall, where he doubtless kept himself dry out of the swampy flats below. The Tiber valley, again (p. 85), cannot have suggested to the Romans that good roads should be made up river-valleys. Any one who has followed the Via Flaminia from Prima Porta knows how little use it has found

for the valley of the Tiber, and how the deep clefts of the streams are a hindrance to it at Cività Castellana and at Narni. It is a different matter in the Gold del Furlo, where, on the far side of the Apennines, there seems only one possible descent. Continuing the very interesting account of Italy, we fail to see how "decaying vegetation" (p. 90) affects the distribution of malaria; and Mr. Lyde must have observed that the frequency of umbrellas (p 91) is just as much a sign of a hot climate as of rain. It is a pleasure to go through this volume critically, because one learns so much upon the way. It is not meant as a compendium of elementary truisms, but as an encouragement to geographic thought. As a sharp contrast in physical conditions, Scandinavia follows upon Italy. Then, in "The Balkan Peninsula," we have a crisp little sketch of Montenegro, land and people. The "dominant note" of the book unfortunately separates it by more than two hundred pages from an equally effective sketch of Carniola and Dalmatia.

It is not likely that any one person has seen all that is here described. Mr. Lyde has gathered his material so skilfully that it is impossible to say how much has depended on personal observation. The description of the Portuguese on p. 166 is out of place in a book that should be used in the impressionable higher forms of schools, and contact with other races than our own usually smooths away a host of prejudices. The book as a whole, however, is an incentive to intelligent and thoughtful travel, and the coloured physical maps at once suggest attractive fields.

G. A. J. C.

The Nature and Origin of Fiords. By J. W. GREGORY, F.R.S., D.Sc. [Pp. xvi + 542. With 8 plates, and 84 figures in the text.] (London: John Murray, 1913. Price 16s.)

THE title and dimensions of this handsome book are a proof of the considerable interest aroused in recent years by questions of physical geography. We are still a long way from the time when the author of a first-class work of travel will be required to show some knowledge of the origins of topographic forms; but Prof. Gregory's own writings, and a general acquaintance with the methods of Prof. W. M. Davis, must surely have helped many in this desirable direction. All visitors to Norway hear something about fjords or fiords, and they will now be able to realise the extent and interest of the literature that has connected these long sea-inlets with the movements of continental margins.

Prof. Gregory dismisses at an early stage the theory that glaciers have been responsible for fjords. The Shetland Islands, for example, record a direction of ice-movement at right angles to the trend of the sea-filled valleys. "In all the fiord districts," the author urges, "of which we have adequate evidence, the fiord-valleys were excavated during the Pliocene period, so that the later ice of the Pleistocene period used the fiords and did not originate them" (p. 15). Even the thresholds where shallow water occurs in the mouths of so many fjords are not relied on as an essential character, although it is admitted that they are often due to the form of the rock-floor. Those who believe more strongly than Prof. Gregory in the potency of glacial erosion have of course found an important argument in the existence of rock-thresholds in glaciated ravines below the sea-level. Such features have been compared with much justice to the rock-

barriers of Alpine valleys, across which the post-glacial rivers have now cut their way.

We find that we have to deal with (i) "fiords," which are long and fairly straight, and which usually have parallel sides ; (ii) "fiards" (p. 67), a name that affords dangerous possibilities for the printer, though they are admirably escaped by Messrs. Hazell, Watson & Viney, Ld., in the present volume ; fiards represent the drowned regions of low coasts that are formed of hard rocks, and they usually have deep interior basins and rock-bars ; (iii) "föhrden," a name rather like the Swedish *fjarden*, the word that has been translated by Prof. Gregory as "fiards" ; the "föhrden" (p. 128) find their type in the inlets of Schleswig, which are fiards originally formed by river-erosion on a low country of soft rocks, and which commonly have alluvial bars ; and (iv) von Richthofen's "rias" (p. 69), which are "submerged valleys found between the ends of mountain-lines which run out to sea."

Those who know the pleasant wooded inlets of Schleswig-Holstein will approve the rather hesitating way in which Prof. Gregory mentions them as a separate type. The conditions of the Danish peninsula, during its recent recovery from ice-sheets and the sea, are so decidedly specialised that we need hardly extend *föhrden* as a geographical term. The distinction between fjords and rias is often difficult enough, unless we limit the former term to grooves resulting from the widening, but not too great widening, of lines of fracture.

This is practically the conclusion of Prof. Gregory. "The most typical fiord-valleys occur where wide areas have been slowly upheaved into a flat dome or arch. The slow uplift has rent the land along parallel or intersecting cracks" (p. 479). He ranges over the coast-lines of the world, and again and again describes their features from his personal observations. Though the triangular facets in the fine photograph of the Cattaro Fjord (Plate VI.) appear to be surfaces of dip and not of faulting, he usually makes a strong case for fracturing as determining fjord-trend. An interesting problem, extending the conception of marginal fractures to the continents as a whole, is stated on p. 468, but so briefly as not to be generally intelligible. Do we not, in ordinary usage, say that the earth rotates from west to east, rather than "from east to west"? The rest of the book is so clear that we should like to hear further of these matters, much in the manner of Mr. Dickson's treatment of the atmosphere in a recent book about the weather.

Geographers, geologists, and lovers of scenery will alike value this new treatise. The photographic plates are very fine, and should set even the indolent turner of pages upon paths of travel. The maps in the text are sometimes rather robust in execution, and we cannot see anything in fig. 10 to justify its introduction as evidence that the mountain-lines in Southern Peru are not parallel with the coast. The text is admirably printed, and draws the reader on throughout its five hundred pages. The author is responsible for "Bohnsland" on pp. 121 and 122 ; but the fief or *län* of Bohns is not translatable as "land." "Polje," on p. 206, should be a singular and not a plural ; if we reject the Croatian plural *polja*, geographers may wisely speak of "poljes." Is not "dolinas" similarly more correct than "dolinje," for the hollows so aptly recognised as vertical valleys by the peasant dwellers of the karstland? The mention of these names shows how wide a field is covered by a book on fjords, written by one who, in regional surveys, has emulated the exploits of Camilla.

G. A. J. C.

Researches on Irritability of Plants. By JAGADIS CHUNDER BOSF, M.A., D.Sc., C.S.I., Professor, Presidency College, Calcutta. [Pp. xxiv + 376; 190 figures.] (London : Longmans, Green & Co , 1913. Price 7s. 6d. net.)

IT is with mixed feelings that one takes up a new volume by Professor Bose. One expects to be filled with appreciation of extremely delicate experimentation and of apparatus most ingeniously devised. On the other hand, one is sure to be repelled by the curious standpoint from which the author views living organisms, a position quite impossible of acceptance by physiologists generally. In the several volumes which the author has already published we always find insistence on the view that the internal energy of the plant, such as is exhibited in movements in response to stimuli, is derived from without by the absorption of "stimuli" received from the environment in the form of light, heat, and even mechanical energy. The author never explains how the energy obtained from, say, wind and heat is stored up in the plant, nor, on the other hand, does he explain why he rejects the ordinary view that the energy exhibited by the plant is derived from the oxidation of organic material elaborated by the leaves. Prof. Bose's almost *bizarre* attitude towards living organisms is perhaps to be explained by the fact that he entered physiology from physics. It is, however, particularly unfortunate, for it is, no doubt, mainly responsible for the neglect of his work by biologists generally.

Fortunately, in the present work theory is kept in the background, though the author speaks in one place of a portion of a stimulus bringing about an immediate response, while *another portion is stored up* and causes response later, or else increases the tonic condition of the plant ! Leaving theory on one side, we find a large amount of valuable work. As was to be expected, he describes some very ingenious apparatus, his Resonant Recorder and Oscillating Recorder being particularly worthy of mention. There is very great difficulty in obtaining in the ordinary way direct records of the movements of such leaves as those of *Mimosa* and *Biophytum*, for the force producing the movements is very slight, so that the mere friction of the style on a smoked plate causes distortion, or even complete arrest of the movement. In these two instruments the difficulty is completely surmounted by making the contact between the smoked plate and writing style *intermittent*. By this means the friction is greatly reduced, the record appearing as a number of dots, and as contact occurs at regular intervals no other time-record is necessary. By means of the first of these instruments Prof. Bose has been able to show that the "latent period in *Mimosa* is $0\cdot1$ sec., and that the rate of transmission of the stimulus in the petiole may reach 30 mm. per sec. ; but is markedly retarded, and finally abolished by lowering the temperature." These results are quite incompatible with the commonly received hydro-mechanical theory of the transmission of the stimulus. They indicate that the transmission is a protoplasmic one and of a nature similar to that in animal nerve. In fact, very strong evidence is brought forward in support of a close similarity between *Mimosa* and a nerve-muscle preparation ; for the pulverius appears to behave like a contracting muscle in doing more work as the load is increased. Besides these important results there are a large number of valuable observations on multiple response to a single stimulus (the existence of this type of response the reviewer can confirm from his own observations), on polar effects of electrical currents, on the contrasted effect of anode and kathode, and on many other phenomena exhibited by motile organs. The book is certainly one that cannot be neglected by workers in the fields of either

electrical response or of irritability of plants in general. Of course many of the problems investigated lie on the border-line between physics and biology, and this volume shows clearly the advantages and disadvantages of an attack on such problems by one who is mainly a physicist. Cannot Prof. Bose, with his knowledge of physics, his great ingenuity in devising apparatus and experiments, and his interest in biological problems, find some sound biologist with whom he could collaborate in what should be an almost ideal partnership?

V. H. BLACKMAN.

Text-book of Zoology. By H. G. WELLS and A. M. DAVIES. Sixth Edition. Revised by J. T. CUNNINGHAM. [Pp. viii + 487.] (University Tutorial Press, London, 1913. Price 6s. 6d.)

IN theory, the proper persons to conduct university examinations are the teachers who have conducted the course, who already know something of the capacities and attainments of the candidates, and who can set the papers so as to make them an adequate test of the fashion in which the students have taken advantage of the range of teaching offered to them. Where the examiner knows precisely what the candidate ought to know, the apparent difficulty of the paper ought to be great, and the standard of the pass mark ought to be high. But it is a hard world, and there is competition even between universities and amongst the schools of a university.

Students who attend a teaching university expect to pass its examinations, and attain its degrees, and it simply does not do if this achievement be made too hard for them. In theory a university that was merely or chiefly an examining body, that knew its candidates only by their examination papers and the examination fees they had to tender, was a poor mechanical thing. In practice the University of London, before its translation to South Kensington and the emergence of its internal side into the arena of competition, certainly secured a very high standard of attainment from its successful candidates. However you chose to sneer at them as the products of an artificial system, you could not doubt but they had acquired a large body of exact knowledge and had attained the art of exhibiting it at the stimulus of examination papers. Many of the applicants for degrees came from the remote provinces, where they had to depend on their own unaided efforts to find in books what was necessary for the syllabus. For such persons the system of tuition by correspondence was devised, and the members of an able staff learned the special needs and difficulties of isolated students, and after such experience, wrote a set of text-books of which Mr. Wells's "Zoology" is an excellent example. It is now in its sixth edition, and has been revised and brought up to date successively by Mr. A. M. Davies and Mr. J. T. Cunningham. It must be judged entirely from its genesis and purpose ; criticisms of the system cannot be applied fairly to a book adapted to the system. From this standpoint it is almost miraculously good. It is self-explanatory, well-arranged, comprehensive, and precise. Even the diagrams are such as could be reproduced in an examination. Mr. Cunningham, perhaps, ought to have explained that his pemmican chapter on evolution was a summary of his own views rather than those of "the ablest biologists from the time of Darwin to the present day," but a fair examiner reading an answer based on the chapter would only laugh and give the necessary marks. It would be more serious, however, if an unlucky candidate were to reproduce the word " Echmodermata " for " Echinoderma."

The Wanderings of Animals. By HANS GADOW, F.R.S. Cambridge Manuals of Science and Literature. [Pp. vi + 150.] (Cambridge : University Press, 1913. Price 1s. net)

DR. GADOW'S little volume of 150 pages with 17 outline maps is a marvel of comprehensive lucidity, and in many respects the best book on the geographical distribution of animals that has been written. He begins with a just and lively account of the history of his subject from Buffon to the latest treatise, and from his criticism of his predecessors leads us gently to his own point of view. The attempt to divide the world into zoological regions of general application is doomed to failure ; even with regard to single groups such as birds, beasts, or fishes, there is the trouble that the regions must have been different at different geological epochs. Study of geographical distribution is nothing less than " the history of life in time and space." The fossil history of each group must be studied and an idea obtained as to the geographical configuration of land and water at the time of its appearance, and throughout its subsequent history. *Pari passu* there must be an investigation of animals with regard to their environment, because the power of taking advantage of land connections or other possible avenues of dispersal is limited by the presence of suitable conditions for the radiating animals.

In his second chapter Dr. Gadow discusses the effect of the environment in moulding the fauna and flora of any locality, selecting forests, deserts, and high mountains as extreme examples. He states briefly the characteristic facies of each of these regions and comments on the possibility of making the difficult discrimination between convergence and blood relationship. In a short chapter on " Spreading " he points out that the many forms with an almost world-wide, continuous distribution must be supposed to have spread from a common centre, and in simple language he enunciates the bearing of limited food-supply and progressive increase in numbers due to reproduction. After discussing the density of the existing fauna, he proceeds to give a short summary of what he conceives to have been the leading features of terrestrial geography from Permian to recent geological ages, and illustrates his views with an ingenious set of diagrams. Obviously he is on controversial ground here, but although every one will not agree with all the details he suggests, no one can dispute the almost incredible amount of information that he has contrived to pack into a short chapter.

The second half of his volume is occupied by an account of the distribution of various selected groups—Earthworms, Fresh-water Crabs and Crayfishes, Fish, Amphibians, Reptiles, Birds, and Mammals. His object appears to have been an explanation of the principles by which the subject must be elucidated rather than a detailed statement of the facts.

We regard the book as quite admirable ; experts will rejoice in the freshness and interest of the exposition, and the novice will acquire from it much knowledge and a wide grasp of how to gain more.

Penal Philosophy. By GABRIEL TARDE. English Translation by RAPELJE HOWELL. [Pp xxxii + 581.] (London : W. Heinemann, 1912. Price 20s)

THE late Prof. Tarde's well-known work, *Philosophie pénale*, appeared first in 1890, and had reached a fourth edition in 1903. The version now before us was undertaken, and has been very ably made, by the translator, and those who have

in various ways shared his labours, with the object of bringing to the knowledge of a number of readers, even larger than he had himself secured in his life-time and through his own language, the researches and the conclusions of a remarkable man.

Mr. Edward Lindsey, who contributes an editorial preface – a gracefully accomplished task, for which his own position and attainments well qualify him— reminds us that Tarde was an original thinker in three separate fields of knowledge—psychology, sociology, and criminology—and that he pursued with success the careers of magistrate, statistician, and professor of political science. It is not irrelevant to note (though in the most summary fashion) the chief phases or chapters of his full and varied life. Born at Sarlat in Southern France in 1843, and educated at the Jesuit College in that place, he early showed an inclination for philosophical inquiry. After studying law at Toulouse and at Paris, he returned to his native town to practise as a lawyer.

In 1869 he was made a judge of the Tribunal of First Instance at Sarlat, and in 1875 *juge d'instruction*. This position he occupied till 1894, when he was appointed chief of the Bureau of Statistics in the Department of Justice. Established in Paris, Tarde was soon appointed to a chair in the School of Political Sciences; in 1900 he became Professor in the College of France, and was elected to the Institute as a member of the Academy of Moral and Political Sciences.

Throughout this life, which ended in 1904, he supplemented and enriched his public and professional work by numerous writings. In 1880 he contributed to the *Revue philosophique* a series of discussions and criticism of the theories of Lombroso. He was associated with Professor Lacassaque in the establishment of the *Archives d'Anthropologie Criminelle*, and regularly contributed to this journal his life long. His books followed one another in rapid succession. *La Criminalité comparée*, the earliest, appeared in 1886, and passed through several editions The author sets out the view, which he developed and illustrated afterwards in other publications, that the criminal is a professional type, and treats crime as a social phenomenon. In 1890, when he also produced the book now given in English translation, he wrote *Les Lois d'Imitation*; in 1895 came *La Logique Sociale*, and two years later *L'Opposition Universelle*. *Études pénales et Sociales* (1891), *Les Transformations du Droit* (1894), *Les Transformations du pouvoir* (1899), *L'Opinion et la Foule* (1901), and *Psychologie économique* (1902), are among his other writings. The titles illustrate at once the wide range of Tarde's interests, and his concentration upon a single problem for the elucidation of which he was able to draw not only upon the resources of his varied learning, but upon his experience as a man of affairs.

The present volume belongs to the Modern Criminal Science Series, in which it is intended to include important treaties on criminology written in foreign languages, but presented in English Versions. It is issued under the auspices of the American Institute of Criminal Law and Criminology, which was organised in 1909 at the National Conference held in that year at the North-Western University in Chicago. A committee was entrusted with the duty of selecting works for translation, and for arranging that they should be published. The members of the committee have prefixed a brief introduction to the book now before us. They declare their opinion that "for the community at large it is important to recognise that Criminal Science is a larger thing than Criminal Law. The legal profession," they add, "in particular, has a duty to familiarise itself with the principles of that science as the sole means for intelligent and systematic

improvement of the Criminal Law." They have been well advised in selecting Tarde's Treatise, for it amply justifies their thesis. Tarde was at the same time a philosopher and a practical man ; his philosophy, based upon a wide observation, which was prompted and directed by a mind of extraordinary versatility and, what too seldom accompanies versatility, precision ; his practice was guided by his philosophy, which again was based upon a shrewd and instinctive common-sense. As M. Bergson pointed out, Tarde was not one of those philosophers who set out with a theory, and devote their labours to establishing it. He set out rather with a mind alert, individual, sensitive, and in itself so well balanced and adjusted that it made the natural and the right response to facts, seeing them justly, feeling them accurately, and interpreting them in such a fashion as to fuse them into a theory, which shows itself to be no subtle invention of the author, but rather his discovery by a law, a principle of unity and intelligibility in things themselves.

It may be expected, and it is certainly much to be hoped, that *Penal Philosophy*, not a new book, as we have seen, and already well known, may in this new form make its way among many readers whom it has not hitherto reached. Produced now, in very clear and vigorous English, for which the translator deserves high praise, it is intended primarily for lawyers, and they will no doubt welcome it ; but it will receive a welcome, we are confident, not only from them. It deserves and should receive careful attention from those who are concerned (and who are not ?) in any way with social problems and sociological investigations, from students of history and anthropology and of philosophy generally. It lends powerful support to the belief, constantly repeated, but perhaps rarely entertained with vivid conviction, in the reality and organic development of Society.

In criticism as well as in construction it is a great achievement. The question of freedom is dealt with in a vigorous and penetrating discussion ; the theoretical and practical defects, both of philosophical and of scientific determinism, are adroitly and convincingly exhibited ; the doctrine that responsibility rests upon freedom of the will is examined in an admirable analysis and rejected, respectfully but definitely. The writer recalls Rameau's distinction drawn between the "will of all" and "the general will" in an eloquent passage on the efficacy of punishments, and the reason for which they are imposed. While refusing to admit "utility" as the justification and ground of punishment, he strikes the "utilitarians" with their own weapon, and yet his thrust is as gentle as it is well aimed, for he finds in them a certain inconsistency—their plea is "æsthetic" after all. Responsibility rests, we learn, upon identity of the self and upon similarity of environment ; and to substantiate his position the writer traverses the territory of the alienist and the religious teacher ; he considers the phenomena of madness and of conversion ; and considers the question whether a new-comer to a society, the traditions of which are wholly unlike those in which he was himself brought up, can be regarded as "criminal" if he violates its rules and customs.

It is not necessary to agree with Tarde in order to admire him ; it is impossible to read a page of his work without receiving the stimulus which is to be derived from witnessing the operations of an intellect, fearless and generous, as it occupies itself with problems which, while they specially attract students in a particular field of human inquiry, get their significance for such students because they touch human experience and rouse human inquiry beyond the borders of any single profession.

E. T. C.

The Wonders of Wireless Telegraphy. By H. A. FLEMING, M.A., D.Sc., F.R.S. [Pp. xi + 279.] (London : Society for Promoting Christian Knowledge, 1913. Price 3s. 6d.)

THERE are very few scientific men of eminence who have the gift of making the most abstruse subjects clear and interesting to the multitude. Prof. Fleming is happily one of these, and his little book on the Wonders of Wireless Telegraphy is not only a model of clear exposition, but is an example of how to select and arrange just the features of interest which the ordinary reader desires to hear about. If we add to these points yet one more, and probably the most important of all, that there is from cover to cover of the little book neither a loose statement nor an unfounded assertion, and that there is nothing in the way of unsound popular science, it is clear that this book may be warmly and heartily recommended to readers young and old.

The first chapter, dealing with the æther, electricity, and electrons, commences with a brief account of what we know of the æther itself, treating historically the different steps in the discovery of the velocity of light, and putting clearly and concisely the electrical and optical phenomena which science helps us to understand, and a knowledge of which is the first step in a study of the nature of the æther itself. An interesting and popular account is given of the researches of Thomson, Rutherford, Soddy, and M. and Madame Curie. Having given an outline of the constitution and structure attributed to the universal space-filling æther, and the way in which the electricity atoms are now supposed to be built up from it, the next step is naturally to discuss electric waves and oscillations, and the second chapter deals with these oscillations, making use of the hypothesis of the lines of strain or lines of force, and the electrons or strain forms or centres from which twists or waves in æther start. In this chapter is clearly shown the difference between damped intermittent oscillations and undamped oscillations, and the way in which lines of electric force surround the Hertzian Oscillator.

The third chapter deals with actual wireless telegraph instruments, and the sending of wireless messages ; but the author leads up to this important subject by a description of signalling both in the Army and Navy, and continental practice and mode generally, in which intelligence is transmitted at a distance by signals, particularly the Morse Code, giving as prelude to wireless telegraphy the more simple case of telegraphy in which wires are used. This is followed by a description of various types of antenna or aerial used with wireless telegraphy. Also a special account is given of the Marconi discharges and arrangements for producing persistent oscillations by the electric arc.

The next chapter deals generally with the subject of force receivers as distinguished from transmitters, showing how connections are made with the receiving circuits. It is explained how with a combination of a telephone in series with an electrical valve a telephone can make audible the sparks of a transmitter hundreds of miles away.

The Marconi magnetic detector and coherer is described, and the author's own cymometer. The two final chapters deal with wireless telegraphy over land and sea, the transmission of wireless waves around the world, wireless telegraphy and telephony in practice, and the utilisation of electromagnetic waves.

It may be remarked—and this is a matter of no small importance—that the illustrations of the book throughout are excellent ; and the diagrammatic figures, many if not most of which are new, tend to make the descriptions of the text admirably clear.

Mechanism, Life, and Personality. By J. S. HALDANE, M.D., F.R.S. [Pp. vi + 139.] (John Murray. Price 2s. 6d.)

THIS small volume consists of four lectures delivered to senior students of the London University, and is an attempt to bring the great biological movement of the nineteenth century into definite relation with the main stream of human thought.

Dr. Haldane has proved himself well fitted for the task, for he, unlike many men of science, never loses himself in "the snare of words" as Locke called it, and has dealt with a somewhat abstruse subject in an admirably simple and lucid manner. The arguments are so clear, that even those wholly unversed in philosophy will have no difficulty in following it to its striking conclusion.

The aim of the first two lectures is to examine the hypothesis that living organisms may be regarded as conscious or unconscious physical and chemical mechanisms, and can be satisfactorily investigated from that standpoint.

The author first states the case of those who hold that the two great physical laws of the conservation of matter and the conservation of energy can be extended with apparently rigorous accuracy to all living mechanisms. We now know, as the fruit of years of experiment and observation, that nowhere does simple protoplasm exist, not even among the lowest and most primitive saprophytic bacteria.

The Mechanistic Theory is obliged to assume that a living organism, such as man, is a complex system of physico-chemical mechanisms, each of which is controlled by the rest in such a way that the normal structure and activity of the organism is, under ordinary conditions, maintained. Many of these mechanisms have been proved to exist by exact experiment, and hence no real difficulty presents itself in the assumption. The fundamental mistake of the mechanistic physiologists of the middle of the last century was that they completely failed to realise that living structure was organised, and such processes as secretion, absorption, growth, were treated as if each were an isolated physical or chemical process instead of being one side of a many-sided metabolic activity, of which the different sides are indissolubly associated.

Dr. Haldane, by some admirably destructive criticism, disposes of the mechanistic theory, and leaves us fully convinced of the inadequacy of that theory to explain the phenomena of Life.

Scientific materialism superseded the scepticism of the Victorian era, and now we are told on many sides that the trend of modern philosophic thought is in the direction of some form of vitalism. It is no longer widely held that "a generation which speculates upon the unknowable sacrifices progress for safety." Dr. Haldane, in his fourth and concluding lecture, comes down definitely on the side of a fundamental dualism. It is, he writes, necessary to draw a sharp and clear distinction between biology which deals simply with organic life, and psychology which deals with conscious life or Personality. He holds that the physiologist, who treats of perception and volition, is going outside his own subject endeavouring to explain psychological phenomena in terms which cannot be applied to them. We are thus led on gradually until we find ourselves compelled to adopt the spiritual hypothesis, an attitude of mind with which Henri Bergson, Poincaré, and others of the modern French philosophic school have made us familiar.

Dr. Haldane concludes with an admirably clear summary of his views on the whole subject:

"The relation of a person," he writes, "to his surrounding world with which he is in contact, through perception and volition is not a mere external relation, since his surrounding world is teleologically determined in relation to his organic life. It is a mere illogical illusion to regard the world we perceive as independent of its relations to us in perception and volition. The visible world around us is a world moulded by our own personality, and there is no other world. In scientific work we can abstract from, or disregard, the psychological aspect of things, but in so far as we do so we are dealing with abstractions. The relations of personality, mere organism, and matter are relations of increasing abstraction from reality.

"Just as the individual organism can only be understood as participating in a wider life, so the individual person exists only in participating in a wider personal existence. He can only realise his true personality in losing his personality as a mere individual. Personality is the great central fact of the universe. This world, with all that lies within it, is a spiritual world." R. F. O.

The Realm of Nature: An Outline of Physiography. By HUGH ROBERT MILL, D.Sc., LL.D. [Pp. xii, 404, with Illustrations.] (London : John Murray. 2nd Edition, 1913. Price 5*s.* net.)

THIS is a second and revised edition of a well-known book, which was originally published in 1891 and which has been previously reprinted six times. We need not be surprised at its popularity, because it really gives concisely a very excellent review of much of our present knowledge of Nature. It is advisable to print the following complete list of the headings of the chapters in order to indicate the scope of the work. These are : The Study of Nature ; The Substance of Nature ; Energy, the Power of Nature ; The Earth a Spinning Ball ; The Earth a Planet ; The Solar System and Universe ; The Atmosphere ; Atmospheric Phenomena ; Climates of the World ; Weather and Storms ; The Hydrosphere ; The Bed of the Oceans ; The Crust of the Earth ; Action of Water on the Land ; The Record of the Rocks ; The Continental Area ; Life and Living Creatures ; Man in Nature. The large number of maps and illustrations are a great additional attraction ; and the whole book, cheap as it is, is one which can well be read, not only by young people, but by scientific men who wish to know about things outside the small tracts to which science condemns them too often to confine their labour. An additional chapter upon what is now known regarding the causation of the infectious diseases would have been useful, but it may have been felt that this was somewhat outside the range of physiography. The whole book gives us what is really indeed a bird's-eye view of natural knowledge ; and the student can afterwards explore what regions he pleases in more detail.

The Ocean. A General Account of the Science of the Sea. By SIR JOHN MURRAY, K.C.B., F.R.S., LL.D., D.Sc., Ph.D. [Pp. 256 with 12 Plates.] (London : Williams & Norgate. Price 1*s.* net.)

THE British public ought to be the best educated people in the world, because they can obtain information from the most highly qualified experts on almost any subject for the sum of one shilling. This little book by Sir John Murray raises the question whether higher education is not most economically given by works of this nature. It is really an admirable summary of the subject. Beginning with some historical notes and a brief account of the various methods and instruments used for deep-sea research, it proceeds to consider the depth of the ocean, its waters, salinity, gases and temperature, compressibility, pressure, colour, viscosity, penetration of light, tides, waves and seiches. The oceanic currents are lucidly described, and the remainder of the book deals with life in the ocean and marine deposits, etc. There is a glossary, a bibliography, and an excellent index ; and also some useful maps and figures. The book is very carefully written and

printed, and can be recommended for all readers from boyhood even to the mature years at which the scientific mind is supposed to reach its zenith of power. The account of the flora and fauna of the ocean, however, would have been somewhat clearer if the various groups had been considered in some better biological order.

The Meaning of Evolution. By SAMUEL CHRISTIAN SCHMUCKER, Ph.D. [Pp. 298.] (London : Macmillan Co., 1913. Price 6s. 6d. net.)

A VERY clearly written popular exposition of evolution. The book begins with a history of the development of our ideas on the subject from the time of the Greek philosophers up to Cuvier and Lamarck, and then gives in much greater detail the life and work of Darwin. Next the various strands of thought which make up the total conception are well analysed and explained, with many examples which will be of interest to all readers. The work also contains numerous references to interesting special discoveries, and has an excellent chapter on evolutionary theories since Darwin. This would have been much better, however, if there had been some account of Mendelianism. The book concludes with a theistic chapter. It is a very sane and lucid abstract of the subject, and will be useful to all general readers.

Life, Light, and Cleanliness. A Health Primer for Schools. Published under the Director of Public Instruction, Punjaub. [Pp. 126.] (Lahore : Rai Sahib M. Gulab Singh & Sons, 1912. Price 8 annas.)

THIS little book is perhaps the very best primer ever published for teaching sanitation in Indian schools, or even in any schools in the tropics. Although it has been published anonymously, it is written by Major E. L. Perry of the Indian Medical Service. A large number of primers of this kind are on the market ; but Major Perry's booklet has the great advantage that it is put in the form almost of the Arabian Nights Entertainments. The story is that of a conversation between a Rajah of India, "who ruled over a very fine country," and various individuals, such as merchants and physicians. In consequence of these conversations the Rajah sent his eldest son to investigate matters of health in a neighbouring country, where the people " were a fine sturdy race because from childhood up they obey the rules of health." The details of this journey are so interesting that children will read them with pleasure, and will learn everything about sanitary matters *en route*—mosquitoes and malaria, the taking of quinine, the causation of plague by rat-fleas, and the mode of spread of cholera. These become fixed in the mind of youth in a manner which, we fear, is not done by the much more stately but less effective works of formal sanitation which are usually disseminated amongst the public. We should like to see this work translated into many languages, and cast broadside throughout the schools in the tropics. The Director-General of the Indian Medical Service pointed out in SCIENCE PROGRESS for October, 1913, the difficulties with which sanitation in India is confronted in consequence of the ignorance of the native population— and the European population is not always very much better. This little book ought to do much to remove that ignorance.   R. ROSS.

Panama : The Creation, Destruction, and Resurrection. By PHILIPPE BUNAU-VARILLA. With Portrait of Author and numerous other portraits, plates, and figures referring to the Panama Canal. [Pp. xx + 568.] (London : Constable & Co., 1913. Price 12s. 6d.)

WE have space only to refer briefly to this great book—written in English and in French by the author himself, who was one of the principal moving spirits in the

construction of the Canal. The book is a great one because it recalls a great work. M. Bunau-Varilla went to Panama at the age of twenty-five in the year 1884, and, owing to the death of senior officials, soon found himself Acting Director of the works. Owing to his great ability and energy the labour progressed rapidly under the French management, in spite of the terrible mortality from yellow fever. The book records in the admirable French style all the difficulties which were contended with, and the part played by him, not only in the early developments, but in the later negotiations resulting in the retention of the Panama Canal Zone as the site of this magnificent enterprise. I may venture to say here that when I was in Panama in 1904, the Americans were loud in their praises of the previous works of the French—with which the author was so gloriously connected. The book contains many points which will interest all scientific men, especially engineers, and those medical men who have been connected with the prevention of disease in the tropics—though, of course, the author, being an engineer, does not deal very specifically with this part of the subject, beyond giving us an idea of how yellow fever impeded the work in the early days. Such books, being records of great work done, should be read by every one.

<div align="right">R. ROSS.</div>

Experimental Domestic Science. By R. HENRY JONES, M.Sc., F.C.S., Chemical Department, Harris Institute, Preston. [Pp. ix + 235.] (London: W. Heinemann, 1912. Price 2s. 6d.)

THE promotion of domestic science is one of the most promising of recent developments in modern education. It is all-important to have science, and especially that of hygiene, applied to the home ; but the chief centre of science in every home is undoubtedly the kitchen, and this well deserves to be elevated to a higher position, and to receive more attention and study than it has done in the past. It is there that energy is developed and health maintained, and without due regard to proper foods and their careful preparation it is almost impossible to have either a healthy or a happy and contented household.

Any work, therefore, which contributes tangibly to attain this end is worthy of consideration, and that which has just been issued by Mr. R. H. Jones is a very practical and useful addition to the limited facilities now at the disposal of the student. It is chiefly devoted to explaining the character, and giving the constituents, of the different foods in every-day consumption and use, while also showing the chemical changes that take place in the various processes of cooking. This is very valuable and much-required information, and Mr. Jones gives it in a simple, concise, and explicit form. A series of experiments are offered throughout the book, which can be tried without any expert knowledge of chemistry, and these should add considerable interest to the study of the subject while impressing facts and results upon the memory. A number of the experiments have been tested in the laboratory of the Institute of Hygiene, and they have been found to be invariably accurate and quite reliable. There is, further, a series of questions at the end of each chapter which should prove very useful when the work is adopted as a class-book in schools.

It is not claimed that "Experimental Domestic Science" deals with such a wide subject comprehensively—that would be impossible within the limitations of a comparatively small volume ; but, as far as it goes, it is well done, and it is a praiseworthy effort to bring science and the home into closer touch. It represents a branch of study yet in its infancy, and, while it is written for and fully

meets the requirements of our present state of progress, it is to be hoped, and we anticipate, that extension on the same lines and a work more advanced will be a pressing need of the future.

J. GRANT RAMSAY

(Incorporated Institute of Hygiene).

BOOKS RECEIVED

(Publishers are requested to notify prices)

The Hindustan Review. Vol. xxviii. No. 167. Verbatim reprints of the Indian Law Reports (1876–1900) in the Indian Decisions (new series). Published by I. A. Venkasawmy Row and T. S. Krishnasawmy Row, The Law Printing House, Mount Road, Madras. (Pp. 540.) Price 10 Annas.

Problems of Genetics. By William Bateson, M.A., F.R.S., Director of the John Innes Horticultural Institution, Hon. Fellow of St. John's College, Cambridge, and formerly Professor of Biology in the University. With illustrations. Humphrey Milford, Oxford University Press, London, E.C., and at Toronto, Melbourne, and Bombay. (Pp. ix, 258.) Price 17s. net.

Fortschritte der Naturwissenschaftlichen Forschung. Edited by Prof. Dr. Emil Abderhalden, Direktor des Physiologischen Institut der Universität, Halle a.S. With 102 Illustrations and 2 Plates. Vol. 9. Urban & Schwarzenberg, Berlin and Vienna, 1913. (Pp. 280.) Price 17 marks.

Rubber and Rubber Planting. By R. H. Lock, Sc.D., Inspector H.M. Board of Agriculture and Fisheries, Sometime Scholar and Fellow of Gonville and Caius College, Cambridge, and Assistant Director of Botanic Gardens, Ceylon. Cambridge University Press, Fetter Lane, E.C. Crown 8vo. (Pp. xiv, 246.) With 10 Plates and 18 Text Figures. Price 5s. net.

Hope and Health. Golden Advice to Overcome the Drink Habit. By " One who Cured Himself." London: A. M. King & Co., Wine Office Court, E.C. (Pp. 44.) Price 1s.

Science of Nature-History. A Line of Study for Assigning Places to all Events in Creation in Order of Time showing their Genesis, which may Define Themselves. A Guide to Systematise Knowledge. By Nasarvanji Jevanji Readymoney. Bombay: The *Times of India* Office ; London Agency, 121, Fleet Street, E.C., 1907. (Pp. 103.)

Metallography. By Cecil H. Desch, D.Sc. (Lond.), Ph.D. (Würzb.), Graham Young, Lecturer in Metallurgical Chemistry in the University of Glasgow. With 14 Plates and 108 Diagrams in the Text. Second Edition. Longmans, Green & Co., 39, Paternoster Row, London, New York, Bombay, and Calcutta, 1913. (Pp. x, 431.) Price 9s. net.

Die Physik der bewegten Materie und die Relativitätstheorie. Von Max B. Weinstein. Leipzig, 1913 : Verlag von Johann Ambrosius Barth. (Pp. xii, 424.) Price 17 marks.

A Dictionary of Applied Chemistry. By Sir Edward Thorpe, C.B., LL.D., F.R.S., Emeritus Professor of Chemistry, Imperial College of Science and Technology, South Kensington, London ; Late Principal of the Government Laboratory, and a Past President of the Chemical Society and of the Society of Chemical Industry. Assisted by Eminent Contributors. Revised and Enlarged Edition. In 5 Vols. Vol. v. With Illustrations. Longmans, Green & Co., 39, Paternoster Row, London, New York, Bombay, and Calcutta, 1913. (Pp. 830.) Price 45s. net.

Scienta (Rivista di Scienza), Organo Internazionale di sintesi scientifica (International Review of Scientific Synthesis). Editors, G. Bruni, A. Dionisi, F. Enriques, A. Giardina, E. Rignano. Vol. xiv., Year VII. Bologna : Nicola Zanichelli. London : Williams & Norgate. French Translations of Italian, German, and English articles. (Pp. 251.)

Spencer's Philosophy of Science. The Herbert Spencer Lecture, delivered at the Museum, November 7, 1913, by C. Lloyd Morgan, F.R.S. Oxford : At the Clarendon Press. (Pp. 53.) Price 2s. net.

American Chemical Journal. Ira Remsen, Editor. Charles A. Rouiller, Assistant Editor. November, 1913. Vol. l. (Pp. 82.)

Les Zoocécidies des Plantes d'Europe et du Bassin de la Méditerranée. Description des Galles. Illustration. Bibliographie détaillée, Répartition géographique. Index bibliographique. 1567 figures dans le texte, 3 planches hors texte, 8 portraits. Tome Troisième. Supplément, 1909-12. Nos. 6240 à 7556. Librairie Scientifique. A. Hermann et Fils, 6 Rue de la Sorbonne, Paris (V.), 1913. (Pp. 310). Price 10 francs.

Text-Book of Paleontology. Edited by Charles R. Eastman, A.M., Ph.D. Professor of Paleontology in the University of Pittsburgh and Curator at the Carnegie Museum, Pittsburgh. Adapted from the German of Karl A. von Zittel, late Professor of Geology and Paleontology in the University of Munich. Second Edition, revised and enlarged by the editor in collaboration with the following named specialists : R. S. Bassler, W. T. Calman, A. H. Clark, H. L. Clark, J. M. Clarke, J. A. Cushman, W. H. Dall, A. Handlirsch, R. T. Jackson, A. Petrunkevitch, P. E. Raymond, R. Ruedemann, C. Schuchert, J. P. Smith, F. Springer, T. W. Vaughan, C. D. Walcott. Vol. I. with about 1,600 Illustrations. London : Macmillan & Co., Ltd., St. Martin's Street. (Pp. xi, 839.) Price 25s. net.

Cabinet Timbers of Australia. Technical Education Series, No. 18. Technological Museum, Sydney. By R. T. Baker, F.L.S., Corr. Memb. Phar. Soc. Great Britain, Curator and Economic Botanist. Published by the authority of the Government of the State of N.S.W. Sydney: W. A. Gullick, Printer, 1913. (Pp. 186.)

Modern Astrology. The Astrologers' Magazine, Christmas Number. Edited by Alan Leo. *Modern Astrology* Offices, Imperial Buildings, Ludgate Circus, E.C. (Pp. xi, 49.) Price 6d.

Logic. Vol. i. Encyclopædia of the Philosophical Sciences. By Arnold Ruge, Wilhelm Windelband, Josiah Royce, Louis Couturat, Benedetto Croce, Federigo Enriquez and Nicolaj Losskij. Translated by Ethel Meyer. London : Macmillan & Co., Ltd., St. Martin's Street, 1913. (Pp. vi, 268.)

Philosophy of the Practical, Economic and Ethic. Translated from the Italian of Benedetto Croce by Douglas Ainslie, B.A. (Oxon), M.R.A.S. London : Macmillan & Co., St. Martin's Street, 1913. (Pp. xxxvii, 590.) Price 12s. net.

The Diseases of Tropical Plants. By Melville Thurston Cook, Ph.D., Professor of Plant Pathology, Rutgers College, formerly Chief of the Department of Plant Pathology for the Republic of Cuba. London : Macmillan & Co., Ltd., St. Martin's Street, 1913. (Pp. vi, 317.) Price 8s. 6d. net.

The Progress of Scientific Chemistry, in Our Own Times. With Biographical Notices. By Sir William A. Tilden, F.R.S., D.Sc. Lond., Sc.D. Dub., D.Sc. Vict., LL.D. Birm., Fellow of the University of London, Corresponding Member of the Imperial Academy of Sciences, St. Petersburg, formerly Professor of Chemistry and Dean of the Royal College of Science, Professor-Emeritus in the Imperial College of Science and Technology, London. Second Edition. Longmans, Green & Co., 39, Paternoster Row, London, New York, Bombay, and Calcutta, 1913. (Pp. x, 366.) Price 7s. 6d. net.

Handbuch der Hygiene. Edited by Prof. Dr. M. Rubner, Geh. Medizinalrat, Berlin ; Prof. Dr. M. S. Gruber, Obermedizinalrat, München ; and Prof. Dr. M. Fischer, Berlin. Vol. 3, Third Part. Die Infektionskrankheiten, Pathogene tierisch Parasiten (Protozoen, Würmer, Gliederfussler). With 192 figures and 32 Coloured Plates. Leipzig: Published by S. H. Hirzel, 1913. (Pp. 390.) Price 24 marks.

CORRESPONDENCE

"MAN AND HIS FORERUNNERS"

To the Editor of "Science Progress"

SIR,—

I have been asked by Prof. v. Buttel-Reepen to correct an error in the review of his book, *Man and His Forerunners*, which appears in the October number of SCIENCE PROGRESS. Your reviewer states that we "go the whole way with Rutot." It is not clear to what this vague statement refers, since Dr. Rutot necessarily "goes" in different directions on different subjects, but if (as appears probable) it refers to that scholar's well-known advocacy of Oligocene eoliths, it is quite erroneous. The question of Oligocene eoliths is discussed on p. 11, and the author rejects their claims, stating that he believes the Upper Miocene to be the oldest stratum in which worked stones have been found. It is true that the "some experts" who believe in the genuineness of the Oligocene specimens are not mentioned by name, but of course we cannot suppose Prof. Elliot Smith to be ignorant of the fact that Rutot is one of the chief upholders of this doctrine. So far from going the whole way with Rutot on this question, Prof. v. Buttel goes no more than half the way with that authority.

Several of Prof. Smith's other comments are misleading, and I may add that the opinions expressed in the book are not necessarily in all cases those of the translator.

Yours faithfully,

A. G. THACKER.

GLOUCESTER, *November* 15, 1913.

To the Editor of "Science Progress"

SIR,—

I accept Mr. Thacker's correction that "Prof. v. Buttel-Reepen goes no more than half way with Rutot"; but at the same time I do not think such an arithmetical qualification seriously affects the real meaning of my criticism. Prof. v. Buttel-Reepen leaves the solid ground of fact (*i.e.* that no unquestionable human remains or certain evidence of human workmanship has been found except in the Pleistocene, so that even to postulate the existence of man in the late Pliocene is straining inference to its uttermost) and when he takes the plunge into the waters of unrestrained conjecture it does not matter much whether he floats in the Upper Miocene or dives into Oligocene, or even Eocene, depths. In either case he is in the water with Rutot.

Yours faithfully,

G. ELLIOT SMITH.

THE UNIVERSITY OF MANCHESTER, *November* 17, 1913.

To the Editor of "Science Progress"

DEAR SIR,—

Will you allow me to correct a misprint in your last issue. In the footnote on p. 263 occurs the term *bad* ratios; it should be *lead* ratios. The value of this method I hope to deal with on a future occasion.

Yours truly,

H. S. SHELTON.

NOTES

The Finances of Tropical Medicine

Whether Britain has taken as leading a place in the great world-movements of recent years as she did in those of last century may perhaps be doubted ; but she has certainly done so in the line of tropical medicine.   Summed up, the recent work in this branch of science has resulted in the finding of the cause and mode of propagation of many of the most important tropical diseases—a discovery which is obviously of fundamental value as regards the development of more than half the world.   It will therefore be of interest to all scientific men to learn some facts regarding the finances of the movement.

Putting aside the large work done by the public medical services and by foreigners, we shall touch only the work of the two schools of Liverpool and London, which were founded in 1899, and have consequently been in existence for fourteen years. From figures which are probably as accurate as can be obtained, we gather that the Liverpool School has collected from the beginning a sum of about £130,000, entirely from private sources, including bequests, special subscriptions, annual subscriptions, donations for the founding of Chairs, students' fees, etc.   In addition to this it has received over £8,000 from Government. The London School appears to have received about £133,000 from such private sources as those mentioned above, and also about £22,000 from various Government sources.   Thus the contributions by Governments amount to about £30,000 during the fourteen years, against a sum of about £264,000 contributed from private sources and students' fees ; so that the Government contribution comes to a little over 10 per cent. of the total receipts of the two schools (£294,000).   For this, our tropical possessions have received very great benefits, including the carrying out of many expeditions and of innumerable researches on tropical medicine, the permanent establishment of two Schools of Tropical Medicine with buildings and two endowed Chairs, the

instruction of about two thousand medical men, including officers of the Services; the maintenance of experts to advise on official committees and on sanitary matters, the establishment of special museums, and the publication of scientific journals. In most other countries, all these expenses would have been met out of Government funds; and it must be admitted that British administration is fortunate in that it can persuade private persons to help it in such matters on such a large scale.

The total sum of money appears to be large, although it is less than the fortune of hundreds of private citizens in Britain. As a matter of fact, the work could not have been done so cheaply but for the circumstance that most of the workers have been content to sacrifice their time and themselves for the public benefit by accepting extremely small payment. The highest salary given at either of the Schools has reached only to £800 a year, and that was continued only for three years. The most serious aspect of the business is that no attempt has yet been made to lay down pensions for any of the workers except small ones in connection with the two Chairs. To put it briefly, this great imperial work has really been carried out, not only by the genius of the workers, but very largely at their own pecuniary expense—a thing which can only be described as being rather dishonourable for a country which is so wealthy as Great Britain. The fact is that this country has come to believe that it will receive almost all its "medical benefit" for nothing. The poor have become accustomed to receiving treatment in hospitals for nothing; the well-to-do frequently escape paying their doctors' fees; and it is scarcely proper that the great British Empire itself should be under the impression that it may adopt the same attitude towards those who have benefited it in the line of tropical medical science.

The result is as may be imagined—that good workers are becoming increasingly difficult to obtain, for the simple reason that the work does not pay. Though Britain probably has greater opportunities for such researches than all the other nations put together, her output of labour in this line is falling below such a standard. Thus, out of two hundred articles considered in Numbers 9 and 10 of the *Tropical Diseases Bulletin* (October and November 1913), only 47 were by British workers; and what work is done is too often of the nature of hasty observation or immature hypothesis. Unless a remedy can be found,

the movement is not likely to continue to prosper so far as the British Empire is concerned.

Eugenics and War

In the *Times* of October 15, Prof. Carl Pearson published an important letter on the position of eugenics as a science, in which he criticised the present tendency to publish premature theorems upon this subject. He admitted that Sir Francis Galton thought that progress towards increased race efficiency should be made by two routes, namely (1) by the scientific study of heredity and environment as they bore on racial development, and (2) by a popular movement emphasising the importance of these factors in national welfare and urging their proper appreciation by legislators and social reformers. Prof. Pearson now thinks that the latter line of work is being rather overdone. Eugenics, he thinks, "has become a subject for buffoonery on the stage and in the cheap press," and he adds that "eugenics is rapidly developing into a topic for the *poseur*, the *Kongressbummler* and paragraphist"; and he gives instances of fallacious dogmas which are being put about. His warning is a timely one—especially in view of such a "Criticism of Eugenics" as is given by A. M. Carr-Saunders in the October number of the *Eugenics Review*. Indeed, the same number of the *Eugenics Review* contains an address on the Eugenics of War by Chancellor Dr. David Starr Jordan of Stamford University, U.S.A., to which Prof. Carl Pearson's criticism appears to be most pertinently applicable.

Chancellor Jordan's position is that "the effect of war on nations is to spoil the breed, by the very simple process of the reversion of selection . . . because the result of it would be that the nations would breed from inferior stock, that the strong men would be destroyed, or kept from marriage, and those at home— those that war could not use—would be the parents of the next generation"—an opinion which he attributes to Benjamin Franklin. "If," he says, "a nation has destroyed its bravest, its most courageous, its most soldierly men, it will cease to breed that kind of man. If a nation destroys its men who are over six feet high, in time it will not have many men who will reach that stature." Thus, he argues, war must be disastrous to a nation which indulges in that business—which many of the philanthro-

pists of Britain and America seem to look upon as being a kind of pernicious pastime easily capable of suppression. " If you go over the history of nations," he says, " you will find that the downfall of any nation arises through the gradual weakening of its people, and the gradual rise of the dominance of the ruling power. That has gone on in proportion to the destruction of the strong and the fit." He gives some attempts at historical proof of this hypothesis ; but on examination they can scarcely be called satisfactory ; and indeed other instances suggesting the opposite conclusion can easily be cited. For example, one of the most virile periods of English history was that which followed immediately upon the dreadful Wars of the Roses. The greatest development of Prussia followed shortly after the wars of Frederick the Great. Rome reached her highest stage at a time when her individual citizens were themselves most engaged in fighting and began to decay as soon as they conducted their wars more by the help of mercenaries. Perhaps, however, the most remarkable historical instances of the good effect of war upon national physique are to be found in the cases of the Zulus and Masai of Africa, who were well known to be at once the finest and most warlike men. The Sikhs of India were originally a religious sect simply drawn from the surrounding population but obliged to defend themselves by incessant fighting from the attacks of their neighbours. Nevertheless they became and are the finest men in India, as admitted by the British soldiers who recruit and train Indian troops. Another example is the case of the mountaineers on the north-west frontier of India—men who look upon fighting as a pleasure, and have been engaged upon it amongst themselves from time immemorial, and yet possess a magnificent physique and morale far superior to that of the plain-dwellers. At the same time the Japanese and the Indian Ghurkas, who have also done much fighting, are small people, but remarkably warlike and efficient nevertheless. On the other hand, some of the feeblest races are those which never fight if they can help it, or always run away in order to live and fight another day. According to Chancellor Jordan, these should be the big and strong men, whereas the Zulus, the Sikhs, and the Afridis should be small and feeble.

Of course the subject, vastly important as it is as regards the whole theory of civilisation, is much more complex than he seems to imagine. It is quite possible that hand-to-hand and modern

fighting may have different effects upon racial development. The former was indeed likely to have precisely the opposite effect to that imagined by Dr. Jordan, because the weak men would be killed out by the fighting, especially where all the males are forced into war. It may be more plausibly argued that in modern war the big, the courageous, and the dutiful men are more likely to be selected as soldiers, and are therefore more likely to be killed in battle; but this would apply chiefly to nations which adopt voluntary service and not to those which adopt universal service. But even here there are qualifying considerations. In modern war the greatest mortality is due, not to the fighting, but to diseases. In fact, the subject is much too complex for such treatment as the Chancellor of Stamford University seems to think. Moreover, universal military training may possibly have such a good effect as will swamp the occasional loss of good men in the comparatively rare moments of war; while, lastly, we have no scientific grounds for assuming the general eugenic law which he appears to accept. The children of weak men are not always strong nor the children of strong men weak. It is possible that the training, the exercise, the stimulation of all effort caused by war do far more towards raising the physique and morale of a nation than the selective slaughter of some of the better individuals does towards depressing them. In our last number we drew attention to the fact that the French and the Germans have obtained leading places among the great nations as regards the scientific Nobel Prizes; and yet these nations are precisely those which were engaged most in the numerous wars of last century. In fact a general survey of human history appears to lead us to a conclusion precisely opposite to that arrived at by Dr. Jordan. War is a dreadful thing; but nevertheless it may quite possibly be utilised by nature for raising racial standards; and the first concern of science is to ascertain truth.

The University of Bristol

Owing to the energetic action of the *Athenæum*, which has devoted weekly articles to the affairs of this University, a resolution was moved at the Meeting of the University Court held on November 14, begging that the Council of the University be asked to inform the Court fully of the circumstances connected with the appointment of Prof. Cowl. The resolu-

tion was rejected by a large majority, and another motion, namely, "that a full public inquiry take place into the serious charges of maladministration in the University," was ruled out of order by the Chairman. The *Athenæum* has expressed itself very clearly to the effect (and we agree with it) that the action of the University Court cannot be looked upon as being satisfactory or final. Moreover, only one of the doubtful points in the action of the University was referred to at the meeting. It will be remembered that the allegations are (1) that one of the professors was dismissed under circumstances which suggest incorrect procedure or unfair treatment, or both; and (2) that the Council bestowed honorary degrees, not previously recommended by the Senate, largely upon its own members. It is unfortunate for the interests of university life in general that no proper inquiry upon these allegations can be obtained.

Science and the Lay Press (Gordon D. Knox, *Morning Post*).

"Nations," in the words of the last number of SCIENCE PROGRESS, "no more than individuals can be allowed to remain ignorant, sluggish, and unscientific." If the statement is to be something more than a pious opinion, means will have to be devised for the education of the public in the results and in the methods of science. The complete victory of the Huxley School of Thought over the older School of Theology has had the unfortunate effect that the missionaries of science have found themselves without an objective. Lacking the stimulus of opposition they have left the market-place for the laboratory, and, science being kept only spasmodically before the public eyes by such sectional disputes as centre in the question of vivisection, the public is ignorant of its aims and indifferent as to its condition. The inadequacy of the payment of scientific men, the lack of funds for research, and the apathetic attitude of the Government to the needs of science are the reflection of the public indifference; and if this attitude of mind is to be changed it will have to be done largely through the public press.

The misunderstanding between the press and men of science is so complete that it may be well to put forward a few of the principles that must inevitably govern any attempt that is made through the columns of the press to enlist the public interest and

support. Men of science must abandon once and for all the idea that the newspapers can be induced to publish articles of the type that could be derived from the evidence of such Blue Books as the Report of the Vivisection Commission. The historical article is a thing of the past, and the history of scientific achievement can only find a place in the columns of a paper in connection with Centenary Celebrations and public events of current interest. To give an illustration. It is of no use to ask the editor of a daily paper to publish afresh the dramatic story of the victory over yellow fever. It is true that nine-tenths of the public are entirely ignorant of it, but the experience of the journalist shows that that same nine-tenths will not read it if it comes before them in the form of an independent article. When the Panama Canal, however, is opened to traffic the public will wish to read of how that engineering feat has been accomplished and it will then be possible for the daily papers to deal with such a subject as that of yellow fever in a way that the public will read. With the way cleared by the ruling out of the article of the historic type, one may consider in what way newspaper co-operation can be looked for. The essential thing to remember is that the primary function of the newspaper is to disseminate news, and that if the work of science is to be reported it must be done in the guise of news.

At the present time few of the learned societies or institutions attempt to co-operate with the correspondents of newspapers. And yet from time to time each of these societies has before it papers that are of great public interest, papers, that is to say, which chronicle an advance in some direction and that by careful handling can be brought into relation with the stock of ideas possessed by the ordinary reader. It is within the knowledge of the secretaries of the societies when such papers are to be read ; but I believe there is not a single case where the governing body of a society or an institution has made it an instruction to its secretary to look out for the reading of such papers and to warn the press that there will be news for them if they care to attend.

While the majority of the scientific societies are self-supporting organisations, and therefore under no obligation to the press, the same is not the case with the majority of the great scientific institutions. These are supported largely by public funds, the money being derived from the Government, from local authorities,

from popular subscriptions, or from the donation of some wealthy individual. The mere fact of a newspaper existing is presumptive evidence that it represents a section of public opinion, and the fact of a newspaper representative being sent to make an inquiry is an indication of there being a popular demand which it should be the duty of the institution to supply. If it is desirable that the public should be interested in science, the heads of the large institutions should be expected to go out of their way to supply the daily papers with news, being no less careful to give suitable material to the halfpenny press to be served up in the style which experience has shown is best suited to their readers than to those newspapers which will treat of the subject more or less from the standpoint of the man of science. Such service will have to be unpaid, unfair as this may seem at first sight. If the matter is looked at as a whole, it has to be remembered that the service rendered to science by the newspaper in publishing outweighs that rendered by the individual organisation in contributing. The politician, the Government office, and the various bodies that wish to influence public opinion all supply their information, recognising that the press gives more than it receives; and the great laboratories and institutions should regard it as an obligation to teach the public to think on scientific lines and to take an interest in scientific progress. Even from the money standpoint, however, the bargain will not be such a bad one. Once let it be recognised, as it already is in some newspapers, that scientific news must be treated as seriously as political news, and a fresh opening will be made for those who have had a scientific training.

Lastly as regards the presentation of news. The man of science must trust the instinct of the journalist. Few papers can afford to present scientific news in the way that is acceptable to those who furnish it, for if it is to be read by the public at large it must be presented in a form that the public will appreciate. Even a distorted representation of the truth is of value, because it will stimulate some readers to inquire further; and it must be remembered that as scientific knowledge increases, the demand for accurate presentation will grow. What journalist to-day would dare to write such a description of a cricket match as Charles Dickens wrote in the *Pickwick Papers*? Or, again, what paper dares to publish absurd *canards* as to motoring or aviation?

In conclusion, it has to be remembered that all is not well in science. Politicians are indifferent to its welfare; public authorities are adepts at sweating the Medical Officers of Health and there is nothing said; the Universities are unable to pay respectable salaries even to their professors; manufacturers are only beginning to realise the part that science can play in developing their business; the city is open at any time to the bait offered by the charlatan (I have myself been sent out by a newspaper to investigate the case of a man who was kept for two years in the city on his bare statement that he could synthesise radium); many of the most promising men who would gladly undertake research, finding that it offers them no career, turn their attention instead to money-making; and industries that are ours by right are being driven abroad. These national evils will continue so long as we remain unscientific as a nation. The salvation of the situation lies in the hands of the press; but the press is and will remain powerless to help, so long as the men of science only give it, as at present, their grudging co-operation.

The Noble Prizes for 1913

These have been awarded as follows:

For Physics, to Prof. Kammerlingh-Onnes, Leyden.
For Chemistry, to Prof. Alfred Weiner, Zurich.
For Medicine, to Prof. Charles Richet, Paris.
For Literature, to Rabindra Nath Tagore, India.

NOTICE

THE EMOLUMENTS OF SCIENTIFIC WORKERS

IT is proposed to undertake an inquiry regarding the pay, position, tenure of appointments, and pensions of scientific workers and teachers in this country and the Colonies. The Editor will therefore be much obliged if all workers and teachers who hold such appointments, temporary or permanent, paid or unpaid, will give him the necessary information suggested below. The figures will be published only in a collective form and without reference to the names of correspondents, unless they expressly wish their names to be published. The Editor reserves the right not to publish any facts communicated to him. Workers who are conducting unpaid private investigations must not be included. The required · information should be sent as soon as possible and should be placed under the following headings :

(1) Full name
(2) Date of birth. Whether married. Number of family living
(3) Qualifications, diplomas, and degrees
(4) Titles and honorary degrees
(5) Appointments held in the past
(6) Appointments now held, with actual salary, allowances, fees, and expected rises, if any. Whether work is whole time or not
(7) Body under which appointment is held
(8) Conditions and length of tenure
(9) Pension, if any, with conditions
(10) Insurance against injury, if any, paid by employers
(11) Family pensions, if any
(12) Remarks

SWEATING THE SCIENTIST

IN the four last numbers of SCIENCE PROGRESS a notice has been inserted asking for information on the emoluments of scientific workers; and a considerable number of interesting replies have been received. They are not numerous enough to form a basis for any statistical investigation of the subject—which it is hoped may be attempted later on when more evidence has been collected; but the replies received, combined with information which may be otherwise obtained, suffice to prove the low scale of payment given throughout the British Empire for such work.

The term "scientific worker" includes, according to the notice, all salaried workers—that is, men of all grades, namely, research students, assistants, professors, directors of laboratories, and other fully paid workers, and also half-time and whole-time workers. The duties generally include teaching and the administrative charge of university departments, museums, and special laboratories. The lowest scale of pay mentioned in the replies is £85 a year for half-time work; but it is notorious that a large number of such workers, especially in medical subjects, are paid nothing at all. The pay of junior posts (which are also sometimes unpaid) rises from about £120 to £200, £250, and, rarely, £300 a year. These are of course not so important as the upper scales of pay for full-time professorships and permanent appointments. For the latter, the highest pay mentioned in the replies amounts to £850 a year, with a small pension (Ceylon). The next highest are salaries of £750, both in South Africa, and one of £500 in Canada, with small pensions generally contributed to by the holders of the appointments. It is well known that many professorships in Britain yield £600 a year, with very small contributory pensions. In no cases do there appear to be any arrangements for family pensions in the event of the holders' death—such as are often provided in the public services; nor insurance against illness or accident. Notoriously, very few

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even of the highest posts receive a salary touching or exceeding £1,000 a year; and in nearly all cases the pensions are contributory and are of a very small amount—retirement being often compulsory at the age of 60 or 65 years. Progressive rises of pay are also seldom provided for; so that a man who obtains an appointment when comparatively young can seldom hope for any increase during the rest of his life. Lastly, payment is laid down at many universities according to a flat rate, or according to fixed endowments which depend upon the funds originally allotted—so that no provision is made for retaining specially good men. In some cases holders of fully paid appointments are able to increase their emoluments by outside work. Many medical professorships are quite unpaid.

The rates of pay must be judged by the locality in which they are given. Thus £750 in South Africa is worth very much less than that sum in Britain, the cost of living being perhaps twice as great. A correspondent from Canada remarks that a salary of £800 a year in England is equivalent only to about £600 a year there, and is not sufficient for a professor. "A member of a learned community," he says, "cannot live in a back street like a labourer, and if he takes an unfurnished house in a good locality here the rent will be about a quarter of his income. . . . The smallness of income results, in my case, in my being unable to buy books, subscribe to scientific journals, or join all the learned societies I ought, or to travel to see other universities." Similar complaints are made from elsewhere; and the conditions in Britain are notorious.

Of course, very junior posts are generally financed by scholarships; and are naturally not highly paid because the holders are young men who are, practically, being apprenticed to their labours. The senior posts are those which must be considered in drawing any comparison between the payment for scientific work and other lines of effort; and even in this respect other conditions besides the payment must be taken into account. On the whole, however, such comparison leads to a very unfavourable conclusion regarding the present payment of scientific workers in Britain. It is bad, compared even with the Church. In middle posts, the salaries may be slightly higher; but in academical life the incumbents are obliged to live in towns and are rarely provided with housing. The highest appointments open in science certainly seem to be

paid much less than the highest appointments in the Anglican Church—though the latter figures cannot be very easily ascertained; and, at least, no scientific men have a seat in the House of Lords by virtue of their office or work. The highest salaries for scientific work are very much less than those given in the Army and Navy—which reach to £4,000 or £5,000 a year, and probably more when certain allowances are added. The scientific and academical sides of the medical profession show a similar state of affairs when compared with the clinical side— the incomes of the former seldom if ever exceeding £1,000 a year, while those of the latter are well known to run to many times that amount, especially in surgery. Compared with the law, science stands nowhere at all in Britain, either in payment or in position. The disparity is still greater in comparison with "business"; and the enormous fortunes made in innumerable directions by manufacturers, shipowners, retail and wholesale traders, vendors of registered articles, financiers, and so on, would in many single cases cover the whole funds allotted to science throughout the great British Empire. Even certain branches of art, such as the drama, singing, and acting, have a large advantage compared with scientific work.

It is in no grudging spirit that men of science will draw such comparisons. That good pay should be given for good work is an elementary principle governing all lines of effort; but another principle must be held in view—that, if possible, payment should bear some proportion to the value of the kind of work done. We pay an architect or a general more than we pay the bricklayer or the soldier, because the labours of the former are the more important; and the same principle should carry weight in comparisons of the emoluments of the several professions. In the two previous numbers of SCIENCE PROGRESS, a survey of the value of scientific work to the world has been attempted. It is probably of greater advantage to the world than any other line of effort. Science has become our premier industry, and governs every other industry just as the work of the architect governs that of the individual bricklayers. The world receives not only "fairy tales" from science, but also the most wonderful fairy gifts—a greater knowledge of the universe in which we live, a greater power over nature and over barbarism, greater precision in invention, in the treatment and prevention of disease, and in our manner of judging re-

garding all matters under discussion. Can it be truly said that the labours of any other professions are so valuable to mankind ? Where the priest, the clinician, and the lawyer do good service to the few people surrounding them, and the soldier, sailor, and politician do good service for their country, the discoverer confers benefits upon the whole world, and not for the present generation only, but for all times. We have already argued the case. Mathematics, chemistry, physics, physiology, and pathology have practically built up all those great and wonderful additions which modern civilisation has added to the civilisation of the past, and, with their sisters of the arts, have made a fitting palace for what ought to be a higher race. Yet the payment of the highly qualified men who formed these sciences in the past and who are still perfecting them is less than that given to all the other professions, and, compared with the value of the work, is almost infinitely less. Indeed it would appear that the second principle enunciated above is just the opposite of the truth—that work is paid for in the inverse ratio of its value : and this is not a mere cynical gibe, but the actual truth. The greatest benefits which the world has ever received, that is, those which it has received from science, literature, art, and invention, have generally been paid for not at all.

But it may now be said that the scale of payment for science is purely a question of supply and demand. That is so—and the same principle governs the case of sweated industries of all kinds. In the latter, the employer exploits the necessities of a crowded and poor population in order to have his work done at the cheapest rate. As regards science, however, the employer is the public itself, and the sweated labourer is the highest type of intellect in the country. The process by which the sweating is rendered possible is something as follows : Young graduates, fired with enthusiasm for science or with the desire of investigating some question which has occurred to them, take scholarships or poorly paid research-studentships. At first, while they are young, everything goes well with them ; but after some years they find that the shoe begins to pinch. Then, unfortunately, it is too late. They have lost the time which they should have used in perfecting themselves for their proper profession, whatever that may be—in which they have already been outpaced by men of the same age who were not

so unwise or so high-minded as themselves. The opening which they may have taken five years previously is now closed to them; and they are compelled to spend the rest of their life under the paralysing influences described above. This also is the actual fact; and it must evidently produce a disastrous influence, not only on the men who suffer, but also upon the great studies to which they devote themselves. The most capable graduates are already beginning to perceive the truth and to avoid the toils. The elder men, seeing that investigation leads to nothing, tend to interest themselves only in teaching, compilation of text-books, and attendance upon committees. The enthusiasm and concentration which when found together are called genius become impossible; and we look almost in vain for that high devotion to science which is the only quality she rewards with success. And the punishment does not really fall so heavily upon the worker himself—his enthusiasm for science may quite possibly compensate him for such troubles as those mentioned above. But the punishment falls upon his family; it falls upon the institution which employs him; it falls upon the nation which allows such a thing; and it falls upon science herself.

Besides the low rate of pay given, there are, in this country at least, many small abuses attached to high intellectual work. Even such funds as may be allotted are not used to the best advantage. Large portions of the income of many institutions are given to the maintenance of more or less useless pursuits—which were useful pursuits in the past, but no longer serve the world, or indeed serve it only in a negative sense. Originality and success in research do not receive their due place in selection for appointments. The best paid posts are seldom given for the best work done, but rather for qualities which are of little account—popularity, eloquence, text-book knowledge, private influence, and skill in the arts of time-service. For obvious reasons it is impossible to cite examples, but the fact remains. Of the few Britons of to-day who have done world-service, how many hold the leading public posts even in their own domain? We appear to judge men, not by the work which they have done, but by the work which we may imagine, from their appearance, that they may do if we give them an opportunity. How many of our most distinguished writers, for example, have received academic posts for teaching their own

art ? And how many of our most distinguished men of science are now heads of British universities ?

Many other disabilities are frequently complained of and resented by scientific workers. The whole system of filling appointments requires careful reconsideration. Some years ago an excellent article on the subject of advertising vacant appointments appeared in the *University Review*. The advertisements are often issued when the post has already been practically allotted—simply as a kind of show to prove impartiality on the part of the advertising body. The result is that numbers of candidates are tempted to put themselves to great trouble and some expense, and are kept upon the tenterhooks of doubt for months. Another abuse, still allowed for academical and hospital posts, is the necessity of canvassing for appointments— a very objectionable system which compels the unfortunate applicant to visit a number of persons with whom he is not acquainted and who often have no knowledge of his subject, and to parade his virtues before them in competition with other unfortunates who are in the same case. We heard some time ago of a distinguished mathematician who was obliged to sue humbly for a poorly-paid post before two local tradespeople— and who was not accepted. Can anything show more clearly than such a state of affairs the low position held by high work in Britain ? Indeed the whole system so frequently adopted here of allowing scientific institutions, hospitals, and even universities, to be governed by committees of persons of whom many have no qualifications for the work, who are often not even moderately distinguished in any line, but who find their profit in the position, is thoroughly discreditable; and recent disputes in the management of certain hospitals have illustrated the defect.

We have recently started the habit of giving our rare professorships to foreigners—not really because the foreigners are the best men for the posts, but because the institution concerned likes to obtain a reputation for magnanimity. Yet foreign nations are not so generous to us. As a matter of fact we buy, not in the cheapest market, but in the dearest one; and do so, not from motives of business, but merely out of ostentation. The same indifference to work done is often manifested in the honours given by many learned bodies. We see the academic laurel placed upon the brows of soldiers, sailors, and

politicians—men who have perhaps done great service in their own line, though not in the line for which such honours should be reserved. The case can of course be argued—as all bad cases can; but it is really a matter of clean taste. Academic honours are meant to promote great world-service; and it is a sign of national degeneracy when they are given for anything lower. One would think that our universities would lead the way in this respect, but it is not so. Some years ago a distinguished Colonial Premier refused an academic honour on these grounds, and attained greater honour by doing so. Few are the struggling workers or the struggling causes which have benefited by the powers in the hands of the great learned bodies. To add grist to their own mill by subserviency to popular idols appears too often to be their chief desire; and where a great worker is honoured by them, he is generally a foreigner. A still lower stage, however, has already been reached—where a learned body decorates itself!

We may now ask, what exactly does the British Empire do, as a State, for science, or indeed for any of the higher forms of intellectual effort? Parliament allots £4,000 a year to one learned society, and another £1,000 a year for publications—a magnificent endowment! It allows also occasional small grants to other institutions; and all these are doled out for the expenses of special researches. The larger grants which it gives to universities are devoted chiefly to teaching—a very small proportion ever being really available for investigation. Very little of the money goes to the workers themselves, either to increase their pay or to reward them for services rendered; and the State seems to think that if it provides their test tubes and microscopes it has done enough. In many countries the government wisely pays members of certain academies; but in Britain, not only is this not done, but the State actually exacts gratuitous services from such members. For example, a Government department wishes for expert advice on some matter—it ought to form a commission of its own and honestly pay the expert members of it. Instead of doing this the Government department goes to some learned society and asks it to advise on the scientific question at issue. The society is honoured by the request, and obtains the advice gratis from its own members. Thus the Government gets what it requires for nothing; the learned body is overpowered with the honour rendered to it;

and the unfortunate worker is the loser. Such action is very common ; unpaid Government committees are now becoming the rule, and even reimbursement of travelling expenses is often boggled at. We heard the other day of a man who was actually found fault with for not attending a committee of this nature for which he was not paid. In other words, the State exploits the man of science on account of his enthusiasm for his work and his patriotism. The thing might be excused if the State were to give large funds for scientific work, but as it does not do so such action is extraordinary in its meanness and impropriety.

Many similar points may be cited. The Board of Education expends annually an enormous sum, amounting to nearly twenty millions a year, on low-class education ; but what does it do for the greatest of educators—science, literature, art, drama, exploration, discovery, invention ? As was pointed out in the last issue of SCIENCE PROGRESS, the Patent Acts do not cover those whom they should most carefully protect, namely the men upon whose investigations nearly all inventions are founded. Quite recently the House of Commons has given itself payment amounting to over a quarter of a million pounds a year. Perhaps this is quite right; but may we not ask whether a small fraction of the money, properly devoted to scientific investigation in many lines, would not be of much greater benefit to the people than are the wranglings of party politicians over questions which will never be honestly decided because they are never honestly considered ? Still more recently the State has given, very wisely, £57,000 a year out of the Insurance Fund for medical researches. It was suggested at the committee which organised the management of this expenditure that a large prize should be available out of the fund for important discoveries ; but the money actually offered has now been reduced to a maximum of £1,000. In other words, if a private medical man were to discover the means of prevention or cure of tuberculosis or cancer—which he would not be likely to do without spending years of study over the theme, and probably losing his practice in consequence of his work—his only reward would be £1,000 ! The discoverer will not be paid ; and yet the country hopes to have discoveries achieved ! And this brings us to what is really the crowning defect of the national attitude towards high effort of such kinds, namely that it makes no attempt whatever to pay for any benefits, however great, which it receives from individuals. A successful

soldier may indeed receive a handsome donation, and many politicians obtain large pensions; but the highest services in the domains of science, literature, and art are not deserving of reward!

The net result may of course be foretold from these data. There is much petty science, petty literature, and petty art; but the more arduous labours which require the devotion of a lifetime are becoming increasingly difficult. The man of science is now exactly in the position in which writers and inventors found themselves before the Copyright and Patent Acts were passed. He is never the master in his own house; he is the slave to institutions which "run him" for what he is worth; and is seldom able to spend his time in the exercise of the lofty gift which nature has given him. Still worse, the most capable minds are at the outset turned away from fields in which their efforts are likely to be of the highest value to humanity.

All this really springs from the curious and stupid attitude of the public towards all forms of intellectual effort. It seems to take no interest in such effort. Politics, game-playing, and picture-shows are the things which amuse it. The great worker is a mere bookworm, or a plodder, or a crank. But the truth is that, just as individuals have duties to perform to their country, so have countries duties to perform to the civilised world. It is the duty of every nation to participate in the discovery of the laws of nature, to ascertain the cause of disease, to enhance the powers of man, and to widen the range of his vision. What does Britain do to fulfil this duty? She still has great workers, it is true; but their work springs from themselves, and not from the nation. The country does not perform the duty referred to. It has become like a trades-man who has reached great wealth by the exercise of inferior arts, but who spends it on amusements, pleasures, and the ostentation of charity, without sparing a penny for higher objects. This figure may at least be reached as a rough integration of the general complex formulæ of our present condition. Behind all there is a shadow: for nations, like individuals, must remain efficient.

PHYSICS IN 1913

WITH SPECIAL REFERENCE TO THE DIFFRACTION OF X-RAYS BY CRYSTALS

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PERHAPS the most important, certainly the most striking, advance in physics during the period of the last year or so is the demonstration of the regular diffraction of X-rays, the experimental confirmation, that is, of a theory often tentatively put forward, that X-rays are a disturbance of the same nature as light, differing only, as far as we can judge, in their wave-length from visible light. While evidence for this was being accumulated, however, the nature of light radiation itself has become more and more mysterious, and the recent investigations as to the laws and nature of radiant energy have rather served to demonstrate the defects of present theories than to provide us with any very convincing and comprehensive ideas on the subject. Planck's theory of quanta, which assumes that radiant energy is emitted not continuously but in discrete units whose magnitude depends only on the frequency of the radiation in question, still provides the reigning hypothesis, although the conception of the discontinuous absorption of energy in such units has been abandoned on account of the insuperable difficulties it presents.

Many atom models have been put forward, most of which have a very limited application, and would seem designed with a view of imitating one set of phenomena only. Apart from these ephemeral fancies is Rutherford's nucleus atom, consisting of a small positive nucleus, surrounded by rings of electrons at distances from it very large compared to the dimensions of the nucleus itself. This has shown itself very successful so far, and a mathematical treatment by Bohr, based on an application of the quantum theory to the radiation from an atom of this type has attracted considerable attention, and will be described in more detail in the following account. Two sensational announce-

ments which await confirmation are Aston's separation of a new gas of atomic weight 22, very closely allied to neon, from this gas, and Stark's splitting up of the hydrogen lines by the application of an intense electric field, corresponding to the Zeeman effect in a magnetic field. The effect of small traces of gas on some of the electric properties of metals has been brought into prominence, and the very existence of the photoelectric effect for absolutely gas-free metals has been questioned. Several other researches of importance are described in the following pages, and we may say that the discoveries of the past year have opened up more than one field of research which promises rich results.

The first experiments on the diffraction of X-rays by a crystal were described in June 1912 by Friedrich, Knipping, and Laue in a paper entitled "Interference Phenomena with the Roentgen Rays." In these experiments a fine pencil of rays from an ordinary X-ray bulb was passed through a slip of crystal and received on a photographic plate placed behind the crystal at right angles to the incident beam. The plate on development showed not only a very dark spot, corresponding to the direct beam, but also a series of other fainter spots surrounding it in a complicated geometrical pattern of high symmetry. These spots are formed by beams of X-rays scattered in definite directions from the crystal, and making, in some cases, angles as large as 45° with the direct beam. Laue's theory, which led to the experiments, was that the crystal, because of the regularity of its structure, formed a natural diffraction grating, which differed from the ordinary ruled grating firstly in having a very much smaller grating space, and secondly in being an arrangement in three dimensions, analogous to a set of plane gratings placed one behind the other. Each molecule he supposed to be capable of emitting secondary wavelets when struck by the incident ray, and by assuming the molecules to be arranged in a simple system, each one being at the corner of an elementary cube, he accounted for the positions of the spots on the plate: to explain the absence of other spots which might be expected he had to assume that the incident pencil of X-rays did not contain a continuous range of wave-lengths, but a certain five wave-lengths which he calculated. The order of magnitude found for them was from 1 to 4 times 10^{-9} cms. This is about the same order as the length previously suggested by Walter

and Pohl from attempts to obtain diffraction with a wedge-shaped slit, and a similar estimate had been formed from the ratio of the intensity of the X-rays to that of the cathode rays excited by them.

Soon after Laue's discovery, W. L. Bragg, the son of W. H. Bragg, suggested a different way of regarding the phenomenon. He considered the crystal as containing different series of parallel planes in which the atoms are closely packed; from these reflection of the rays takes place, for by Huygen's principle a number of points arranged regularly on a plane will give rise to secondary wavelets which build up a wave reflected at the angle of incidence. Now in a crystal supposed built up of the so-called face-centred cubes [1] a system of series of parallel planes rich in atoms can be picked out, from which such reflection, obeying the ordinary laws of optical reflection, takes place. W. L. Bragg obtained a simple geometrical construction for the points that would result from reflection from such planes, and the diagrams he obtained agreed excellently with photographs taken by Laue's method. There is no need to assume the incident radiation homogeneous, or consisting of a few definite wave-lengths; the crystal structure will account for the sorting out of the general, or "white," radiation,[2] or, in other words, will impress the regularity on it.

Bragg confirmed his theory of reflection by throwing a beam of X-rays on a cleavage face of mica, cleavage faces of crystals being rich in atoms; he obtained reflection according to optical laws. W. H. and W. L. Bragg then examined in more detail the reflection of the rays, making use, not of the photographic plate, but of the ionisation produced by the reflected rays in order to detect them. The apparatus resembled a spectroscope in form, in which the collimator was replaced by a lead slit through which the incident rays passed, the telescope by an ionisation chamber to which the rays obtained access through a second narrow slit. They found that reflection took place always in accordance with the law of equal angles of incidence and reflection, but that with different angles of incidence the intensity of the reflected ray, measured by the ionisation produced, varied markedly, showing a series of pronounced

[1] A cube with a point at each corner and one in the centre of each face.
[2] By analogy from white light, which can be resolved into a continuous group of wave-lengths.

maxima at definite angles. These maxima correspond to homo-geneous rays of wave-length λ, given by nλ = 2d sin θ, where θ is the angle of incidence, n an integer, and d the distance between successive planes, for only at such an angle will the waves reflected from the successive planes reinforce one another. Some of the radiation from an X-ray bulb is "white" radiation, and accordingly contains components reflected at any angle; in addition there are strong homogeneous beams reflected only at certain angles, the angle depending on the wave-length of the particular radiation in the way described. The homogeneous radiations are the "characteristics" of the metal of the anti-cathode, investigated by Barkla by measurements of their absorp-tions; the target, or anticathode used by the Braggs in their earlier experiments was of platinum, and they determined the wave-length of the characteristics of platinum from the formula already given, d being worked out from the weight of the atom, the assumed structure of the crystal, and its density.

Moseley and C. G. Darwin about the same time examined the radiation from a tube with a platinum target by reflecting it from the principal cleavage-planes of different crystals, rock-salt, selenite, and potassium ferrocyanide being used. They detected the reflected beam by means of the ionisation produced, and showed that the primary and reflected beams contained the same constituents, present, however, in different proportions in the two beams, in other words that the crystal did not manufacture a special type of radiation, but picked out radia-tions already present. They detected five homogeneous radiations, reflected at definite angles from each of the crystals, and measured each radiation in three different orders, that is, they found for each radiation successive values of θ, the angle of reflection, corresponding to the values 1, 2, 3 for n in the formula nλ = 2d sin θ. The reflection theory was strongly confirmed both by comparing the results with the different crystals, which showed d a constant for the given face in each crystal, but differing from crystal to crystal, and by the obtaining of each line in different orders. Thus a homogeneous X-radiation is reflected from a crystal plane rich in atoms at certain definite angles, whose sines are simple multiples of one another; this corresponds to the different orders in the grating spectra of visible light. To the grating space corresponds not the distance between the atoms in a

single plane, but the distance between two adjacent planes of the series of parallel planes in question. Of course, part of a heterogeneous radiation will be reflected at any angle. In Bragg's explanation of the Laue patterns the position of the spots depends on the existence of several series of parallel planes rich in atoms; the incident radiation is supposed heterogeneous, and from this the planes pick out the wave-length required for reflection to take place for the fixed angle of incidence.

More recently Moseley has examined, using the method of the X-ray spectrometer described, the characteristic radiation from all the metals whose atomic weights lie between 40 and 65, by employing them successively as the targets in an X-ray tube; the characteristics are excited by the fast cathode rays. The metals were mounted on a little truck, so that they could be brought at will in the path of the cathode rays; the X-rays produced were reflected from a crystal of potassium ferrocyanide and detected photographically. The X-ray, or high frequency, spectrum of each element he found to consist of two lines, one stronger than the other, which he calls the a (strong), and the β line; Bragg also found two lines for the rhodium spectrum. The wave-length of each line was found in terms of the spacing of the planes in the rocksalt crystal, known from Bragg's researches, and it was found that the quantity $Q = \sqrt{\dfrac{\nu}{\frac{3}{4}\nu_0}}$ (where ν is the frequency of the a radiation, ν_0 a constant, the fundamental frequency of ordinary line spectra) was a whole number, increasing by one for each successive element taken in the order of their atomic weights. If N be the atomic number, the number, that is, of the place occupied by the element in the periodic system (H = I, He = 2, Li = 3, . . . , Ca = 20, etc.), Moseley found that $Q = N - I$. or ν, the frequency, varies as $(N - I)^2$. This suggests that the atomic number is perhaps more important for physical processes than the atomic weight; the point will be referred to again later.

Rutherford and Andrade are investigating the γ rays from radium by the method of reflection from crystals, and have in a preliminary note announced that they have photographed groups of lines given by the γ rays from radium B and radium C; hence the γ radiation from these substances is not

of one wave-length, but complex. De Broglie has taken excellent photographs by the reflection method by arranging the crystal to rotate very slowly by means of clockwork, and letting a narrow beam of X-rays strike the crystal face where the axis of rotation passes through it. As the correct angle for any homogeneous ray present is reached, the ray is reflected and recorded on a fixed photographic plate.

By the experiments described much light has been thrown upon the nature of X-radiation; the Braggs have applied these results to study the structure of crystals. For this purpose X-ray photographs are taken with the crystal to be examined, either by the Laue method of transmission, or the reflection method; for the former heterogeneous radiation is required, for the latter a homogeneous beam, such as that found to be emitted from a rhodium anticathode. The reflection method gives the more direct information; photographs are taken by reflection from the planes richest in atoms in the crystal, the so-called (100), (110), (111) planes, and the given line, corresponding to the homogeneous radiation, is sought at the series of angles corresponding to the first, second, and subsequent orders. In general the line cannot be found in all the orders; for instance, for 'the (111) planes in diamond there is no second order spectrum, although first, third, and fourth are found. There is no space here to enter into the details of the deductions which can be drawn from such evidence; it will suffice to state that for a crystal of an element, such as the diamond, the absence of certain orders indicates that the series of parallel planes from which the reflection under consideration takes place are not equally spaced from one another, but a series of equally spaced planes are separated by another series of equally spaced planes arranged so as to divide the spaces between the first set unequally. By considerations of this kind, the Braggs have obtained a detailed model of the spacing of the carbon atoms in the diamond, which they checked by photographs of the Laue type; the model shows the atoms arranged at the points of two interpenetrating space lattices; between a series of planes equally spaced other planes are placed so as to divide the distance between them in the ratio of one to three.

Further very interesting information has been obtained as to the arrangements of the atoms of different kinds in crystals of

chemical compounds, such as the simple halogen salts of potassium. Assuming that the atoms of a crystal are arranged as points in a space lattice, W. L. Bragg has shown that a structure the same in all cases can explain the transmission patterns obtained with this series of salts, the differences in the patterns being due to the fact that the efficiency of an atom as a diffracting centre increases rapidly with its atomic weight. If the atoms are of nearly equal atomic weights, as in K Cl, they are nearly equivalent as centres; if one is at least twice as heavy as the other, as in K Br or K I, the lattice formed by the heavy atoms alone gives the pattern. The experiments also point to the single atoms acting as diffracting centres, the lighter atoms not being associated in any special way with, or grouped closely round, the heavier atoms, but occupying intermediate positions between the neighbouring heavy atoms, so that they can equally well be considered as belonging to different ones. For instance, an atom of sodium is equally close to six chlorine atoms in a crystal of rocksalt. Thus in such a crystal a molecule cannot be considered as having any individual existence; rather the whole crystal constitutes one huge molecule. There can be no doubt that the method is likely to prove very valuable in examining further the structure of crystals.

It would be expected that the heat motions of the atoms would influence the diffraction pattern formed by the X-rays. That the heat vibrations do not disturb the patterns at room temperature might be immediately explained by Lindemann's conclusion that at such temperature the distance of the centres of the molecules—or atoms—only varies a few per cent. owing to heat agitation. In an extended mathematical paper Debye has come to the conclusion that the heat motions will not affect the positions of the spots of the patterns, or their sharpness, but only their intensity, increasing agitation causing the spots to become fainter and fainter. The independence of the positions and sharpness of the temperature have been experimentally confirmed by de Broglie, the weakening of the spots at high temperature by Laue and van der Lingen, so that Debye's theory has been, at least qualitatively, confirmed.

A good deal of rather more random work has been already done on the diffraction of the X-rays by substances other than crystals. Keene has shown that ordinary rolled metal sheets give X-ray patterns owing to the metal possessing a crystalline

structure, and Nishikawa and Ono have shown that many fibrous substances, such as asbestos and bamboo, give patterns of a rather different type.

Not very much progress has been made in the general theory of radiation during the past year. Since Poincaré in 1912 showed that no law of continuous emission of radiant energy could account for the form of the radiation curve in the short wave-lengths, all attention has been concentrated on the application of Planck's quantum theory, which asserts that radiant energy cannot be emitted continuously in amounts of a completely arbitrary magnitude, but only in whole numbers of discrete units, or quanta, of energy, whose magnitude is a constant, h, times the frequency number of the given radiation. The universal constant h is often referred to as Planck's constant. In its present form the theory does not exclude continuous waves of energy in the ether, or the continuous absorption of energy, as without waves in the ether of the nature assumed in the classical electromagnetic theory it seems impossible at present to account for the phenomena of polarisation and interference. The polarisation of light by crystals would seem to depend on an interaction between the matter and radiant energy which does not take place quantum fashion. The quantum theory presents grave difficulties in the way of a satisfactory physical interpretation, especially over the question of absorption, but its justification lies in the brilliant agreement which many of its consequences show with experimental results at present inexplicable on the older theories. The older Hamiltonian equipartition of energy among the degrees of freedom has proved insufficient; Jeans has abandoned his idea that its predictions fail because the steady state is never realised. The simplest of the present methods of deducing the radiation formula is to count the number of degrees of freedom of the equilibrium radiation by means of the number of different stationary waves set up in an enclosure with reflecting walls (Jeans, Rayleigh), and distribute the energy among them in quanta of magnitude proportional to the frequencies according to a probability law. Debye, by an extension of this method, has obtained a formula connecting the specific heat of metals with the temperature, agreeing remarkably well with experiment. He assumes that the heat energy consists of elastic vibrations of the atoms about positions

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of equilibrium, and calculates the number of such possible vibrations from the elastic constants of the body. The quanta being distributed among these degrees of freedom as above, the formula for the heat energy can be calculated.

On Planck's second theory, since the absorption of radiant energy can take place continuously, and an atom cannot emit less than an amount $h\nu$ of energy, there must always remain in the atom an amount of energy varying from O to $h\nu$, or a mean amount of energy $\frac{h\nu}{2}$. This energy is the latent energy of the atom, which Wien ascribes to the energy of electrons in the atom. He distinguishes between the electronic energy and the energy of the atom ; this mean amount $\frac{h\nu}{2}$ does not belong to the atomic energy considered by Debye in his treatment of the specific heat problem, and so does not interfere with the deduction of his formula.

The theory of the electrical and thermal conductivities presents still many problems which await solution. Drude's old theory seems to be almost universally given up, since it stands in contradiction to the radiation results, as demonstrated by Lorentz, and also to the experiments on specific heats at low temperatures. Lenard has worked out a theory based on the assumption that the electrons in the metal are not gas-kinetically reflected from the atoms, which seems impossible in the face of recent experiment, but are absorbed by the atoms and subsequently liberated, the liberation depending on the proximity of the atoms (Nähewirkung). This gives the velocity of the electron independent of the temperature, which is the assumption favoured by Wien, who connects it with the latent energy of the atoms mentioned above, which is independent of the temperature. Many of the observed results are given by Lenard's treatment ; it involves, however, in its present form too many indeterminate factors to be very useful, and it is doubtful if it will give the abnormally high conductivity of metals at temperatures near the absolute zero found by Kammerlingh Onnes. Wien, using Debye's assumptions made for the specific heat, has obtained a formula which gives a good agreement with experiment in this direction, even for the very low temperatures. But in both the electric and thermal conductivities there are many points still unsatisfactorily explained, and,

in general, the physical properties of the elements at low temperatures are inadequately accounted for by present theories.

The quantum theory has recently received a rather more direct confirmation than is afforded by the work on the radiation formula or the specific heats. Bjerrum put forward a new conception of the mechanism of the absorption of radiations in the infra red region. The charged atoms in the molecule, which are the resonators accounting for the absorption, are supposed not only to execute linear vibrations of frequency ν_1, but also to rotate with a frequency of rotation ν_2. If they vibrate in a direction normal to the axis of rotation there are resultant vibrations of frequency $\nu_1 + \nu_2$, $\nu_1 - \nu_2$; if along the axis of rotation, the movements are independent. There will thus result the four frequencies ν_1, ν_2, $\nu_1 + \nu_2$, $\nu_1 - \nu_2$. This will give three periods in the short infra red and one in the long infra red. Supposing the rotation frequencies continuously distributed according to the Maxwell probability law, this would give three near absorption bands in the shorter infra red for a gas, and Burmeister found experimentally that the absorption in this part of the spectrum always occurred in broad double bands, the midway absorption line predicted by the theory being too narrow to detect. Now on the quantum theory the rotation frequencies are not continuously distributed, but increase in jumps ; according to this the double absorption bands should not be smooth, but show a serrated edge, the series of maxima corresponding to a series of separate absorption frequencies. E. von Bahr has, by increasing the resolving power of the infra red spectroscope used, actually found a series of jagged irregularities in the absorption bands, of the kind predicted by the theory indicated. This furnishes striking evidence for the physical existence of quanta of energy, at any rate in some cases. Eucken has further pointed out that the measurements of the infra red absorption spectrum of water vapour present exactly the irregularities required by Bjerrum's theory if the rotational energy is distributed in quanta. The band in the longer infra red has likewise been experimentally found.

In the matter of the specific heats at low temperature Dewar's latest results are of considerable interest. He has measured the mean atomic heat over a range of 60° C. for fifty-five elements at 50° absolute (the temperature fall being

from 80 to 20° absolute), and plotted them against atomic weight. The atomic heats, ranging from 6·82 for Cæsium to 0·03 for diamond, then reveal a definite periodic variation resembling the Lothar Meyer curve for atomic volumes in the solid state. If experiments were made between the boiling point of liquid hydrogen and that of liquid helium, the atomic specific heats would probably be all nearly equal and very small.

Turning to theories on the structure of the atom, Bohr's work claims attention; it is a mathematical treatment of Rutherford's nucleus atom, which has been very successful. Bohr's atom gives a theoretical account of the line spectra of the elements, especially those of the relatively simple hydrogen and helium atoms. It is interesting as referring the discontinuities of wave-length in the line spectrum of a gas back to the discontinuities of energy emission postulated by the theory of quanta. As a result of experiments on the scattering of a particles by matter Rutherford in 1911 put forward the theory that the atom consists of a central positive nucleus, of dimensions very small compared to the atomic radius, in which practically all the mass of the atom is concentrated, this nucleus being surrounded by a distribution of electrons making the atom neutral as a whole. The number of electrons was deduced to be about half the atomic weight; it is now considered likely that it is the "atomic number" already mentioned. To get the spectral lines which would be emitted by such an atom Bohr makes the assumption that the electrons are rotating round the nucleus in circular orbits; there is no energy emitted when the electrons are rotating steadily in a stationary state. An electron can, however, pass from one such stationary state to another, changing the radius of its orbit; during this transition a homogeneous radiation is emitted, whose frequency ν is determined from the change of energy between the two stationary states by the equation $E = h\nu$, where h is Planck's constant, and E is the energy change. The amount of energy emitted is thus always a whole quantum, and a further assumption as to the connection between the total energy of formation of the system and the frequency of rotation of the electron in the system formed leads to the conclusion that the angular momentum of any electron round the nucleus is an entire multiple of the quantity $\frac{h}{2\pi}$; in

the most stable system consequent on the emission of the maximum amount of energy the angular momentum is $\frac{h}{2\pi}$. This is probably closely connected with Weiss' magneton, or elementary unit of magnetism. Balmer's and Rydberg's laws follow from these assumptions, and, considering the case of hydrogen as represented by a central nucleus with one rotating electron, that of helium as a nucleus with two electrons, Bohr has calculated a value for Rydberg's constant in close agreement with the empirical value. His theory gives all the hydrogen spectra observed; in connection with the spectral lines calculated by him for helium the question has arisen whether certain lines observed from hydrogen contaminated with helium, and previously attributed to hydrogen, are not due to helium. Fowler attributed the lines to hydrogen; but the recent work of Evans points to these lines being part of the helium spectrum as required by Bohr's theory. It may be noted that Bohr finds that the charge on the nucleus, or the number of electrons present in the neutral atom, is equal to the atomic number of the element; this has also been suggested by van der Broek, and agrees with Moseley's X-ray spectrum results, as already mentioned.

Bohr's atom is satisfactory as agreeing with Rutherford's postulates required by the scattering experiments, and giving a good account of the spectral series in a way much more in accord with modern ideas on radiation than Ritz's atom; the numerical agreement is especially good. It remains to be seen if the atom will give the Zeeman effect, and a new effect discovered by Stark—the resolution of a spectral line into components in an electric field—which will be described later. Warburg has very recently worked out that such an atom model will not give the required resolution into lines, but merely a broadening of the spectral line; however, these results obviously depend upon the nature of the special assumptions made, as further assumptions are necessary, and we await further work from Bohr himself upon this point.

The new effect discovered by Stark, just mentioned, is the resolution of the chief hydrogen and helium lined into components by means of a strong electric field, corresponding to the Zeeman effect in the magnetic field. The great difficulty in such experiments is to obtain a strong electric field in a

luminous gas; owing to the strong ionisation taking place an
arc tends to set in. The principle employed by Stark was
to use canal rays, and apply the potential difference between
the pierced cathode and a subsidiary electrode behind it, that
is, on the side remote from the anode. If the cathode dark
space is adjusted to be much greater than the distance between
these electrodes (about 2 mm.), a potential difference corre-
sponding to a field up to 31,000 volts/cm. can be applied without
arcing. The canal rays were observed in a direction normal to
their path (and so normal to the electric field), to avoid the
Doppler effect. The hydrogen lines H_β and H_γ were resolved
for this transversal effect into five components, the three middle
ones polarised normal to, the two outer ones parallel to, the
electric field. The extreme separation obtained with a field of
estimated strength of 30,000 volts/cm. was nearly as great as
that of the two sodium D lines (under normal circumstances).
The helium lines were also resolved, but into different com-
ponents, the effect varying with the series to which the line
belonged. The separation of the components seems to be
proportional to the field. The effect is obviously of the first
importance, and, as the author points out, may have disturbed
the finer observations of the Zeeman effect, as the application
of the magnetic field, by diminishing the cross section of the
positive column, increases the longitudinal electric field, perhaps
sufficiently to cause the Stark effect to appear.

    J. J. Thomson has continued his work on the positive rays,
examining them by his well-known method of photographing
the trace of the rays simultaneously deflected in an electric
and a magnetic field on a plate placed at right angles to the
undeflected beam. The form of the curve obtained on the plate
gives the ratio $\frac{m}{e}$ for the rays causing any particular curve, and,
further, information concerning the velocities of the particles
constituting the ray in question. The most recent results are
those which concern the lines which correspond to $\frac{m}{e} = 22$,
and $\frac{m}{e} = 3.$[1] The former line was obtained when the residual
gas in the tube consisted of the lighter constituents of the
atmosphere; a molecule of CO_2 with a double charge would

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[1] $\frac{m}{e}$ is taken as unity for the singly charged atom of hydrogen.

give a line coinciding with this line, but the CO_2 can be removed without influencing the line. J. J. Thomson came to the conclusion that it corresponded to a new gas of atomic weight 22, closely allied to neon (atomic weight 20), with a single charge. After many attempts Aston, working in the Cavendish laboratory, has apparently succeeded in isolating such a gas by allowing the mixed gases (neon and the supposed new gas) to diffuse repeatedly through a porous wall; owing to the difference in rates of diffusion consequent on the heavier atom of the new gas, a separation would be expected. In this way Aston obtained two gaseous components which showed the 22 line in different intensities, and also gave differences of density when weighed on a specially constructed quartz balance. The two gases, however, gave the same spectrum, and in other respects showed like chemical properties, so that on the present evidence it appears they differ in atomic weight, but not in chemical properties; this is the case with some of the radio-active elements. On Rutherford's nucleus atom theory such a state of things is quite possible, since the chemical and physical properties of an atom depend on the charge on the nucleus, while the atomic weight depends on the inner structure of the nucleus, and may not be proportional to the charge.

As regards the unknown substance " X_3 " causing the line for which $\frac{m}{e} = 3$, J. J. Thomson has come to the conclusion that it is triatomic hydrogen with one charge. He has shown that it cannot be a carbon atom with four charges, and the fact that it can be obtained by the bombardment by cathode rays from salts containing hydrogen, but not from those which contain no hydrogen, the salts in both cases being previously freed from absorbed gases, lends support to the hydrogen hypothesis. J. J. Thomson has also been examining by the positive ray method the gases given off from a great variety of substances when they are exposed to the bombardment by cathode rays, with special reference to the production of helium, found by Ramsay in old X-ray bulbs, and neon. He finds that in all cases small amounts of helium are liberated, even when the bombarded salts have been dissolved and dried several times to free them from occluded gases. The experiments are still in progress, and the source of the helium must be regarded as still in question. Very recently Strutt has published an account of attempts made

to observe the production of neon or helium by electric dis-
charge : his results were negative.

In connection with J. J. Thomson's work on the positive rays,
which seem to offer a new and very sensitive method of chemical
analysis, it may be mentioned that Moseley found that the X-ray
spectra already described gave good evidence of traces of foreign
metals in some of the metals used, the strong line in the spectrum
of the impurity appearing distinctly along with the spectrum of
the metal itself.   The X-ray spectrum, being apparently so much
simpler in character than the ordinary spectra, may in this way
afford a very powerful method of chemical analysis.

Two very interesting papers have appeared which trace effects
previously assumed to be of purely electronic origin to the pre-
sence of small quantities of gas.   The effects in question are the
photoelectric effect and the thermionic effect, which have been
the object of so much study in recent years, and concerning
which so many inconsistent results have been obtained.   From
a careful study of the behaviour of purified carbon Pring has
come to the conclusion that the emission of electrons by this
substance ordinarily observed when it is raised to a high tem-
perature is due largely, if not entirely, to the presence of traces
of gas.   He has shown that by very carefully purifying the
carbon and reducing the pressure of the surrounding gas as
much as possible the discharge of negative electricity can be
diminished to an enormous extent, that on the admission of a
little gas the thermionic current gradually increases correspond-
ing to the occlusion of the gas, and that the effect depends in a
high degree on the nature of the traces of residual gas, being
very small indeed with the inactive gases helium and argon, and
relatively considerable with carbon dioxide, which is known to
react with carbon at high temperatures.   These and other results
furnish a strong presumption that the so-called thermionic effect
is due to the presence of traces of gas, which react, probably in
a cyclic process, with the carbon, the reaction resulting in the
liberation of electrons; the effect is thus probably a chemical
one.   Freydenhagen came to similar conclusions in 1912 with
respect to sodium and potassium in a high vacuum, after Pring
had already published preliminary results on carbon.   It remains
to be seen if there is a residual true thermionic effect, which
must be in any case very small, and if the effect in different
metals is due to traces of gas.   Considered in conjunction with

the work described in the next paragraph it seems likely that the whole effect is due to traces of gas.

At the suggestion of Freydenhagen a student of his, Küstner, has studied the photoelectric effect in zinc, one of the metals hitherto supposed to be particularly active in this respect, which had been carefully purified and scraped by means of a magnetically actuated blade while actually in a vacuum. Special means were resorted to in order to obtain a very high vacuum, and to rid the zinc of the last traces of the gas; Freydenhagen had come to the conclusion that under these circumstances the photoelectric effect would cease. It was found experimentally that by prolonged scraping in vacuo and exhaustion the effect could be reduced until it was not detectable; further, that the various types of photoelectric fatigue and abnormal initial effects were easily explained on the assumption that they were due to the occlusion of residual gases, and could be imitated at will. The results are striking enough, and present remarkable similarities throughout with those of Pring on the different effect. The work is being extended in both directions, and, while it is as yet too early to dogmatise, it certainly seems probable that the emission of electrons due to the action of both heat and illumination by ultraviolet light is bound up with and dependent on the presence of occluded gases. The many irregularities observed by workers in these fields confirm this belief.

The foregoing does not pretend to be a complete record of all work of any importance done during the past year, but rather an account of certain pieces of work performed, and theories put forward, during that period, selected because they seem likely to prove of far-reaching importance and to modify and extend our existing ideas. Thus there can be no doubt that the methods of investigating the X-rays opened up by Laue and the Braggs, father and son, have already led to results of fundamental importance, and are likely to lead to many more. Much careful work done in elaboration of older lines of research has been passed over without mention, not because the author is possessed of that passion for the new which to-day seems in so many cases to express itself in a desire rather to tear down than to build up, but because considerations of space have prohibited an adequate treatment of more than a relatively few selected themes. It is to be hoped that the work here described will all of it prove of permanent value to the progress of physics.

References to Literature

A list of the chief papers describing the work mentioned in the foregoing article. Further references will in many cases be found in the papers here cited.

Diffraction of X-rays by Crystals, and Allied Work

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VERTEBRATE PALÆONTOLOGY IN 1913

By R. LYDEKKER, F.R.S.

THE first point to notice is that the complete paper, by Messrs. Dawson and Smith Woodward, on the famous Piltdown skull appeared in vol. lxix., pp. 117–51, of the *Quarterly Journal of the Geological Society*, where the full name, *Eoanthropus dawsoni*, was for the first time published, thus dating from 1913. As so much space was devoted to this subject in my review of vertebrate palæontology in 1912, published in last year's volume of this journal, it might well have been thought that there was little or nothing to add on the present occasion. Additional material— in the shape of a lower canine tooth—has, however, been brought to light since the publication of the original memoir; and a regrettable controversy has taken place with regard to Dr. Smith Woodward's restoration of the skull, and the nature and affinities of the being to whom it pertained. Into the details of this controversy I have no intention of entering; and I shall content myself with quoting certain extracts from the report of an evening discourse delivered by Dr. Woodward before the Royal Institution on September 16, 1913, in which the criticisms of his work are discussed and, for the most part, refuted.

As regards the discovery of the aforesaid tooth Dr. Woodward spoke as follows:

"Fortunately, Mr. Dawson has continued his diggings during the past summer, and on August 30 Father P. Teilhard, who was working with him, picked up the canine tooth which obviously belongs to the half of the mandible originally discovered. In shape it corresponds exactly with that of an ape, and its worn face shows that it worked upon the upper canine in the true ape-fashion. It only differs from the canine of my published restoration in being slightly smaller, more pointed, and a little more upright in the mouth. Hence, we have now definite proof that the front teeth of *Eoanthropus* resembled those of an ape."

In regard to the question whether the lower jaw pertains to the same individual as the cranium, or skull proper, the lecturer expressed his views in the following words :

" We can only state that its molar teeth are typically human, its muscle-markings are such as might be expected, and it was found in the gravel near to the skull. The probabilities are therefore in favour of its natural association. If so, it is reasonable to suppose that the skull will prove to be that of a very lowly kind, not that of a highly civilised man. I have accordingly made a new study of the specimen . . . and find that the only alteration necessary in our original model is a very slight displacement of the occipital and right parietal bones."

Finally, he sums up by remarking that "in *Eoanthropus* we have a human being with a distinct remnant of ape-like ancestors in his jaws ; and in the human mandible, probably of the same period, found near Heidelberg, we have a slightly more advanced stage with teeth which are distinctly human. When the Pliocene forerunners of these species are found, they will probably fall rather into the category of apes than of man.

" Next, in connection with the remarks I have made about the evolution of the brain in mammals, it is interesting to notice that the brain of *Eoanthropus* makes a much nearer approach to that of modern man than his face. It therefore appears that the excessive development of the brain preceded the loss by the mouth of its functions as a weapon. Increase of intelligence removed the necessity for so much brute force, and the face then became reduced in size, while the familiar weakness of the jaws of man was the result."

It should be added that in the *Geological Magazine* for October 1913 (decade 5, vol. x. pp. 433–4) Dr. Smith Woodward published a short supplementary note on *Eoanthropus*, with a figure of the amended restoration of the whole skull, together with one of the lower jaw containing the newly found canine in position.

Many years ago Dr. Ameghino described a small monkey from the Patagonian Miocene under the name of *Homunculus*, and considered that it showed affinity to the human phylum. Mr. H. Bluntschli (*Verh. Anat. Ges.* 1913, pp. 33–43) has now shown that *Homunculus*, together probably with *Anthropops* and *Pitheculus*, is nearly allied to the existing South American douroucolis (*Nyctipithecus*). On the other hand,

a number of other Patagonian genera referred by Ameghino to the Primates do not belong to that order, some, such as *Pitheculites* and *Homunculites*, being apparently marsupials, while others, like *Archæopithecus* and *Henricosbornia*, seem to be ungulates.

In this place attention may be directed to a few faunistic mammal papers, among which reference may first be made to one by Mr. J. W. Gidley (*Proc. U.S. Nat. Mus.* vol. xlvi. pp. 29–102), recording the results of the exploration of a cavern near Cumberland, Maryland, U.S.A. The remains include those of a bear closely related to the existing *Ursus americanus*, but with larger tusks, which has been named *U. vitabilis*, and also of a dog, *Canis ambusteri*, of the size of a wolf, but with lower carnassial teeth resembling those of a coyote.

Brief reference may also be made to a popular article contributed by Dr. W. D. Matthew to the *American Museum Journal* for November 1903 on the vertebrate remains discovered in the great asphalt-springs of Rancho La Brea, California, which formed during the later part of the Tertiary period a death-trap for the fauna of the adjacent country. Remains of fully fifty species of birds have been identified, and there were probably as many mammals; remains of wolves, lion-like cats, sabretoothed tigers, eagles, and vultures being the most common, while next in abundance are those of bisons, horses, and gigantic ground-sloths, as well as of wading-birds. On the other hand, bones of the smaller mammals and birds are comparatively rare. The obvious corollary from this is that the aforesaid large mammals ventured heedlessly on to the apparently solid surface of the treacherous asphalt, in which they soon became hopelessly bogged and condemned to a lingering death by suffocation or starvation. While thus hopelessly trapped, they served as lures to attract all the beasts and birds of prey within sight, which in their turn became enmired, and thus drew others of their kin to the fatal snare. So things went on from year to year and from century to century, with the result that the palæontologist has now a rich museum of the remains of the old fauna of the country ready to his hand.

In connection with cavern and other superficial formations, reference may be made to the identification by Mr. J. W. Jackson (*Geol. Mag.* decade 5, vol. x. pp. 259–62) of remains of the lynx from caves in North Wales and Derbyshire. Here, too, may be

mentioned a paper by Dr. J. C. Merriam (*University of California Publications, Bull. Dep. Geol.* vol. vii. pp. 373–85) on the vertebrate fauna of the Orindan and Siestan beds of California, which are of Miocene age.

Another faunistic paper is one by Mr. H. G. Stehlin (*Bull. Soc. Géol. France*, ser. 4, vol. xii. pp. 198–212, 1912) on the palæontology of the Tertiary sands of Rosières, near St. Florent, Cher. A new species of *Cervus* is described.

Of wider interest is an article by Dr. Ernst Stromer (*Zeits. deutsch. Geol. Ges.* vol. lxv. pp. 350–72) on the Middle Pliocene fauna of the Wadi Natrun, Egypt, in which, among numerous other forms, an extinct otter is described as new, under the name of *Lutra libyea*.

Reverting to systematic work on mammals, the next paper for notice is one by Dr. W. D. Matthew (*Bull. Amer. Mus. Nat. Hist.* vol. xxxi. pp. 307–14), on the skull of a new type of the Insectivora—*Palæoryctes puercensis*—from the Puerco, or Lowest, Eocene of New Mexico. It is referred to the primitive and scattered group now represented by the solenodons (*Solenodontidæ*) of the West Indies, the otter-shrew (*Potamogale*) and the golden moles (*Chrysochloridæ*) of Ethiopian Africa—a convenient term to denote that part of the African continent lying to the south of the northern tropic—and the tenrecs (*Centetidæ*) of Madagascar; the affinity with the last being so close that Dr. Matthew is inclined to include the extinct genus in the same family. Here it should be mentioned that although the group is now unknown on the continent of America, it was represented in Patagonia during the Miocene by *Necrolestes*, which appears to have been nearly related to the golden moles, and also by four more or less closely related genera in North America. The problem now awaiting solution is whether the living and extinct southern members of these Zalambdodont Insectivora, as the whole group is called, reached their respective habitats by means of one or more land-bridges between the great southern continents, which were almost certainly in existence during the early part of the Tertiary period, or whether they travelled southwards from the northern hemisphere by independent routes.

Be this as it may, the newly described genus seems to indicate that the *Centetidæ* are the oldest existing family of placental mammals. It likewise points to the great antiquity of the triangular, or tritubercular, type of molar tooth, which forms a

distinctive feature of all the members of the group under consideration.

Mention of the tritubercular type of dentition leads conveniently on to a paper by Mr. K. S. Bardenfleth (*Vidensk. Meddel. Dank. naturh. Foren*, vol. lxv. pp. 61-111) on the form and structure of the carnassial teeth of Carnivora—this paper being, of course, only in part palæontological. Its chief interest, from the latter standpoint, is concentrated on a discussion as to the possibility of the tritubercular molar being formed by a rotation of two of the three longitudinally arranged cusps of a tooth like that of the Mesozoic *Triconodon*. The author adduces evidence to show that, in the first place, such a rotation of the cusps could not have taken place, and, secondly, if it did, the cusps are not respectively homologous with those of the tritubercular molar. He adds that "if this holds good, the whole nomenclature and theory of Osborn falls to the ground."

Turning to the Carnivora, it may be noticed that Dr. Merriam has considerably extended our knowledge of fossil *Canidæ* by a study of the osteology and dentition of the North American Tertiary genus *Tephrocyon* in a paper issued in the *Publications of California University, Bull. Dep. Geol.* vol. vii. pp. 359-72. In the opinion of the author, the genus in question was to a considerable extent intermediate in dental and skeletal structure between the extinct American *Ælurodon* and modern wolves and jackals (*Canis*); its range extending from the middle portion of the Miocene to the early part of the Pliocene period.

Remains of the small bear known as *Ursus etruscus*, or *arvernensis*, from the Pliocene of Tegelen-sur-Meuse, form the subject of a paper by Mr. E. T. Newton, published in the *Verhand Geol.-Mijnbouw, Genoots. Nederl. en Kolon. Geol.* ser. 1, 1913, pp. 249-54. Hyæna remains from the Pleistocene of the Lower Rhine in the neighbourhood of Mosbach have been referred by Dr. H. Pohlig (*Bull. Soc. belge Géol.* vol. xxvii. *Proc.-Verb.* p. 147) to a new race of the striped species (*Hyæna striata trogontherii*).

The only paper on fossil rodents that has come under my notice is one by Dr. T. Studer (*Mitt. naturfor. Ges. Bern*, 1913, 8 pp.) on remains of marmots from the European Diluvium. Many of these belong to the large form of the Alpine species known as *Arctomys marmotta primigenia*, but those from Bohemia are identified with *A. bobac* of Eastern Europe.

Among the numerous papers on fossil ungulates which have been published during the year, the first place may be accorded to one by Dr. O. P. Hay (*Proc. U.S. Nat. Mus.* vol. xlvi. pp. 161–200) on the extinct North American bisons. After a review of the large number of previously described species, with figures of the skulls of many of them, the author describes a new one, on the evidence of a Kansas skull, as *Bison regius.* This skull (fig. 1) differs from that of its near relative *B. latifrons*, from the Pleistocene formation of Ohio, by the longer, more slender, and more highly curved horn-cores. Such a difference might, indeed, be merely sexual, but as the enamel-islets in the crowns of the upper molars display a folding which is not found in those of the
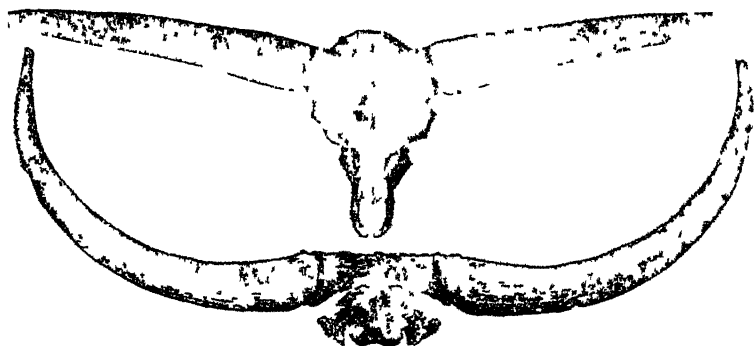
FIG. 1.—Front and back views of skull of the extinct Kansas
Bison (*Bison regius*).

Ohio species, the author feels justified in regarding the Kansas bison as distinct. Remains of cattle from the Pleistocene of Pianosa Island, Italy, are referred by Mr. G. de Stefano, *Bull. Soc. Geol. Ital.* ser. 3, vol. xii. pp. 50 and 70, to the new species *Bos bubaloides* and *B. intermedius.*

In vol. lx. No. 27 of the *Smithsonian Miscellaneous Collections* Mr. J. W. Gidley refers an associated series of five upper cheek-teeth of a large ruminant from a Pleistocene cave-deposit near Cumberland, Maryland, U.S.A., to the existing African genus *Taurotragus*, under the name of *T. americanus.* Although elands are now restricted to Ethiopian Africa; the present writer (see *Cat. Siwalik Vert. Ind. Mus.* part. i. p. 18, 1885) has provisionally referred certain teeth from the Indian Siwaliks to

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Taurotragus (= *Oreas*), and if this identification be correct, it would tend to show how elands might have reached America from Asia by the Bering Sea route. Mr. Gidley refers, moreover, to the occurrence in the Pleistocene of Nevada of remains of certain ruminants (*Ilingoceros* and *Sphenophalus*) as corroborative evidence of the former existence of tragelaphine or eland-like antelopes in America, although omitting to mention that these genera are regarded by Dr. Merriam (*Univ. California Publications, Bull. Dep. Geol.* vol. vi. p. 292) as akin to the American family *Antilocapridæ*; and if this be correct, it does not seem impossible that the supposed eland represents another member of the same group, as on distributional grounds it is highly improbable that *Taurotragus* should occur in America. This is also the opinion of Dr. P. Matschie, who, when describing a new African race of eland (*Sitzber. Ges. nat. Freunde, Berlin*, 1913, p. 294), takes occasion to state that he can see nothing particularly eland-like in the Maryland teeth.

In connection with elands, it may be mentioned that, in an article on the association of man with extinct mammals in South Africa, Dr. R. Broom (*Ann. S. African Museum*, vol. xii. pp. 13–16) has described remains of certain antelopes apparently representing extinct species of existing African genera. One of these, *Connochætes antiquus*, is of particular interest on account of being, in the opinion of its describer, intermediate between the two existing species of gnus.

An important memoir by Dr. J. Chomenko (Khomenko) on the Tertiary ruminants of Taraklia, Bessarabia, is published in the *Annuaire géol. et min. Russ.* vol. xv. pp. 107–43, with a French translation of the first part. Antelopes, gazelles, sivatheroids, and giraffes are abundantly represented in this fauna, which serves to connect that of Pliocene India with modern Africa. In the hollow-horned group, *Criotherium*, typically from the Pliocene of Samos, is placed with the hartebeests, while further indications of African affinities are displayed by *Procobus*, a genus of antelopes akin to *Cobus*. Three species of deer are also assigned to new genera, namely, *Cervocerus*, *Cervavitus*, and *Damacerus*; the first two of these being apparently related to the Asiatic rusine group, while the third is considered to be allied to the Mesopotamian fallow group. All three are referred to an extinct subfamily, for which

the name *Pliocervinæ* is suggested, but as there is no such genus as *Pliocervus*, this is obviously inadmissible.

Reverting to America, it may be noticed that in the *Publications of California University, Bull. Dep. Geol.* vol. vii. pp. 335–9, Dr. Merriam has described a peculiar type of horn or antler from the Orindan Miocene of California, which he tentatively assigns to the extinct genus *Merycodus*, that genus being apparently more or less closely allied to the modern prongbuck (*Antilocapra*).

For two short papers on deer (*Cervidæ*), one by Mr. L. Joleaud (*Bull. Soc. Géol. France,* ser. 4, vol. xii. pp. 468–71) on the systematic position of *Cervus pachygenys*, of the Algerian Pleistocene, and the other, by Mr. E. Kiernik (*Bull. Ac. Sci. Cracovie,* 1913, pp. 449-69), on antlers of *Dicrocerus* from Poland, bare mention will suffice.   Reference has already been made 'to Mr. Stellin's description of a new Tertiary *Cervus* from France.

More interest attaches to a couple of papers on fossil North American camels, in the first of which Mr. Gidley (*Smithson. Misc. Collect.* vol. lx. No. 26) records the occurrence of a toe-bone of a camel in a superficial deposit at the mouth of the Old Crow River, in the Yukon Territory, in association with remains of mammoth, horse, and bison.   The occurrence of the camel-bone confirms, to quote the author's own words, " the theory of the existence of a wide Asiatic-Alaskan land-connection of comparatively recent date, which for a very considerable length of time served as a great highway for the free transmission of mammals between America and the Old World."   In discussing the question whether the Pleistocene North American camels described as *Camelops*, of which seven species are recognised, are really distinct from the South American llamas (*Lama*, or *Auchenia*), Dr. O. P. Hay (*Proc. Amer. Mus. Nat. Hist.* vol. xlvi. pp. 161-200) points out that the northern forms lack the vertical ridges at the antero-external angles of the last two lower molars distinctive of their southern cousins, while their skulls are also longer and narrower, with the upper part of the nasal bones less expanded, the crowns of their upper molars have larger grinding surfaces, and the lower incisors are less proclivous.   It may be mentioned that the ridge in the lower molars of the llama group is also developed in the corresponding teeth of the true camels of the Pliocene of the Siwalik Hills, Northern India.

It has for some time been known that the brain of the African aardvark (*Orycteropus*) exhibits a distinct approximation to the ungulate type; and, in a paper contributed to the *Proceedings of the Zoological Society* for 1913 (pp. 878-93), Mr. R. W. Palmer has shown that this resemblance is most pronounced when the brain of *Orycteropus* is compared with the cast of that of the Oligocene artiodactyle genus *Anoplotherium*. So marked, indeed, is the general similarity of the two structures as to lead the author to remark that "if cerebral anatomy be of any systematic value, the view that *Anoplotherium* and *Orycteropus* arose from a common, though necessarily remote, ancestry can hardly be doubted."

The pig-like *Anthracotheriidæ* of the Miocene or Oligocene strata of the Bugti Hills, Baluchistan, form the subject of a preliminary communication from Mr. C. Forster-Cooper, published in the *Annals and Magazine of Natural History*, ser. 8, vol. xii. pp. 514-22. These are referred to several new species and one new genus. Personally the present writer is much interested in the reference of one of these to *Hemimeryx*, a genus established by himself, with a certain degree of trepidation, some thirty years ago, on the evidence of a single upper molar tooth from the Siwaliks of the Punjab. The great numerical abundance of members of the anthracothere group, especially of the genus *Brachyodus*, is a notable feature of the fauna of the Bugti beds.

In connection with the above reference may be made to a note by Mr. Guy Pilgrim in the *Records of the Geological Survey of India*, vol. xliii. pp. 74 and 75, amending the generic designations of certain Bugti mammals. The most interesting item in connection with the Bugti fauna is, however, the description by Mr. Forster-Cooper (*Ann. Mag. Nat. Hist.* ser. 8, vol. xii. pp. 376-81) of a gigantic perissodactyle ungulate, which must apparently have exceeded an ordinary elephant in bulk. Unfortunately the generic name *Thaumastotherium*, proposed in the original description, proved to be pre-occupied, and it was accordingly replaced later on in the same volume by *Baluchitherium*, with the specific affix *osborni*. The skull and dentition of this monster are not yet known, certain jaws and molars described as *Paraceratherium bugtiense*, which are of a rhinoceros-like type, being relatively small in comparison with the huge dimensions of the vertebræ and limb-bones. Never-

theless, I am inclined to think some of them pertain to the new genus. The femur lacks a third trochanter.

In a paper published in vol. xxii. (pp. 407–20) of the *Bulletin of the American Museum of Natural History* Prof. H. F. Osborn makes a further contribution to his favourite study of the skulls of the horned ungulates of the families *Uintatheriidæ*, and *Titanotheriidæ*, dealing in this instance with species from the Wind River Lower Eocene of Wyoming. A very interesting point is that in the members of the family *Uintatheriidæ* characteristic of this stage, such as *Bathyopsis*, the skull lacks the great bony horn-cores of the later types, their place being taken by small insignificant bony knobs. In the perissodactyle family *Titanotheriidæ* it has been found that two phyla of the genus *Eotitanops* are recognisable, one comprising relatively small, persistently primitive light-limbed species, and the other animals of a larger and more progressive type. Several new species are named in the course of the article.

From the point of view of geographical distribution special interest attaches to the description by Mr. E. de L. Niezabitowski (*Bull. Ac. Sci. Cracovie*, 1913, pp. 223–5) of part of the skull of a rhinoceros from the Tertiary of Odessa, which is referred to the extinct American genus *Teleoceras*, under the name of *T. ponticus*. It is one more instance of the affinity between the Tertiary faunas of Eastern Europe and North America.

A Pleistocene rhinoceros from the Lower Rhine in the neighbourhood of Mosbach has been made the type of a new race by Dr. Hans Pohlig (*Bull. Soc. belge Géol.* vol. xxvii. *Proc.-Verb.* p. 145), under the name of *Rhinoceros mercki mosbachensis*, Falconer's *R. etruscus* being also regarded as a race of the same species.

North American Tertiary horses belonging to the modern genus *Equus* form the subject of a paper by Dr. Hay, published in the *Proceedings of the U.S. National Museum*, vol. xliv. pp. 569–94. Four species are described as new, two of these being based on teeth alone, while each of the other two is represented by the skull. As this paper is very technical, and therefore of interest only to specialists, fuller notice would be out of place on the present occasion, but the following passage in reference to the difficulties incidental to the study of fossil horses may be quoted:

" It may be perfectly obvious that two species are

present, and that they differed in size; but the teeth of the larger individuals of the smaller species may equal in size the teeth of the smaller individuals of the larger species. The matter is likewise complicated by the fact that [in all horses] the premolars are larger than the molars of the same individual."

As the result of the acquisition of additional remains, Dr. Broom (*Bull. Amer. Mus. Nat. Hist.* vol. xxxii. pp. 437–9) has been enabled to give further particulars with regard to the affinities of the extinct South African horse described by himself in 1909 under the name of *Equus capensis.* These are stated to indicate a heavily built, short-legged species, standing about fourteen hands, and apparently distinct from all the existing South African members of the genus, as well as from the Arab stock.

The only literature relating to extinct tapirs published during the year appears to be a note by Dr. Merriam (*Pub. California Univ., Bull. Dep. Geol.* vol. vii. pp. 169–75) on a lower molar of a tapir obtained many years ago from the auriferous gravels of California. It is considered to represent a new race of a species described by Leidy from the Pleistocene of South Carolina. To this race (*Tapirus haysii californicus*) is also provisionally referred a set of three upper molars from the late Tertiary of Oregon. *T. haysii* appears to be nearly related to the existing Central American *T. bairdi.*

Several papers on extinct elephants have appeared during the year, notably one in the *Palæontographica* (vol. lx. pp. 1–114) by Mr. Wolfgang Soergel on the relationship and phylogeny of *Elephas trogontherii* and *E. antiquus,* and their value in the matter of zoning the German Diluvium. The fossil elephants of the Pleistocene of St. Acheul and Montières form the subject of a paper of six pages by Mr. V. Commont, published in *Bull. Soc. Linn. Nord France* for 1912 (1913); they included *trogontherii antiquus,* and *primigenius,* the first of these being regarded as a race of *meridionalis.* In a third paper, by Mr. H. Pohlig (*Bull. Soc. belge Géol.* vol. xxvii. *P.V.* pp. 142–7), the occurrence of *trogontherii* (regarded as a race of *primigenius*) on the Lower Rhine near Mosbach is recorded.

Stegodont elephants from the Kendeng beds of Java form the subject of a memoir by Mr. Soergel in the *Palæontographica,* suppl. iv. pp. 1–24, most of which are referred to *Stegodon airawana* and *S. trigonocephalus.*

The phylogeny of the whalebone-whales is discussed in an

article by Dr. O. Abel which I have not yet seen; the subject being as much zoological as palæontological, bare mention of the communication must suffice. Nearly as brief notice must also suffice for an article by Mr. Gidley (*Proc. U.S. Nat. Mus.* vol. xliv. pp. 649-54) on a remarkably fine skeleton of a zeuglodont recently set up in the American Museum. For these primitive whales the author retains the extremely inappropriate name *Basilosaurus*, despite the fact that it was replaced by its sponsor, Sir R. Owen, by *Zeuglodon* when the mammalian nature of the remains, which were at first regarded as pertaining to a reptile, became apparent.

In an article on the ancestry of the mammals of the order Edentata, published in the *American Museum Journal* (vol. xii. pp. 300-3), Dr. Matthew, after mentioning that armadillos are probably the most primitive existing members of the whole group, and that remains of "armadillos without armour" occur in the early North American Tertiary, observes that although neither the latter nor the rodent-like tæniodonts of the North American Eocene can be regarded as direct ancestors of the typical South American edentates, such as sloths and anteaters, yet they suggest the possibility that the group originally came from North America, penetrated to South America about the beginning of the Tertiary period, where they developed into a host of new forms, which constituted a most important element in the fauna of the country.

Remains of ground-sloths of the genera *Nothrotherium* and *Megalonyx* from the Pleistocene of Southern California form the subject of an article by Mr. Chester Stock (*Univ. California Pub., Bull. Dep. Geol.* vol. vii. pp. 341-50), in which a new species of each genus is named and described; most of the bones being from the asphalt-beds of Rancho La Brea, referred to in an earlier paragraph of the present review. As *Megalonyx* is typically from the North American Pleistocene, while *Nothrotherium* (formerly *Cœlodon*) is Brazilian, Southern California is precisely the locality where the two might be expected to be found in association. Remains of both genera are much less abundant at Rancho La Brea than are those of *Mylodon*.

As regards marsupials, the only paper that has come under my notice is one by Dr. E. C. Stirling (*Mem. R. Soc. S. Australia, a*, vol. i. pp. 128-78), in which conclusive evidence is adduced to show that the upper incisors described by Sir R. Owen as

Sceparnodon really pertain, as first suggested by myself, to the giant wombat, *Phascolonus gigas*.

Two articles by Dr. R. W. Shufeldt on fossil birds have appeared during the year. The most interesting item in the first of these (*Bull. Amer. Mus. Nat. Hist.* vol. xxxii. pp. 285–306) relates to certain bones of the gigantic Eocene birds originally described by the late Prof. E. D. Cope as *Diatryma*, the type species being from New Mexico. The new specimens, for which the name *D. ajax* is suggested, are from the Wasatch Eocene of Wyoming, and, with the exception of a couple of toe-bones, are in a fragmentary condition. Nevertheless, the author considers himself justified in making the astounding statement that the bird to which they pertained was "fully double the size of *Diatryma gigantea* of Cope, and that it may possibly have attained a height of over thirty feet," or nearly five times that of a big ostrich! The other bones described, which are from various horizons, are referred for the most part to birds of prey and game-birds ; a new genus, *Palæophasianus*, of the latter group being named on the evidence of imperfect bones from the Wasatch Eocene of Wyoming.

In the second communication (*Journ. Geol.* vol. xxi. pp. 628–52) Dr. Shufeldt discusses fossil specimens in which the imprints of birds' feathers are preserved, commencing with those of *Archæopteryx*. Such impressions, accompanied by one leg, on slabs from the well-known Green River Shales of Wyoming are made the type of a new genus and species of perching-bird, under the crude name of *Yalavis tenuipes*, with the significant remark that "birds of the same genus and species may or may not be still in existence ; the probabilities are that they are not."

To vol. i. (pp. 111–26) of the *Memoirs of the Royal Society of South Australia* Dr. E. C. Stirling contributes an account of additional remains of the giant Australian bird, *Genyornis newtoni*, from Lake Cadibona.

Papers on reptiles, especially those of South Africa, have been unusually numerous during the year. The first for consideration is one by Mr. R. W. Hooley (*Quart. Journ. Geol. Soc.* vol. lxix. pp. 372–422) on the skeleton of a large pterodactyle from the Wealden of Atherfield, Isle of Wight, referred to the genus *Ornithodesmus*, under the name of *O. latidens*. Whether this reference is correct seems open to doubt, as it is stated in

the report of the discussion following the reading of the paper that the generic name *Ornithodesmus* was " applied originally to a number of fused vertebræ which differ materially from either of the two groups of fused vertebræ in the specimen under consideration." It is accordingly quite probable that the generic designation may have to be changed, although this will in no wise detract from the value of the communication as illustrating an altogether peculiar type of the ornithosaurian order. So markedly distinct, indeed, is this Wealden ptero-dactyle that, in the opinion of the author, it should be regarded as the representative of a distinct family, the serial position of which is indicated in the following revised classification of the Ornithosauria proposed by Mr. Hooley:

I. Suborder SCAPHOGNATHOIDEA.
    1. Family Scaphognathidæ.
        Genera *Scaphognathus* and *Dimorphodon.*
    2. Family Ornithodesmidæ.
        Genus *Ornithodesmus.*

II. Suborder PTERODACTYLOIDEA.
    Family Pterodactylidæ.
        Genera *Pterodactylus* and *Ptenodraco.*

III. Suborder RHAMPHORHYNCHOIDEA.
    1. Family Rhamphorhynchidæ.
        Genera *Rhamphorhynchus, Dorygnathus,* etc.
    2. Family Ornithostomatidæ.
        Genera *Ornithostoma* (= *Pteranodon*) and *Nycto-*
           saurus (= *Nyctodactylus*).
    3. Family Ornithochiridæ.
        Genus Ornithochirus.

For the distinctive character of the new Wealden species and its probable relationships, reference must be made to the original paper, in which the osteology is described in great detail.

Turning to dinosaurs, the first papers for notice are two by Mr. Barnum Brown (*Bull. Amer. Mus. Nat. Hist.* vol. xxxii. pp. 387–407) on the skeletons of *Saurolophus osborni,* a duck-billed dinosaur of the family *Trachodontidæ,* and of *Hypacro-saurus altispinus,* a new genus and species of the same family,

both from the Upper Cretaceous beds of Edmonton, Alberta, Canada. The type skeleton of the first of these, which measures about 32 ft. in length, or nearly the same as that of the contemporaneous *Trachodon mirabilis*, has been mounted on a slab for exhibition in the American Museum This genus, it appears, is much more numerously represented in the Edmonton beds than is its cousin *Trachodon.* The skeleton of the second
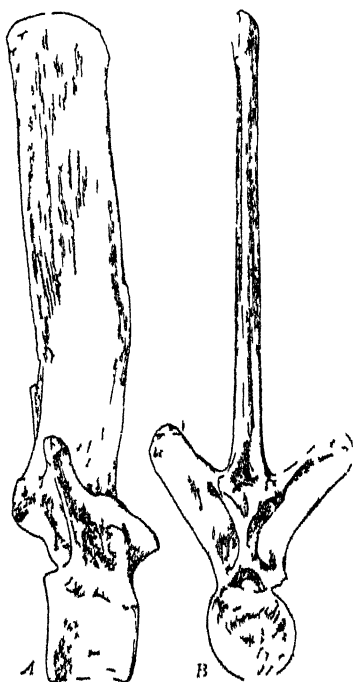
FIG 2.—Lateral (A) and posterior (B) aspects of dorsal vertebræ of *Hypacrosaurus altispinis.*

(From *Bull Amer Mus Nat Hist*)
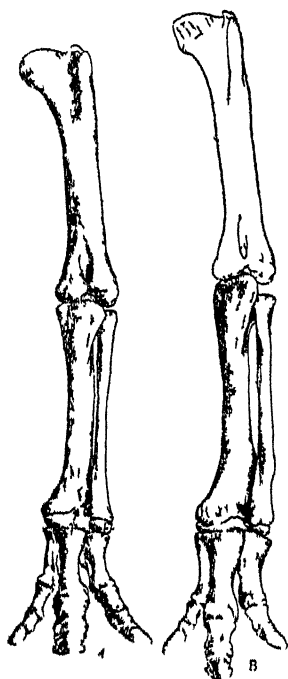
FIG 3 —Skeleton of the left hind limbs of *Trachodon* (A) and *Hypacrosaurus* (B).

(From *Bull Amer Mus Nat Hist*)

genus, *Hypacrosaurus*, is characterised by the great height of the spines of the dorsal vertebræ (fig. 2), coupled with the presence of nine vertebræ in the sacrum, against eight in the allied genus. In all the members of the family the hind limbs were tridactylate ; the distinctive features of those of *Trachodon* and *Hypacrosaurus* being shown in fig. 3.

The skeleton of the fore-limb of a species of *Trachodon* from

the Edmonton Cretaceous forms the subject of a note by Mr. L. M. Lambe in the *Ottawa Naturalist*, vol. xxvii. p. 21. In a second article in the same volume (pp. 109–16) Mr. Lambe describes a new generic type of horned dinosaur, *Styracosaurus albertensis*, from the Edmonton beds, in which the margin of the great posterior flange of the skull carries a series of long spines.

Reference in this place may be made to a note in vol. ix. No. 11 of *The South African Journal of Science*, relating to the discovery in the Lower Cretaceous marls of Bushman's River, South Africa, of the broken femur of a presumably dinosaurian reptile fully as large as the corresponding bone of *Diplodocus*, and, when complete, measuring about 5 ft. in length. Here it may be mentioned that the preoccupied generic name *Gigantosaurus* used by Dr. Fraas for the gigantic dinosaur from the Cretaceous of German East Africa has been replaced by *Tornieria* (Sternfeld, *Sitzber. Ges. Nat. Freunde*, 1911, p. 398).

In the Parasuchia (Belodontia) remains of a new species of the genus *Rutiodon*, from the Upper Triassic beds of Fort Lee, New Jersey, at the base of the " Palisades," opposite New York, are described by Prof. H. von Huene (*Bull. Amer. Mus. Nat. Hist.* vol. xxxii. pp. 275–83), under the name of *R. manhattanensis*. In the opinion of the describer, the members of the genera *Rutiodon* and *Mystriosuchus*, on account of the taller spines of their vertebræ and their more compressed bodily form, were probably better swimmers than those of the typical genus *Phytosaurus*. The species of both the American *Rutiodon* and the European *Mystriosuchus* were long-snouted reptiles of larger bodily size than *Phytosaurus*; the newly described representative of the first of these being the biggest of the whole group.

More or less nearly related to the Parasuchia is the group of early reptiles known as the Pseudosuchia, certain members of which form the subject of a paper contributed by Dr. Broom to the *Proceedings of the Zoological Society for* 1913 (pp. 619–33). The main object of this communication is the description of the skull and skeleton of a rhynchocephalian-like reptile from the Trias of Aliwal North, South Africa, for which the author had previously proposed the new generic and specific designation *Euparkeria capensis*. The opportunity is, however, taken of discussing the osteology of the genera *Ornithosuchus* and

Herpetosuchus, from the Trias of Elgin, with the description of a new species of the former; and likewise of considering the affinities of the South African *Mesosuchus*, which it is suggested may not be parasuchian at all. The paper concludes with a discussion of the affinities of the Pseudosuchia as a whole, in the course of which the writer, after alluding to the views of other palæontologists, expresses himself as follows:

" There cannot, I think, be the slightest doubt that the Pseudosuchia have close affinities with the dinosaurs, or at least with the Theropoda. . . . In fact there seems to me little doubt that the ancestral dinosaur was a pseudosuchian. The skulls of such types as *Euparkeria* or *Ornithosuchus* are practically dinosaurian even in detail, and the skulls of the early dinosaurs, such as *Anchisaurus*, differ less from the skulls of pseudosuchians than do those of the early dinosaurs from many of the later types. And there is nothing in the post-cranial skeleton that is not just what we should expect to find in the dinosaur ancestor. . . .

" Another group to which the pseudosuchians seem to have affinities . . . is the Ornithosauria. In general proportions the pterodactyles differ very greatly, but the form from which they arose must have been very much like that seen in the pseudosuchians. The pterodactyle and pseudosuchian skull are almost exactly similar in essentials. . . .

" There is still another group to which some pseudochian has probably been ancestral, namely, the birds."

Crocodiles of the families *Teleosauridæ* and *Geosauridæ* form the subject of the second half of vol. ii. of the British Museum " Catalogue of the Marine Reptiles of the Oxford Clay," the genera included being *Stencosaurus, Mycterosuchus* (new), and *Metriorhynchus*.

In a communication which did not come under notice when writing the palæontological review for 1912, Mr. C. W. Gilmore described (*Proc. U.S. Nat. Mus.* vol. xli. pp. 479 *et seq.* 1912) a new generic type of mosasaur, or " sea-serpent," from the Cretaceous of Alabama, remarkable for having teeth of a blunted character adapted for crushing hard substances; this feature being expressed in the generic name *Globidens*, to which is added the specific title *alabamaënsis*. About a year later Prof. L. Dollo was enabled to record (*Archiv Biol.* vol. xxviii. pp. 609–20) the occurrence of a very similar mosasaur in the Maestrict Cretaceous, which he referred to the same genus,

under the name of *G. fraasi.* It differs from the American species by the teeth being laterally compressed, instead of hemispherical. In both cases the food probably consisted, according to Prof. Dollo, of echinoderms; such a diet being, of course, indicative of diving habits. In the allied genus *Plioplatecarpus*, belemnites and other cephalopods may have constituted the staple food, in which case these reptiles would likewise have been divers. On the other hand, the members of the typical genus *Mosasaurus*, in which the dentition is of a highly carnivorous type, were probably fish-eating surface-swimmers.

In connection with this part of the subject, reference may be made to a paper by Dr. R. Broom (*Bull. Amer. Mus. Nat. Hist.* vol. xxii. pp. 507-8) on the squamosal and associated bones of mosasaurs and lizards, in which the author endorses the view that the outermost of the two bones which have been alternately regarded as representing the squamosal is really that element. This view is strongly supported by the condition obtaining in the carnivorous anomodonts, in which the structure of this region is so mammal-like as to leave no doubt which bone is the squamosal, and the outermost of the two bones in the corresponding region of the skulls of lizards and mosasaurs appears to be the homologue of the former. If this be correct, the inner bone will apparently represent the tabulare; and as this is a primitive element, it would seem to follow that lizards are really a very ancient group.

The preceding paragraph naturally leads to the consideration of a letter from Dr. Broom, published in *Nature* on the vomer of dicynodonts. After referring to the fact that a pair of bones—the prevomers—in the fore part of the palate of lizards seem to represent the two elements in the mammal *Ornitho-rhynchus* which eventually unite to form the so-called dumb-bell bone, the author proceeds to observe that while some of the carnivorous anomodonts seem to have a single mammal-like vomer and a pair of bones in front, in the dicynodonts only the former element is present; other carnivorous forms (thero-cephalians), on the contrary, have a pair of large anterior elements and no median bone. To solve the problem a specimen showing a large median true vomer and a pair of prevomers was essential, and this has turned up in the shape of a dicy-nodont skull in which the median element lying between the

posterior paired bones represents the anchylosed prevomers. Above, and completely concealed by this element, is a large, well-developed, typically mammalian median vomer extending from the basisphenoid behind to the premaxillæ in front. Along its upper side the vomer is grooved for the large basal and ethmoidal cartilage; while posteriorly it is closely united to the basisphenoid. This confirms the view that the mammalian vomer is the reptilian parasphenoid, and thus quite different from the prevomers.

Passing on to Chelonia, it may first be noticed that a remarkable new generic type of the side-necked or pleurodiran group from the Keuper, in the neighbourhood of Stuttgart, has been described by Prof. O. Fraas in the *Jahresheft Ver. Naturkunde Württemberg*, 1913, No. 80, under the name of *Proterochersis robusta*. Its most striking peculiarity consists in the presence of two complete pairs of mesoplastral bones in the lower shell, which is believed to be a unique feature in the order. As a mesoplastron seems to be a primitive feature, its duplication may perhaps represent a still more archaic type.

Two tortoises from the Oligocene of Wyoming form the subject of an article by Mr. L. M. Lambe in the *Ottawa Naturalist* (vol. xxvii. pp. 57–63). The first represents a new species of land tortoise (*Testudo præexstans*), characterised by the great development of the horn-like epiplastra on the front edge of the lower half of the shell—a feature displayed to a minor degree in *T. thomsoni* of the Oligocene of South Dakota. The second is referred to a well-known Tertiary species, *Stylemys nebrascensis*.

Reference may also be made to a paragraph in *Nature* based on a cutting from the *Daily Malta Chronicle* of February 17, 1913, in which Mr. N. Tagliaferro records the discovery in a rock-fissure at Corradino of a large series of remains of giant land-tortoises. Many of these, it is stated, are referable to *Testudo robusta* and the smaller *T. spratti* of Leith Adams; but one specimen indicates a tortoise nearly half as large again as the biggest described example of the former, and may, it is suggested, represent a third species. These and other deposits have been deposited in the museum at Valletta.

Leurospondylus ultimus is the name proposed by Mr. Barnum Brown (*Bull. Amer. Mus. Nat. Hist.* vol. xxxii. pp. 605–15) for a new generic type of plesiosaurian from the Edmonton Creta-

ceous of Alberta, Canada, which is of more than ordinary interest on account of being the latest representative of the Sauropterygia at present known, the Edmonton beds being separated from the Eocene Tertiary series only by another set of Cretaceous rocks. Nearly related to the long-necked *Elasmosaurus*, the new type has a neck of medium length, and very short centra to the vertebræ; the total length of the vertebral column being about seven feet. The author gives a synopsis of the chief structural features by which it is distinguished from five other genera of American plesiosaurians. The plesiosaurians of the genus *Murænosaurus* form the subject of a paper by Dr. E. Koken, published in the *Neues Jahrbuch für Min.* 1913, voi. i. pp. 101–15.

The first portion of the above-mentioned "Catalogue of the Marine Reptiles of the Oxford Clay" deals with the pliosaurs of the genera *Pliosaurus*, *Simolestes*, and *Peloneustes*, of the skeletons of some of which Dr. Andrews gives almost complete restorations.

As skulls of ichthyosaurs which have escaped the effects of crush are comparatively rare, considerable interest attaches to an uncrushed specimen of *Ichthyosaurus acutirostris*, from the Lias of Holzmaden, described and figured by Dr. E. Fraas in the *Jahresheft Verh. Naturkunde Württemberg*, 1913, 12 pages.

A large series of papers, chiefly from the pen of Dr. R. Broom, on the mammal-like anomodont, or theromorphous, reptiles of the Permo-Trias of South Africa, have appeared during the year. Before considering those of the author just mentioned reference may, however, be made to an ingenious attempt by Miss I. J. B. Sollas and Prof. W. J. Sollas to solve some of the problems connected with the structure of the skull of the well-known genus *Dicynodon* by means of a series of sections. The results of this investigation are embodied in a report published in the *Philosophical Transactions* (ser. A, vol. cciv. pp. 201–25). No analysis of such an extremely technical communication can, however, be attempted on the present occasion, and it must suffice to refer to a very interesting restoration of the canals of the labyrinth of the internal ear.

Brief mention may be made in this place of a paper by Mr. D. M. S. Watson (*Geol. Mag.* decade 5, vol. x. pp. 388–93) on the beds of the South African Karru series, which, although mainly stratigraphical, deals to some extent with palæontology.

Among the series of papers by Dr. Broom reference may

be made first to one in the *Bull. Amer. Mus. Nat. Hist.* (vol. xxxii. pp. 441–57) in which are described a number of remains of dicynodonts; many of these being regarded as representing new species of the typical *Dicynodon*, while others are assigned to new genera. It is interesting to note that a skull described by Huxley as that of a lizard, under the name of *Pristerodon mackayi*, really represents a dicynodont with cheek-teeth. Two other new species of *Dicynodon* are described by Messrs. Broom and Haughton in vol. xii. pp. 36–9 of the *Annals of the South African Museum*.

In the first of three papers by Dr. Broom in part 6 of the viith of the serial just quoted[1] it is shown that while in *Pariasaurus* the digital formula is 2.3.3.4.3, in the allied *Propappus* it is probably 2.3.4.5.3, thus affording another point of distinction between them. In the second he describes, as *Noteosaurus africanus*, a new genus allied to *Mesosaurus*, of which three of the known species are South African, while the fourth is Brazilian. The third paper contains a systematic list of the early Mesozoic reptiles of South Africa, which, apart from dinosaurs, crocodiles, rhynchocephalians, etc., are arranged in no fewer than nine ordinal groups, brigaded in three so-called superorders.

In vol. xii. of the *Annals of the South African Museum* (pp. 17-24) Messrs. Broom and Haughton describe the imperfect skeleton of a new generic type of pariasaurian from Beaufort West, under the name of *Pareiasuchus peringueyi*. One of the points of difference from the typical genus consists in the larger size of the temporal roof of the skull, which descends below the quadrate, in a manner also found in the Russian pariasaurians, which may, however, represent another genus. In the same issue (pp. 43-5) Mr. H. S. Haughton describes a new species of the allied genus *Propappus*; while he also communicates (*op. cit.* pp. 40-42) a note on a very fine skull of the gigantic *Tapinocephalus atherstonei* from Beaufort West, the locality of the type specimen described by Sir Richard Owen.

If his conclusions are well founded, Dr. Broom, in a paper published in the *Bulletin of the American Museum of Natural History* (vol. xxxii. pp. 465–6), adduces an important piece

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[1] It may be well to mention that instalments of vii. and of vol. xii. of this serial were published during the year.

of evidence in regard to the affinity between certain carnivorous (cynodont) anomodonts and mammals. For he describes and figures, under the new specific name of *Diademodon platyrhinus*, a lower jaw of an anomodont which is stated to furnish evidence of a single successional replacement of the teeth similar to that of mammals. " We may thus safely conclude,"
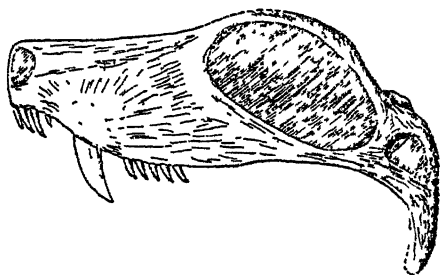
Fig. 4.—Skull, without lower jaw, of *Ictidorhinus martinsi*.
(From *Bull Amer. Mus. Nat. Hist.*)

he observes, " that as the cynodont approaches full maturity the incisors, canines, and premolars are replaced as in mammals, and as no completely adult specimen has ever shown any trace of a later succession, we may conclude as probable that there is only a single succession."

New carnivorous South African anomodonts of the group
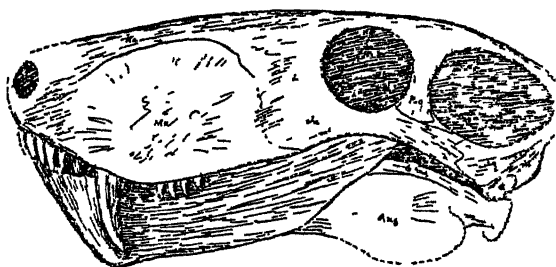
Fig 5.—Skull of *Scymnognathus angusticeps*.
(From *Bull. Amer. Mus. Nat. Hist.*)

Therapsida form the subject of an article by Dr. Broom (*Bull. Amer. Mus. Nat. Hist.* vol. xxxii. pp. 537-61) in which four species are described, one referable to a new genus, under the name of *Ictidorhinus martinsi*. Of the latter the skull (fig. 4) is altogether unique in shape; the peculiarity being in great part due to the unusually large size of the orbits, and the

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consequent crater-like elevation of the pineal foramen, which is situated in the median line between and above them. As an example of the skull and dentition of a more normal type of these most mammal-like reptiles Dr. Broom's figure of the skull of the new species *Scymnognathus angusticeps* is repro-ʲ duced in fig. 5. Both genera are related to *Gorgonops*. Two new species of *Scymnognathus* are described in this paper, and a third is added in an article by Messrs. Broom and Haughton in the *Annals of the S. African Museum*, vol. xii. pp. 26–35.

In the same issue, pp. 8–12, Dr. Broom describes a skull from Beaufort West as a new species of *Gorgonopsis*, a genus originally proposed by the late Prof. H. G. Seeley for a reptile allied to *Gorgonops*, and now revived by Dr. Broom, who seems, however, to have omitted to confer a specific name on the new form.

In a subsequent paper (*Proc. Zool. Soc.* 1913, pp. 225–30) Dr. Broom reviews the structure and affinities of the group Gorgonopsia, which is taken to include *Scymnognathus* as well as *Gorgonops* and *Gorgonopsis*. "Most of the characters," he observes, "in which the Gorgonopsia differ from the Therocephalia are characters in which they agree with the [typical] Anomodontia. The Therocephalia are unquestionably the more primitive group, but there are also some early characters in the Gorgonopsia and also in the [typical] Anomodontia. Of course we only know well one or two of the later gorgonopsians, and we have good reason to believe that the group is very early."

The structure of the gorgonopsid palate forms an important item in a paper by Mr. Watson (*Ann. Mag. Nat. Hist.* ser. 8, vol. xii. pp. 65–72) on certain features in the skulls of the therocephalians; the same author (*op. cit.* pp. 217–28) also contributing an article on the skull, brain, nose, and internal ear of *Diademodon*. The special interest attaching to the sense-organs in this genus is that they appear to indicate the commencement of a change from the lowly reptilian to the higher mammalian type, and it is hence possible that *Diademodon* may have been warm-blooded.

The work of Dr. Broom has not been altogether restricted to the early reptiles of South Africa, for he has contributed two articles to vol. xxxii. of the *Bulletin of the American Museum of Natural History* on some of those of North America. In the

first of these (pp. 509–16) are discussed the structure and affinities of *Bolosaurus*, a genus originally described by Prof. Cope on the evidence of a skull from the Lower Permian of Texas, who regarded it as representing a distinct family of the Cotylosauria, more or less nearly related to the *Diadectidæ*. In the light of new material and fresh investigations, Dr. Broom is of opinion "that the *Bolosauridæ* represent a primitive group of theromorphs near to the common ancestors of the pelycosaurs, varanosaurids, and dromasaurians. Even without knowing anything of the *Bolosauridæ*, we know that these three groups had a common post-cotylosaurian ancestor, and while the bolosaurids are too specialised to have been ancestral, they are probably members of the suborder that included the

Fig. 6.—Skull of *Pantylus cordatus*, to show the closed temporal region of the Cotylosauria.

(From *Bull. Amer. Mus. Nat. Hist.* It has not been considered necessary to give the names of the constituent bones indicated by letters.)

common ancestor. If we place the *Bolosauridæ* in this central position we get a satisfactory explanation of its seeming varied affinities." It is added that a suggested affinity between *Bolosaurus* and the European Permian genus *Palæohatteria* has been rendered the more probable by the discovery of sclerotic plates in the orbits of the former, which have long been known in those of the latter.

In the second article (*op. cit.* pp. 527–32) the author considers the cotylosaurian genus *Pantylus*, likewise described by Cope on the evidence of remains from the Permian of Texas. Dr. Broom's paper is of too technical a nature to be referred to in any more than an incidental manner. At present the genus is known only by the skull (fig. 6); but even from this evidence

alone Dr. Broom is of opinion that "we may safely conclude that it is a cotylosaur, and in the meantime it may be safest to follow Case in keeping it as the type of a distinct suborder—the Pantylosauria."

As containing descriptions both of primitive reptiles and of stegocephalian amphibians, mention may be made of an important and exhaustive memoir on the vertebrate fauna of the Permo-Carboniferous beds of north-central New Mexico by Messrs. Case, Williston, and Mehl, issued, as Publication No. 181, by the Carnegie Institution of Washington. The total number of species from this horizon at present identified includes a shark akin to *Pleuracanthus*, five amphibians, and ten reptiles of a low, although in some cases specialised, type. The most noteworthy of the amphibian remains is a skull described as the representative of a new genus and species, under the name of *Chenoprosopus milleri*; the generic title referring to the superficial resemblance of the specimen to the skull of a goose (*Chen*). This genus belongs apparently to the temnospondylous amphibians, in spite of certain indications of affinity with reptiles. Among undoubted reptiles special interest attaches to the restoration of the skeleton of the pelycosaurian described by O. C. Marsh as *Ophiacodon mirus*, on account of the enormous size of the skull as compared with that of the trunk. According to the figures, the shoulder and pectoral girdles of this and certain allied forms present a striking resemblance to the corresponding elements of African anomodonts.

In close connection with the above is an article by Dr. Broom (*Bull. Amer. Mus. Nat. Hist.* vol. xxxii. pp. 563-5) on the temnospondylous stegocephalians of the North American Permian, which is chiefly devoted to the further elucidation of the structure and affinities of genera, such as *Cricotus*, *Trimerorhachis*, *Eryops*, and *Zatrachys*, described by previous writers. New species of the second and third of these are, however, named; and the communication is of special value on account of the excellent figures of the skulls of all four.

In another communication (*Ann. S. African Mus.* vol. xii. pp. 6 and 7) Dr. Broom describes the skull and skeleton of a comparatively small stegocephalian from the Permo-Trias of the Fraserburg district of South Africa, under the name of *Phrynosuchus whaitsi*; the generic designation (as well, of

course, as the specific) being apparently new, although this is not definitely stated to be the case. The broad flattened head, coupled with the short and nearly straight ribs, suggests relationship to the *Protritonidæ (Branchiosauridæ)*.

In Europe Dr. E. Fraas has described (*Palæontographica*, vol. lx. pp. 275-94) several new species of large labyrinthodont stegocephalians from the Swabian Trias, one of which is referred to *Cyclotosaurus*, as typified by Meyer's *Capitosaurus robustus*; the genus also including the so-called *Capitosaurus stantonensis* of the Warwickshire Keuper. And from the Trias of Heligoland Mr. H. Schroeder has described and figured (*K. preuss. Geol. Landesanstalt* for 1913) a beautifully preserved skull of a member of the same group as a new species of the genus typified by Meyer's *Capitosaurus nasutus*, from the Trias of Bernberg, under the name of *C. heligolandi*.

Yet another paper on stegocephalians is to be found in the *Proceedings of the Zoological Society* for 1913 (pp. 949-62), in which Mr. D. M. S. Watson discusses the osteology and relationships of *Batrachiderpeton lineatum*, a genus and species from the Coal Measures of Northumberland originally described by Messrs. Hancock and Atthey. Its nearest known relative appears to be *Ceraterpeton* of the Kilkenny and Staffordshire Coal Measures, from which it differs by certain features in the structure of the skull—notably the greater development of the posterior "horns"; all such points of distinction being in the direction of greater specialisation. A still more specialised type is represented by *Diplocaulus*, in which not only are the "horns" still longer than in *Batrachiderpeton*, but the nasal bones, which are small in the latter, have altogether disappeared.

Mr. Watson (*Geol. Mag.* decade 5, vol. x. pp. 340-46) has also reviewed in the light of recent knowledge the skull of the small South African temnospondylous amphibian described by Prof. Huxley in 1859 as *Micropholis stowi*.

Finally, remains of two stegocephalians from the Permian of Texas are described by Mr. F. Broili in the *Neues Jahrbuch für Min.* 1913, vol. i. pp. 96-100; one of them, apparently allied to *Diplocaulus*, being referred to a new genus and species under the name of *Goniocephalus willistoni*, while the second is made the type of the new species *Acheloma casei*.

Premising that as my acquaintance with the year's literature relating to fossil fishes is very imperfect, my review of this

portion of the subject must be extremely brief, the first papers for notice are connected with the structure and origin of the primitive types of fin-structure, and the evolution therefrom of the tetrapod limbs of the higher vertebrates. In the first of these Mr. Watson (*Anat. Anzeiger*, vol. xliv. pp. 24 *et seq.*) expresses the opinion that the limbs of the Tetrapoda have been evolved from a reduced archipterygium such as occurs in the crossopterygian genus *Eusthenopteron*. Dr. Broom (*Bull. Amer. Mus. Nat. Hist.* vol. xxxii. pp. 459–64), on the other hand, favours the view that the chiropterygium, as found in *Sauripteris*, is nearer the type from which the tetrapod limb was developed; this form of fin being of particular interest from the fact that it was used partly for progression on land. It is, however, of a too specialised type to have given rise to the tetrapod limb, and the author accordingly surmises the existence of a "presauripterid" type of fin, from which the original tetrapod limb was evolved by the loss of the fin-rays and the disappearance of a considerable portion of the hinder or postaxial elements of the skeleton proper. A diagram—scarcely very convincing—illustrates the mode in which the author believes the five digits of the tetrapod limb to have been produced. "Had six or seven [digits] been retained for a time," remarks the author, "they would have been found too feeble to usefully reach the preaxial border. Even as it is, the aquatic Amphibia found the fifth useless, and it accordingly disappeared."

In a paper on four new species of North American Palæozoic fishes Mr. L. Hussakof (*Bull. Amer. Mus. Nat. Hist.* vol. xxxii. pp. 245–50) remarks that one is an arthrodire of the genus *Dinomylostoma*, of which it is the second known species; the second, provisionally assigned to *Apateacanthus*, is represented by a spine with large denticules remarkable for increasing (in place of diminishing) in size towards the tip. The third and fourth belong to *Stenacanthus*.

In 1906 Dr. Ameghino described certain sharks' teeth from the Tertiaries of Patagonia as the representatives of the new generic type *Carcharoides*; the name alluding to the fact that these teeth have sharply acuminate crowns like those of *Lamna*, associated with the serrated margins of those of Carcharodon. Teeth of a similar type from the Tertiaries of Victoria are described in *The Victorian Naturalist* (vol. xxx., pp. 142–3) by Mr. F. Chapman. The discovery affords additional evidence of

the affinity between the Tertiary littoral faunas of Patagonia, New Zealand, and Australia, which appear to have inhabited different portions of a single sea-bed.

An appendix to a previous memoir on fossil sharks is communicated by Messrs. Jordan and Bell to the *Publications of the University of California, Bull. Dep. Geol.* vol. vii. pp. 243–56, in which several species are described as new.

During the last few years a number of papers have been published on fish-remains from various horizons in different parts of Africa, a list of which is given by Dr. E. Henning in an article on remains of this nature from Equatorial and South Africa issued in the *Sitzber. Ges. naturfor. Freunde,* 1913, pp. 305–18. In this communication the bearing of these remains on the physiography and former connections of the African continent is discussed at some length. Of the aforesaid papers, those published during the year under review include, in addition to the one just cited, the following: The Older Eocene Fishes of Landana, Congo, by Mr. Leriche, *Ann. Mus. Congo Belge, Geol. Pal.* ser. 3, pp. 69–80; a second communication on West African Tertiary fishes by the same author, *op. cit.* pp. 81–91; new Mesozoic vertebrate remains from the Cameruns by Dr. Henning (*K. preuss. geol. Landesanstalt,* 1913); Tertiary fish-remains from Spanish Guinea, by Dr. C. H. Eastman, *Ann. Carnegie Mus.* 1913, pp. 370–78; and, lastly, fish-remains from the Karru beds of South Africa, by Dr. Broom, published in the *Annals of the S. African Museum,* vol. xii. pp. 1–5. Space does not permit of fuller notice of these, but in the case of Dr. Broom's paper it may be mentioned that five species are described as new, four being referable to the *Palæoniscidæ* and one to the *Platysomatidæ.* The last represents a new generic type, for which the atrocious name *Carnichthys* is proposed.

An unusually fine example of the gigantic *Portheus molossus,* from the Cretaceous of Kansas, recently acquired by the Natural History Branch of the British Museum, forms the subject of a note, accompanied by a plate, contributed by Dr. Smith Woodward to the December number of the *Geological Magazine* (decade 5, vol. x. pp. 529–31).

Lastly, it may be mentioned that five new American Cretaceous pycnodonts, referable to *Microdon* (recorded for the first time in America), *Cœlodus,* and *Anomœodus,* are described by Mr. Gidley in vol. xlvi. (pp. 445–9) of the *Proc. U.S. Nat. Mus.*

THE NATURE OF THE ARGON FAMILY OF GASES

By FREDERICK SODDY, F.R.S.

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THE question of the nature of the argon family of gases has recently been discussed by Prof. Armstrong and Sir Oliver Lodge (SCIENCE PROGRESS, 1913, April, p. 648; and October, p. 197). Sir Oliver Lodge defends the accepted view that the molecules of these gases consist of single atoms against Prof. Armstrong's view that the molecules are polyatomic. Although I regard the evidence in favour of the monatomicity of the molecule in the case of these gases as beyond dispute, I think something can be said also for Prof. Armstrong's theory that these gases have an intense affinity—"an affinity so intense that it is far beyond anything we have experienced in the case of any other element." Where I should join issue with Prof. Armstrong is on the question exactly what it is for which the argon gases have intense affinity. He regards it as exercised between the constituent parts of the molecule, and in 1895, when the view was first suggested, no other result was possible than that the molecule must therefore necessarily be polyatomic, and that the atomic weight and position of the inert gases in the periodic table cannot be determined. I will try to show that, whilst modern progress seems to leave no loophole of escape from the conclusion that the argon gases have molecules composed of single atoms, and that these elements are correctly represented as occupying a new zero family in the periodic table, we can reconcile this with the essentials of Prof. Armstrong's view with a distinct gain in clearness as to the chemical character of these gases, and the meaning of the numbers of the families of the periodic table.

It is, of course, as Prof. Armstrong is well aware, even more difficult to demonstrate convincingly that the molecule of an element consists of a single atom, and is not merely an hitherto undecomposed collection of atoms, than it is to prove

convincingly that a substance is an element, and not merely an undecomposed compound. In neither case is any single proof absolutely satisfying. In both cases a review must be made of all the available evidence. I shall use the word molecule generally to indicate a single particle, whatever its complexity, whether mon-atomic or not, which is capable of existing an appreciable time, long enough to be studied, and the word atom only for the smallest particle that, as in a chemical change, has an existence so momentary that it cannot be studied alone. Thus the hydrogen atom, H, cannot yet, under any known circumstances, properly be termed a molecule, though no doubt extension of our means of high temperature research would result in its becoming experimentally known as is the case for the single iodine or bromine atoms which at high temperatures exist as molecules. But the hydrogen ion (H^+), the a-particle (He^{++}), the various positive-ray ions studied by Sir J. J. Thomson recently, are molecules, in the strict sense of the definition, and whether they are also single atoms or not, need not be prejudged by the name by which they are called. They exist, unchanged in mass, for periods long enough to enable their mass to be determined. I shall treat it as beyond dispute, that from Avogadro's Law, the molecular weight of a gas in terms of hydrogen is identical with that of its density in terms of the same unit, and shall follow the usual convention that the unit of gaseous density is $H = 1$, and the unit of molecular weight $H_2 = 2$. The molecular weight of helium is thus 4, and that of the radium emanation, from diffusion, effusion, and direct density determinations by the micro-balance, is 222 ($\pm$ say 10 per cent. at most). The molecular weight of the a-particle is experimentally found by comparing the value of the ratio of its mass to its charge, m/e, as determined by electrostatic and electromagnetic deviation, with that of the hydrogen ion, and by comparing the charge on the single a-particle (which it is possible to determine experimentally since the number of a-particles can be counted and their total charge measured) with that on the hydrogen ion. As Perrin has shown the value for the single atomic charge carried by the hydrogen ion—or what is the same thing, experimentally, the determination of Avogadro's constant, the number of molecules in a cubic centimetre of gas at N.T.P.—can be determined by at least nine independent methods, with results in agreement far closer

than is necessary for this kind of calculation, where it is the magnitude of a small whole number which is in question. The value of the ratio m/e for the a-particle and the value of its charge are both twice that of the hydrogen ion. So the molecular weight of the a-particle is 4, in terms of that of the hydrogen ion as unity. That is, the a-particle has a molecule identical with that of helium gas and not a fraction of it. In this respect it is unlike the hydrogen ion, which, as is everywhere accepted, has a molecular weight one-half of that of hydrogen gas. Sir J. J. Thomson finds that the positive ray in helium is the same as the a-particle, though he has indications of a more complex molecule (He_3) $^+$, which is a highly interesting and may be very significant observation.

From Avogadro's constant, known certainly to $\pm$ 20 per cent., the number of molecules of radium chloride in a gram is easily calculated, the molecular weight of radium chloride being known. The number of a-particles expelled from it per second in its first disintegration in which the emanation is produced has been determined by Rutherford, as is well known, by direct counting measurements. We shall get a different value for the period of average life of radium, or its reciprocal, the fraction disintegrating per unit of time, if we suppose that one molecule of radium chloride gives rise to one, two, three and so on a-particles. The period of average life found by several quite independent methods is about 2,500 years, whereas that found by assuming that one a-particle results per molecule of radium chloride disintegrating in the first change is 2,560 years. Hence one molecule of radium chloride gives in its first change one a-particle, identical in mass with one whole molecule of gaseous helium, one also in each of its four later a-ray changes, and never less than the single whole molecule of helium. Again, the experimental value for the volume of helium produced per gram of radium per year (Dewar) is 0·164 c.c., whereas the calculated value on the assumption that each a-particle is a whole molecule of helium is 0·158 c.c., calculated to the same radium standard in each case. This, in itself, constitutes a simple independent proof of the point being argued.

Now consider the radium emanation also produced. Again we find it is produced in molecules of the same mass as exist in the state of gas, one whole molecule per molecule of radium chloride disintegrating. The volume of emanation in equilibrium with

1 gram of radium, 0·6 cu. mm. at N.T.P., is almost exactly that calculated, 0·585 cu. mm., on the assumption that one whole molecule of emanation results in the disintegration of one molecule of radium chloride.

There is, therefore, something like direct proof that in the decomposition of a single molecule of radium chloride "the disintegration of a single atom of radium of mass 226," as I should prefer to say, one molecule of radium emanation of molecular weight 222 and one molecule of helium of molecular weight 4 is produced.   Must it not be admitted that this is dead against Prof. Armstrong's theory that " proto-helium," the hypothetical single constituent atom of the known polyatomic helium molecule, is the wondrous material at the root of radioactivity? The radium atom is not decomposing into proto-helium and something else, but into whole molecules of helium and emanation.   The view, therefore, that it is the intense affinity of proto-helium and the single atoms of the complex molecules of the inert gases generally, which is the cause of the peculiarities and extraordinary energy of the radioactive changes, does not, in itself, help towards the explanation of the facts.

The logic of events has manœuvred Prof. Armstrong out of his formerly impregnable position that, since the argon gases do not form compounds or enter into chemical changes, their true atomic weight must remain unknown.   For radioactive changes now give precisely the same kind of evidence as chemists rely on in the determination of atomic weights, and this evidence shows that the smallest particle taking part in a radioactive change is a whole molecule of helium or of emanation.   In deciding atomic weights the periodic law is the final court of appeal.   Can the elements be properly accommodated therein, or must the multiple of the equivalent be altered before a vacant place can be found for them?   I need only mention the periodic law generalisation, that in an α-ray change the group number of the element is reduced by two, whereas in a β-ray change the group number of the element is increased by one.   The non-valent emanation results from radium in one α-ray change, from thorium in three α-ray and two β-ray changes, from actinium in two α-ray and one β-ray change, in full accord with the accepted group numbers II, IV, and III for radium, thorium, and actinium respectively.   Moreover, if radium emanation had a polyatomic molecule, since its molecular weight is 220, its atomic weight

could not be greater than 110. Yet its products, radium B and radium C, are respectively chemically identical and non-separable from lead and bismuth respectively, which is easy enough to understand if the atomic weight is also 220 and impossible to explain if it is 110 or less. The fact that, in the final chemical court of appeal, the rare gases fit beautifully into the periodic law, as a new family of zero group number, only if their molecules are considered monatomic, and cannot be fitted in at all if their molecules are considered polyatomic, has been strikingly extended in the case of the radioactive emanations. The sequence radium, (vacant), emanation, (vacant), polonium, bismuth, lead of the last two horizontal rows is completely analogous to the other sequences—barium, cæsium, xenon, iodine, tellurium, antimony, tin; strontium, rubidium, krypton, bromine, selenium, arsenic, germanium; calcium, potassium, argon, chlorine, sulphur, phosphorus, silicon; magnesium, sodium, neon, fluorine, oxygen, nitrogen, carbon.

, Now let us turn to the physical evidence. This is not so logical in argument, perhaps, because the underlying causes are not always clear. In the first place I would put the stopping power of helium to the α-rays. Though helium is twice as dense as hydrogen, it stops the α-ray to nearly the same extent as hydrogen. The range of the α-particle of polonium in hydrogen is 15·95 and in helium 16·7 cms. at N.T.P. (T. S. Taylor, *Phil. Mag.*, 1913 (vi.) 26, 402). This would be unexpected but for Prof. Bragg's generalisation. He found the stopping power of matter to be purely an additive or colligative property, and to depend only on the numbers and kinds of atoms and not of the molecules into which the atoms are combined. The stopping power of any atom is approximately proportional to the square root of its mass, not directly to its mass as might be supposed. If helium be monatomic, then, since hydrogen is diatomic, at the same pressure the number of atoms of hydrogen in the path of the α-ray will be twice the number of helium atoms, but the stopping power of each helium atom will be approximately $\sqrt{4}$ times that of each hydrogen atom, so that the stopping power of equal thicknesses of the two gases at the same temperature and pressure will be approximately the same. Lest it should be thought that helium is peculiar, because the α-particle is a helium molecule, it may be said that argon, like helium, also obeys the square-root law, only on the assumption that its

molecule is monatomic. The generalisation affords a powerful means of checking the chemist's value of the atomic weights, and it is at least very satisfactory that no discrepancies have so far been encountered.

I should be inclined to give the specific heat evidence rather less weight than Sir Oliver Lodge does, not on account of its relative importance, but, rather, because it involves other and more important issues which cannot yet be held to be satisfactorily decided. At bottom, the first question at issue is whether, during changes of temperature, heat energy is communicated to the structure of the atom or not. Has the atom a structure, that is, that can absorb thermal energy from without and be put into vibration or caused to "squirm" with *thermal* energy ? If it has, then one never ought to get the 5/3rds ratio for the specific heats. If it has not, this value should be given by monatomic gases, provided, but only provided, that one further and rather unexpected assumption is made.

The first assumption that the atom, in addition to being the chemical unit of matter, is also the physical unit as regards the degradation or deco-ordination of the energy of motion of matter, may now be generally admitted. It certainly was not admitted for all temperatures, I remember, by Prof. Schuster in a discussion at the Manchester University Physical Society, which I had the honour to take part in some years ago ; and whether it does apply for temperatures at which the atom radiates its characteristic spectrum is a matter which still may be considered open to discussion. But for ordinary ranges of temperature a closely allied assumption is made in the kinetic theory of gases in the form that the molecules are perfectly elastic. The assumption is really a far more important one than the thesis of the monatomicity of certain molecules, and though one might employ the latter in discussing the former, the opposite process is hardly safe.

On the second point, I have sought light and leading from physical friends in vain. The 5/3rds ratio involves the further assumption that the thermal energy of rotation of a monatomic molecule is zero. The monatomic molecule, in fact, possesses one degree of freedom less than it ought to have if it were merely a molecule and nothing more. For the rotation of spherical granules under Brownian movement has been observed by Perrin, and the equipartition of energy for this

degree of freedom experimentally established. In fact, the meaning of the 5/3rds ratio of the specific heats involves a knowledge first of the monatomicity of the gas molecule before it can be used to discuss the larger questions of the absence of rotational energy and the thermal isolation of the atomic structure from its environment.

Let us now turn to the other side of the shield, and consider the progress of knowledge in other directions since Prof. Armstrong made his suggestion in 1895. The electron, as the atom of negative electricity first definitely proved to be capable of existence apart from matter by Sir J. J. Thomson, has to be reckoned with, together with the evidence that the electron is a constituent of all atoms, and is responsible for their chemical affinities and the vast majority of their physical properties. Do the argon gases contain these electrons? It might be thought that the absence of chemical affinities and the zero number of this family in the periodic grouping indicated complete absence of electrons, or, at least, of electrons capable of being detached from the atom. Whereas the fact that the α-particle has two atomic charges of positive electricity is proof that the helium atom has two electrons capable of being detached, at least during radioactive changes. It is, in this respect, analogous to an element of the alkaline-earth family, and the question arises, therefore, why compounds of the type ACl_2 and $HeCl_2$ are not possible.

Nor is it only in the more drastic changes of radioactivity that this valency is manifested. The positive ray in helium, for example, shows that the atom carries two positive charges. Helium, when it is specially pure, exhibits what may be described as a peculiar electrical inertness, in that at low pressure it conducts the discharge with great difficulty. In the purest helium all the phenomena of a high vacuum—production of cathode-rays and of X-rays—are exhibited under the discharge at a pressure, $o\cdot5$ mm. of mercury, when other gases conduct with the maximum facility. At first it appeared possible that absolutely pure helium might not conduct the discharge at all, but a careful investigation showed that this inertness is relative only. Five to ten molecules of helium are electrically equivalent to one of a common gas like hydrogen and nitrogen, so that if the discharge in pure helium is compared at any pressure with that in a common gas, not at the same pressure

but at a pressure five to ten times less, the same discharge phenomena are observed in the two cases. This explains the original observation of Sir William Ramsay and Prof. Collie that helium at *atmospheric pressure* conducts the discharge with far greater facility than other gases. (Soddy and Mackenzie, *Proc. Roy. Soc.*, 1908, **80 A**, 92.) The fact, though it has never been adequately accounted for, shows clearly that the electrical inertness of helium is relative only, and therefore one may conclude that its chemical inertness is also only relative. The molecule possesses detachable electrons, but no chemical agency has yet succeeded in detaching them.

The recognition of detachable electrons as a normal constituent of the atom alters the significance of the latter term. The term atom, as it is used by chemists, now signifies a complex of one material particle—the positive ion of the element— together with a certain number of electrons. The single un-combined material particle is the positive ion, not the atom. It would be idle to pretend, in spite of the now generally accepted dictum, that "the forces of chemical affinity and electricity are one and the same," that we have yet a complete explanation of the nature of chemical affinity in terms of the electron. But the numbers of the families in the Periodic Table from O to VII, representing the maximum positive valency of the elements, do probably represent also the maximum number of electrons in the ring systems detachable in *chemical* changes. That they do not always represent the whole number of detachable electrons in *any* change is shown by the case of the inert gases. They are of relative rather than absolute significance, and represent how one atom will behave with regard to another. If an electrically neutral atom, that is, the complex of the positive ion with its electrons, is the most stable compound of that ion which can exist, the atom will appear to be chemically inert or devoid of affinity, as in the case of the gases of the zero group. In proportion as this electrically neutral complex is unstable, so will the chemical activity of the element increase. Just as chemists suppose that the peculiar inertness of nitrogen is best accounted for on the view that the elementary nitrogen molecule, the complex, N_2, is the most stable and readily formed compound of all compounds containing nitrogen, and that the single nitrogen atoms have intense affinity for one another, so it seems reasonable to regard the helium molecule, the compound

of the helium ion with two electrons, as the most stable and readily formed of all compounds of helium. The intense affinity of helium, " an affinity far beyond anything we have experience of in the case of any other element," is; on this view, exerted not between hypothetical smaller material atoms within the helium molecule, as Prof. Armstrong originally proposed, but between the elementary material particle as a whole and its electrons. This affinity is so intense that the compound has never been decomposed by chemical agencies, and, in consequence, other compounds than the monatomic complex containing two electrons have never been obtained.

MOLECULAR VOLUME THEORIES AND THEIR RELATION TO CURRENT CONCEPTIONS OF LIQUID STRUCTURE

By GERVAISE LE BAS, B.Sc. (LOND.)

THERE can be no doubt that of all the Properties of Matter which have been correlated with the Chemical Structure of the Molecules, Molecular Volumes have been of little use to the chemist in his endeavour to determine the manner in which the atoms are arranged in these structures. Many successful attempts have been made with other physical properties, such as Optical Refractivity, the Magnetic Rotatory Power, Viscosity, and so on, but this success has not attended the study of Molecular Volumes. The reason for this seems to be that the introduction of the conception of a *co-volume* or molecular vibration volume into modern theories has exercised a retarding effect, in that it has side-tracked the subject from the main line of its historical development. It is possible to show that, even supposing such a theory of liquid structure as we have indicated should turn out to be based on fact, no possible reason exists why the original point of view of Kopp should be abandoned, for in any case, as Kopp supposed, the volumes of the molecules at the normal boiling point are approximately equal multiples of their real molecular volumes. In these circumstances, important results from the point of view of molecular structure may be expected if constitutive influences be considered.

Two types of Liquid Structure may be considered.

(a) *Space completely filled by matter.*—This condition assumes that the atoms of a molecule are in constant vibration about a mean position, which is determined by the combined influence of the chemical forces of affinity and the expanding heat forces. The atoms are regarded as those separate entities which by combination make up the molecules, but which are in actual contact at $-273°$ C. The effect of the vibratory motion

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just mentioned is to cause the atom to occupy a space considerably larger than that of its own substance, so that the volume of the molecule at any temperature above absolute zero is greater than $\Sigma A . V._{-273}$. The enlarging effect of the heat forces, combined with the attractive intermolecular forces, still preserves the compact arrangement, but in such a way as to allow of the slow diffusion of the molecules through the mass. One possibility of this unstable equilibrium is the alternate formation and dissolution of molecular aggregates. At any rate, this view accounts for the undoubted rigidity which has been found characteristic of liquids, the possibility of the formation of liquid crystals, and the enormous resistances to pressure which distinguish the liquid from the vapour states.

The passage of a substance into the solid state is easily accounted for, because the diminishing temperature causes the intermolecular forces to assume greater relative importance, owing to the gradual approximation of the molecular centres. Under such conditions a point is found where the relative movement which involves slipping becomes impossible, and there may also result an orientation of the molecules.

This compact condition of matter is involved in the theory of Barlow and Pope on the Morphotropic Relationships of Crystalline Structures, and it has been deduced by Richards as a result of his work on the Compressibilities of Solids and Liquids.

(b) As an alternative to the above, there exists the conception of *space only partly occupied by matter*. The molecules under the circumstances are not in contact, but are separated from each other by a molecular vibration space, which is maintained against the enormous internal attractions by comparatively feeble vibratory movements. This space is called the co-volume—a term borrowed from Van der Waals' theory.

(i) *Co-volume Constant at Equal Temperatures.*—Traube considers that the co-volume is the same for all non-associated substances at the same temperature (*Ueber den Raum der Atome*, Stuttgart, 1899, and *Berichte*, 1892-5).

Thus $V_m = \Sigma n V_a + \phi$, ϕ being constant at t° say. At 15°, ϕ is 25 — 26 for different non-associated compounds. This volume is a considerable fraction of the total volume, and it is difficult to make this circumstance coincide with the existence of the Law of Coincident States.

If, as Traube assumes, the constitution of liquids, and even solids, resembles in some respects that of vapours, it is logical to suppose that the co-volume would be the same for all substances under similar physical conditions. Facts are, however, against this supposition. The following diagram, which gives the volumes of the normal Paraffins at the different temperatures, shows this.

The Volumes of the n-Paraffins at the Different Temperatures
(Data due to Young)
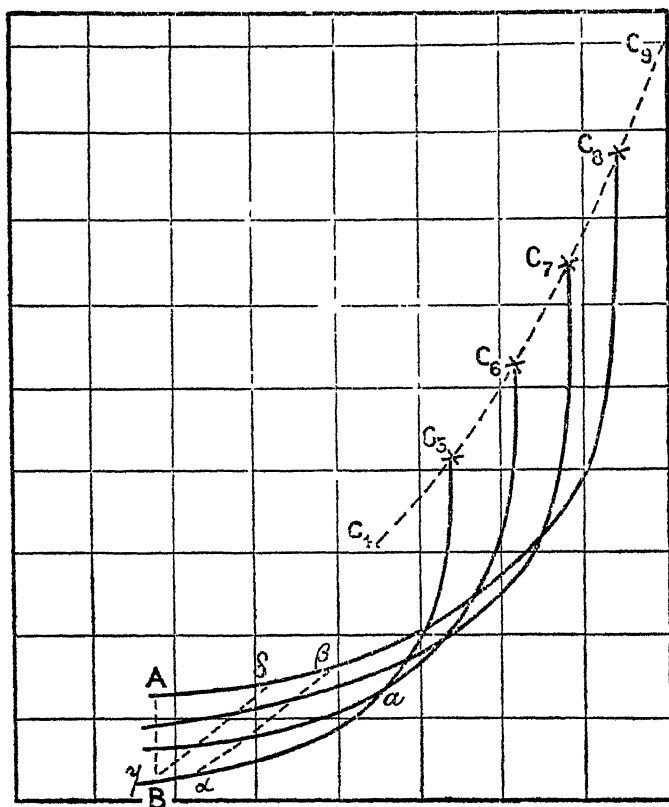
Temperature in degrees absolute.

An inspection of the curve shows that the molecular critical volumes lie on a curve represented by the dotted line $c_5c_6c_7$. The molecular volumes at the B.P. lie along another dotted line $\alpha\beta$.

The volumes at the reduced pressure $P/P_x = 0.011796$ are

represented by points along a third dotted line $\gamma\delta$. The temperatures at which the pressures are equal are known, and thus the line can be drawn.

A remarkable feature of these three dotted lines is that they are all in the same direction, a fact which indicates that the volumes represented by points of intersection of the dotted and the full lines are all similar functions of the molecular magnitudes or the complexities. Moreover, if the molecular critical volumes of the different compounds are equimultiples of the molecular volumes at absolute zero, the molecular volumes at the boiling point, and the reduced pressure $P/P_{K} = 0.011796$ are also equimultiples of the real molecular volumes:

$$\text{Thus} \quad V_{K} \quad = xV_{o} = 4V_{o} \text{ approx.}$$
$$V_{B\,P.} = yV_{o} = \tfrac{3}{2}V_{o} \quad \text{,,}$$
$$V_{P/P_{K}} = zV_{2} = 1.46V_{o} \text{ approx.}$$

x, y, and z are constants under the different conditions for the most various substances.

It is owing to this fact—one which involves the *Law of Coincident States*—that an investigation of Molecular Volumes from the point of view of Kopp is justified, independently of the fact of the existence or non-existence of a molecular vibration space or co-volume.

The Law of Coincident States is fatal to Traube's hypothesis.

(ii) *Co-volume Proportional to M.V.'s under Conditions of Equal Pressure.*—Prideaux, who also favours the conception of a co-volume or molecular vibration space (*Trans. Chem. Soc.* 1910, Nov. 577, 2032), concludes that it is proportional to the real molecular volumes under the conditions laid down by Young, at equal fractions of the critical pressures, or below the normal boiling point equal pressures. He connects the vibration volume with the existence of a vapour pressure above the surface. The co-volume and vapour pressures are zero at a certain point (not -273); and as the vapour pressure increases, the co-volume also increases. They are both functions of the temperature, and thus the increase in co-volume may be expressed as a function of the pressure.

If V_{p} and V_{o} represent the volumes of liquid at " p " and " o " pressure

$$V_{p} = V_{o}\,[1 + \phi(p)] = \text{Vol. at o press.} + \text{co-vol. at zero pressure.}$$

For another liquid

$$V'_p = V'_o[1 + \phi'(p)]$$

At a higher pressure p_1 the volumes become

$$V_o[1 + \phi(p_1)] \text{ and } V'_o[1 + \phi'(p_1)] \text{ respectively.}$$

In the case of normal liquids

$$\frac{V_o[1 + \phi(p_1)]}{V_o[1 + \phi(p)]} = \frac{V'_o[1 + \phi'(p^1)]}{V'_o[1 + \phi'(p_1)]}$$

$$i.e. \quad \frac{V'_o[1 + \phi'(p_1)]}{V_o[1 + \phi(p_1)]} = \frac{V'_o[1 + \phi'(p)]}{V_o[1 + \phi(p)]}$$

$$\therefore \phi(p) = \phi'(p) \text{ and } \phi = \phi'$$

The function ϕ may, however, change towards the critical point. He concludes that: (1) The increase in co-volume is the same function of p for all these liquids, assuming the same law of expansion holds down to the lowest and eventually to zero pressure.

$$\frac{V'_o[1 + \phi'(p)]}{V_o[1 + \phi(p)]} = \frac{V'_o}{V_o}$$

(2) The co-volumes $V'_o\phi'(p)$ and $V_o\phi(p)$ at any pressure are proportional to the actual volumes of the molecules V'_o and V_o. (3) The ratios between the volumes of the liquids at equal vapour pressures are equal to the ratios between the actual volumes of the molecules.

These conditions are all that is necessary to justify the investigation of molecular volumes from the point of view of Kopp. This is, however, no proof that the space $V\phi(p)$ is actually a molecular vibration space, and can be equally well explained on another assumption.

(i) In the first place, no account is taken of the vibration of the atoms, which is certainly a fact. The reason it does not do this is owing to a particular conception of the atom which prevails, and which tends to regard it as merely a central nucleus which is measured by the Refractivity. Prideaux, like Traube, believes that the vibration space of the atom is the external shell of dielectric, and thus the internal vibrations of the molecule are *within* the space b_o or V_o. The central nuclei are spoken of as the atoms themselves, and the shell of bound ether, which is impenetrable to other atoms, is not considered as a fundamental part thereof.

We believe that both the central nuclei and the external dielectric shells together constitute the physical atoms, and that it is this physical atom which vibrates as a whole when the temperature is raised.

It is quite possible that, as Traube shows, both the sum of these central nuclei which make up $MR\alpha$, and the sum of the external shells $b_o - MR\alpha$, are proportional to n the number of valencies :

$$\frac{MR_\alpha}{n} = \cdot 790 \text{ and } \frac{b_o - MR_\alpha}{n} = 1\cdot 75 \text{ on the average.}$$

At any rate, $MR\alpha$, b_0, V_κ, $V_{s r}$, and the molecular volumes under equally reduced pressures, show constant relations with each other in the different substances examined, and thus the additive relations which apply to one apply to all.

It consequently seems one-sided to refer to $MR\alpha$ only when for spatial relations volumetric standards are available. Moreover, as just indicated, it does not seem justifiable to consider the atom as being just that portion which happens to be impermeable to light and is conducting, and to neglect the remainder $b_o - MR\alpha$. It leads to results which are imperfect, although in themselves of utility and interest. Molecular Volume relations introduce another aspect which is of equal importance.

(ii) In the second place, the existence of the co-volume has been shown to be connected with an external vapour pressure. The vapour and solid phases can, however, exist together independently of the liquid phase, and the solid can even be transformed into the vapour without the appearance of liquid. Are we, then, to suppose that a vibration space separates the molecules in solids, which manifest the property of rigidity? This is inconceivable, and we conclude that in the solid state the molecules are in contact.

Since expansion is a well-known property of solids, we must suppose that augmentations in volume with temperature are due to the increased vibration spaces of the atoms. It cannot be supposed that this mode of expansion extends only as far as the melting point, but must persist in the liquid state.

We are thus left with the alternative of supposing that the extra-atomic space is occupied partly by vibrating atoms, which increase the molecular magnitude, and partly by a molecular

vibration space, or by the more simple one of compact structure. The latter is capable of explaining all the known properties of liquids, many of which are inconsistent with the first alternative.

Since $V_m^o = \Sigma n(V_a^o) = $ the sum of the combined A.V.'s

$$V_m^p = V_m^o[1 + \phi(p)] = \Sigma n(V_a^o)[1 + \phi(p)].$$

The space $V_m^p - \Sigma n(V_a^o)$ is thus a function of the composition and constitution of the molecules because it is proportional to $\Sigma n(V_a^o)$.

So also is the whole volume V_m^p proportional to $\Sigma n(V_a^o)$.

On the assumption of compact structures

$$\frac{V_m^p}{V_m^o} = \frac{\Sigma n(V_a^p)}{\Sigma n(V_a^o)} = [1 + \phi(p)]$$

$$\text{and } (V_a^p) = (V_a^o)[1 + \phi(p)].$$

If p *represents some fraction of the critical pressure, or when the pressure is sufficiently low some common pressure, then we suppose that the atomic volumes change under the different physical conditions in the same ratio as the whole molecular volumes. This constitutes, at least in part, a physical interpretation of the Law of Coincident States as it applies to liquids.*

Such a ratio is probably independent of the chemical composition and constitution of the substances, and depends mainly on the physical conditions of comparison.

A

This is the first of three lines of investigation which form the experimental basis of this theory, and may be called a proof of the existence of the Law of Coincident States in liquids.

Suppose that the two reference points for the volumes of a number of liquids are the equal or reduced pressure p and p_1,

Then for Substance I.

$$\frac{V_m^{p_1}}{V_m^p} = \frac{V_m^o[1 + \phi'(p_1)]}{V_m^o[1 + \phi(p)]} = \frac{[1 + \phi'(p_1)]}{[1 + \phi(p)]}$$

For Substance II.

$$\frac{V_m'^{p_1}}{V_m'^p} = \frac{V_m'^o[1 + \phi'(p_1)]}{V_m'^o[1 + \phi(p)]} = \frac{[1 + \phi'(p_1)]}{[1 + \phi(p)]} \quad \text{(Regularity I.)}$$

and so on for a number of substances.

These ratios are thus the same for all.

(*a*) Young has made the Critical Molecular Volume, or rather Density, the basis of comparison, and has shown that the

densities at equal fractions of the critical pressures are equal multiples of the critical densities.

Owing to certain irregularities which occur at or near the critical points, the author has indicated (*Phil. Mag.* S. 6, vol. xiv. No. 81, Sept. 1907, pp. 340-4; vol. xvi. No. 91, July 1908, pp. 87-9) the advantage of making the densities or volumes at some reduced pressure like $P/P_x = \cdot011795$ the basis of comparison.

Comparison of the Volumes of a number of Hydrocarbons at Corresponding Pressures.

P/P_K.	C_5H_{12}.	C_6H_{14}.	C_7H_{16}.	C_8H_{18}.	C_6H_6.	C_6H_{12}.
·011795	—	—	—	—	—	—
·044232	1·0614	1·0614	1·0617	1·0636	1·0609	1·0610
·144744	1·1594	1 1604	1·1620	1·1638	1·1593	1·1587
·58978	1·4696	1·4684	1·4695	1·4798	1·4674	1·4620
·82568	1·712	1·715	1·715	1·736	1·713	1·700
·97313	2·110	2·112	2·106	—	—	2·089
1·00000	2·731	2·740	2·754	2·780	2·741	2·715

The Law of Coincident States is thus approximately realised, as Young has shown. The divergences therefrom have been connected by him with differences in constitution.

(*b*) Prideaux, as already stated, has given some data under conditions of equal pressure (normal atmospheric), showing that the volumes or densities are comparable with similar data for a pressure of 200 mm. in the case of non-associated compounds.

$$\text{Thus} \quad \frac{V_m(760)}{V_m(200)} = \text{const.}$$

$$
\begin{array}{ll}
C_6H_6 & 1\cdot050 \\
PCl_3 & 1\cdot050 \\
HCl & 1\cdot049 \\
O_2 & 1\cdot048 \\
C_2H_4Br & 1\cdot048.
\end{array}
$$

It appears that constant relations are also obtained by summing the atomic volumes of the free elements under the two or more equal pressures and comparing them:

$$
\frac{V_m(760)}{V_m(200)} \qquad\qquad \frac{\Sigma V_a(760)}{\Sigma V_a(200)}
$$

$$
\begin{array}{ll}
PCl_3 & 1\cdot050 \\
ICl & 1\cdot041
\end{array}
\qquad
\begin{array}{ll}
P + Cl_3 & 1\cdot050 \\
I + Cl & 1\cdot043
\end{array}
$$

It follows that the relation

$$\frac{V_m}{\Sigma V_a} = \text{const.}$$

holds at various equal pressures for all normal substances. In the following cases 100 volumes become on combination:

		P 860 mm.	760 mm.	560 mm.
HgCl$_2$	Hg + 2Cl . .	101·9	101·8	101·7
HgBr$_2$	Hg + 2Br .	107·5	107·4	107·3
HgI$_2$	Hg + 2I . .	109·5	109·5	109·6

Table illustrating the Law of Coincident States among the Complex Paraffins near the M.P.

M.P. + n × 10.	C$_{14}$H$_{30}$.	Ratio.	C$_{15}$H$_{32}$.	Ratio.	C$_{16}$H$_{34}$.	Ratio.	C$_{17}$H$_{36}$.	Ratio.
M.P. . .	255·4	1·0000	273·2	1·000	291·2	1·000	309·00	1·000
„ + 30 .	262·45	1·028	280·8	1·028	299·34	1·028	317·54	1·028
„ + 60 .	270·01	1·057	288·94	1·057	307·73	1·056	326·31	1·056
„ + 80 .	275·34	1·078	294·47	1·078	313·71	1·077	332·37	1·076

(From Krafft's observations, *Ber.* 15, 1687.)

Under the above conditions the molecular volumes are equal multiples of the molecular volumes at the melting point.

(*c*) It is possible to go a step farther, and to trace the Law of Corresponding States into the solid state. The data which furnish evidence for this are due to Vincentini and Omodei (*Beibl.* 12, 178), and are from observations on certain Pb-Sn alloys. The compounds were examined in the solid state from 20° C. up to the melting point (182° C. approx.), and in the liquid state up to 356° C. As in the previous case, the conditions for comparison are met with at equal intervals of temperature above and below the respective melting points.

Comparison of the Volumes of certain Pb-Sn Alloys under Various Conditions.

Alloys.	Solid 20° C.		Solid M.P. (182° C).		Liquid 356° C.		M.P.
	V$_m$	Ratio.	V$_m$	Ratio.	V$_m$	Ratio.	
SnPb . .	34·49	1·000	35·02	1·015	36·69	1·064	181·8°
Sn$_2$Pb . .	50·67	1·000	51·36	1·013	53·76	1·061	182·3°
Sn$_3$Pb . .	66·80	1·000	67·63	1·012	70·87	1·061	182·9°
Sn$_4$Pb . .	83·06	1·000	84·10	1·012	88·13	1·061	183·3°

The ratios which have been given in the above tables are indications of the changes in the physical states simply, and are independent of the nature of the substances. They show the increases in the vibration spaces from one physical condition to another.

If the space were a molecular vibration space, there is no reason why it should be practically constant. Indeed, it would be likely to diminish with increasing complexity, because the greater molecular weight of the more complex compounds would tend to diminish the amplitudes of the vibrations and so diminish the co-volumes. The constancy of the ratio must therefore be connected with the atomic vibrations, which show similar amplitudes of vibration under similar physical conditions.

B

After tracing the Law of Coincident States in the two states—the Solid and the Liquid—we are led to a Second Regularity. This refers to the relations between the volumes of the compounds under particular physical conditions.

Since it is likely that the effects of differences in constitution would show themselves in the ratios, care must be taken to compare chemically similar substances. Suppose the conditions be equal or reduced pressure p.

Then for a number of substances which are structurally similar and which form a homologous series or one characterised by some constant difference in composition:

$$\text{At pressure } p \left. \begin{cases} V_m = V_0[1 + \phi(p)] \\ V'_m = V'_0[1 + \phi(p)] \\ V''_m = V''_0[1 + \phi(p)] \\ \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \end{cases} \right\} \text{Since } \phi = \phi' = \phi'' = \ldots$$

Comparing the volumes of the second and the third with the first we find:

$$\text{At pressure } p \begin{cases} \dfrac{V'_m}{V_m} = \dfrac{V'_0[1 + \phi(p)]}{V_0[1 + \phi(p)]} = \dfrac{V'_0}{V_0} = 1 + nx \\[2mm] \dfrac{V''_m}{V_m} \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \dfrac{V''_0}{V_0} = 1 + (n+1)x \\[2mm] \dfrac{V'''_m}{V_m} \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \dfrac{V'''_0}{V_0} = 1 + (n+2)x \end{cases}$$

$$\cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \text{(Regularity II.)}$$

x being some difference and n a number which is usually unity.

All these ratios are similar to the ratio of the volumes at absolute zero, and if the compounds differ in composition by a similar constituent, the ratios are likely to form an arithmetical series. Differences in constitution would interfere with this regularity somewhat.

Such a regularity as the above is very significant, because it indicates what are the ratios of the volumes at absolute zero, which is a condition where atomic vibration is absent. Thus a study of M.V.'s, as ordinarily understood, leads to the detection of constant relations between the combined volumes of the atoms.

The law which is involved in Relation II. may be called the *Law of Constant Volume Relations*. This relation, while true in principle, may be interfered with by Constitutive Influences to some extent.

(a) *The Law of Constant Volume Relations in the Normal Paraffins under Equally Reduced Pressures*

P/P_k	C_8H_{18}	Ratio.	C_7H_{16}	Ratio.	C_6H_{14}	Ratio.	C_5H_{12}	Ratio.	C_6H_6	Ratio.	C_6H_{12}	Ratio.
·011795	175·83	1·000	154·58	·8792	133·65	·7601	113·20	·6436	93·5	1·000	112·97	1·208
·044232	187·02	1·000	164·02	·8770	141·95	·7596	120·15	·6425	99·19	1·000	119·82	1·208
·144744	204·62	1·000	179·62	·8778	155·09	·7580	131·25	·6414	108·49	1·000	130·89	1·207
·5°978	260·3	1·000	227·17	·8728	196·25	·7539	166·35	·6386	137·20	1·000	165·17	1·204
·82568	305·2	1·000	265 25	·869	229·20	·751	193·80	·635	160·19	1·000	191·74	1·197
1·00000	488 9	1·000	427·7	·8707	366·3	·7490	309·2	·6325	256·3	1·000	306·7	1·196
Ratio of Valencies	$\frac{26}{26}$	1·000	$\frac{24}{24}$	·880	$\frac{22}{22}$	·760	$\frac{20}{20}$	·640	$\frac{18}{18}$	1·000	$\frac{18}{18}$	1·20

The ratios are seen to form an approximate arithmetic series, comparing the paraffins with C_8H_{18}, and C_6H_{12} with C_6H_6. Thus in the case of the *Molecular Critical Volumes*

$$C_8H_{18} \; 1·000 \; C_7H_{16} \; 1 - 1 \times 0·1293 \; C_6H_{14} \; 1 - 2 \times 0·1255 \; C_5H_{11} \; 1 - 3 \times 0·1225$$
$$P/P_k \; ·011795$$
$$C_8H_{18} \; 1·000 \; C_7H_{16} \; 1 - 1 \times 0·1218 \; C_6H_{14} \; 1 - 2 \times 0·1200 \; C_5H_{12} \; 1 - 3 \times 0·1188$$
$$C_6H_6 \; 1·000 \; C_6H_{12} \; 1 + 1 \times 0·1208.$$

Also, if we compare compounds belonging to the same class, we see that the volumes are nearly in the same ratio as that of the *sum of the valencies*. This relation must consequently obtain at absolute zero, and represent a constant relation between the volumes of the constituent atoms C and H.

The ratio holds more accurately the further away from the

Critical Point that observation is made. The following table emphasises this:

The Law of Constant Volume Relations among the Complex n-Paraffins near the Melting Point

M.P. + n × 10° C.	$C_{14}H_{30}$.	$C_{15}H_{32}$.	$C_{16}H_{34}$.	$C_{17}H_{36}$.
M.P.	1·000	1·070	1·140	1·210
„ + 30°	1·000	1·070	1·140	1·210
„ + 60°	1·000	1·070	1·140	1·209
„ + 80°	1·000	1·070	1·140	1·204
Ratio of Valency Numbers .	$\frac{80}{80}$ 1·000	$\frac{85}{80}$ 1·070	$\frac{92}{80}$ 1·140	$\frac{104}{86}$ 1·210

The relations between the molecular volumes near the melting point are similar to those for such compounds near the critical point.

It will be now useful to compare the volumes of normal paraffins *at* the *Normal Boiling Point*:

	C_4H_1		C_5H_{11}.		C_6H_{14}.		C_7H_{16}.		C_8H_{18}.	
	V_m.	Ratio.	V_m.	Ratio.	V_m.	Ratio.	V_m.	Ratio.	V_m.	Ratio.
Ratio of Valencies	96·0	1·000	117·8	1·227	139·93	1·458	162·56	1·694	186·26	1·940
	$\frac{26}{26}$	1·000	$\frac{32}{26}$	1·231	$\frac{38}{26}$	1·462	$\frac{44}{26}$	1·693	$\frac{50}{26}$	1·923

The rule is very closely followed at the boiling point. Divergences therefrom can be ascribed to differences in constitution; *e.g.* lengthening of the Hydrocarbon chain.

We now discuss a similar relation in the Sn-Pb alloys—a series of similar substances differing in composition by Sn.

The Law of Constant Volume Relations in the Sn-Pb Alloys near the Melting Point.

Alloys.	Solid 20°.		Solid M.P.		Liquid 356° C.	
SnPb.	V_m.	Ratios.	V_m.	Ratios.	V_m.	Ratios.
SnPb . .	34·49	1·000	35·02	1·000	36·69	1·000
Sn_2Pb .	50·67	1 + 1 × 0·469	51·36	1 + 1 × ·467	53·76	1 + 1 × ·465
Sn_3Pb .	66·80	1 + 2 × 0·469	67·63	1 + 2 × ·466	70·87	1 + 2 × ·466
Sn_4Pb .	83·06	1 + 3 × 0·469	84·10	1 + 3 × ·467	88·13	1 + 3 × ·467

The results show that the series is an additive one under the various conditions, a fact which agrees with the arrangement of the substances in series. It follows that the volumes of the atoms maintain their relative values under the different conditions This is because the changing physical conditions affect the atoms Sn and Pb similarly—that is, change of temperature, *and, what is more important, change of state.*

C

The symbolical relations previously given enable us to arrive at a third regularity, which is suggested by the above tables.

We have seen that

$$e.g. \ \frac{V'_m}{V_m} = \frac{V'_o}{V_o} = const. \ [1 + \phi(p)]$$

which suggests unchanging internal volume relations because

$$\frac{V'_m}{V_m} = \frac{V'_o}{V_o} = \frac{\Sigma n'(V_a)_o}{\Sigma n(V_a)_o}.$$

We also see that since

$$\frac{V_m}{V_o} = \frac{V'_m}{V'_o} = \ . \ . \ . \ . \ . \ [1 + \phi(p)]$$

$$\frac{\Sigma n(V_a)_p}{\Sigma n(V_a)_o} = \frac{\Sigma n'(V_a)_p}{\Sigma n'(V_a)_o} = \ . \ . \ . \ . \ . \ [1 + \phi(p)]$$

$$and \ (V_a)_p = (V_a)_o \ [1 + \phi(p)].$$

At any pressure p, we have thus to do with a number of atomic vibration volumes, which bear the same relation to each other as their volumes do at absolute zero; for if $(V'_a)_p$ and $(V_a)_p$ be two combined atoms at pressure p,

$$\frac{(V'_a)_p}{(V_a)_p} = \frac{(V'_a)_o [1 + \phi(p)]}{(V_a)_o [1 + \phi(p)]} = \frac{(V'_a)_o}{(V_a)_o}$$

For the above reason the investigation of molecular volumes from the point of view of Kopp is justified.

The Third Regularity is the *Law of Additive and Constitutive Relations* in molecular volumes. The investigation is possible, because we choose conditions such that the molecular volumes are equimultiples of the volumes at absolute zero, or the real molecular volumes.

In accordance with the above ideas, we note the unchanging relation which exists between the volumes of C and H, viz.

$C:H = 4:1$, which is at the bottom of the valency rule met with in the Hydrocarbons.

The change in structure involved in passing from Hexane, C_8H_{14}, to Benzene, C_6H_6, does not prejudice the relation mentioned.

The explanation of this on the basis of a co-volume or molecular vibration volume is not apparent.

The Law of Additivity in the Simple Normal Paraffins, etc., at Corresponding Pressures

P/P_K	C_8H_{18}. W=50.		C_7H_{16}. W=44.		C_6H_{14}. W=38.		C_5H_{12}. W=32.		C_6H_6. W=30.		C_6H_{12}. W=36.	
	V_m	$\frac{V_m}{W}$	V_m	$\frac{V_m}{W}$	V_m	$\frac{V_m}{W}$	V_m	$\frac{V_m}{W}$	V_m	$\frac{V_m}{W}$	V_m	$\frac{V_m}{W}$
·011795	175·83	3·517	154·58	3·513	133·65	3·517	113·20	3·537	93·5	3·12	112·07	3·14
·044232	187·02	3·740	164·02	3·728	141·95	3·736	120·15	3·755	99·19	3·30	119·82	3·33
·144744	204·62	4·092	179·62	4·082	155·09	4·082	131·25	4·101	108·49	3·61	130·89	3·64
·58978	260·3	5·206	227·17	5·163	196·25	5·164	166·35	5·198	137·20	4·57	165·17	4·59
·82568	305·2	6·10	265·25	6·03	229·20	6·03	193·80	6·05	160·19	5·34	191·74	5·32
1·00000	488·9	9·77	427·7	9·67	366·3	9·64	309·2	9·66	256·3	8·54	306·7	8·52

The ratios $\frac{V_m}{W}$ give the volumes of one valency or the atom of combined Hydrogen under the different conditions. The volume of combined Carbon is four times this value. Such differences as we find are due to differences in constitution.

The accurate investigation of molecular volumes is thus greatly facilitated by a recognition of the Law of Constant Volume Ratios:

The Additive Rule in the Complex n-Paraffins near the Melting Point

M.P. + n10.	$C_{14}H_{30}$. W_m = 86.		$C_{15}H_{32}$. W = 92.		$C_{16}H_{34}$. W = 98.		$C_{17}H_{36}$. W = 104.	
	V_m	$\frac{V_m}{W}$	V_m	$\frac{V_m}{W}$	V_m	$\frac{V_m}{W}$	V_m	$\frac{V_m}{W}$
M.P.	255·4	2·970	273·2	2·970	291·2	2·971	309·0	2·971
„ + 30°	262·45	3·051	280·8	3·052	299·34	3·054	317·54	3·053
„ + 60°	270·01	3·139	288·94	3·140	307·73	3·140	326·31	3·138
„ + 80°	275·34	3·201	294·47	3·200	313·71	3·200	332·37	3·200

The atomic volumes are practically constant under the circumstances.

The rule $C : H = 4 : 1$ has been traced from the Critical to the Melting Point or throughout the liquid state.

According to the Prideaux's result it should also be noticed at the Boiling Point.

C_8H_{18}		$C_7H_{16}.$		C_6H_{14}		C_5H_{11}		$C_4H_9.$	
V_m	$\frac{V}{W}$	V_m	$\frac{V_m}{W}$	V_m	$\frac{V_m}{W}$	V_m	$\frac{V_m}{W}$	V	$\frac{V_m}{W}$
186·26	3·725	162·56	3·695	139·93	3·682	117·8	3·681	96·0	3·693

We cannot doubt that at absolute zero the relative volumes of bound C and H is as $4 : 1$ in each compound and approximately for different compounds.

Similarly, it is probable that a constant relation exists between the volumes of other atoms.

It is to be expected also, that additive relations exist in the volumes of the Sn-Pb alloys in both the solid and liquid states.

The Additive Rule in the Sn-Pb Alloys near the Melting Point

Alloys.	Solid 20°.		Solid M.P. (182)°.		Liquid 356° C.	
	V_m	$\Sigma A.V.$	V_m	$\Sigma A.V.$	$V_{\bar{m}}$	$\Sigma A.V.$
SnPb .	34·49	34·49	35·02	35·02	36·69	36·69
Sn$_2$Pb .	50·67	50·66	51·36	51·37	53·76	53·78
Sn$_3$Pb .	66·80	66·84	67·63	67·72	70·87	70·88
Sn$_4$Pb .	83·06	83·02	84·10	84·08	88·13	87·98
	Sn 16·176, Pb 18·313		Sn 16·354, Pb 18·665		Sn 17·097, Pb 19·592	

In the *free state*:

A.V.$_{20}$.	.	.	. Sn 16·186	Pb 18·259
A.V.$_{182}$	.	.	. Sn 16·375	Pb 18·565
A.V. M.P. .	.	.	Sn 16·426	Pb 18·818
M.P. .	.	.	226° C.	326° C.

We note from the above table that the additive rule holds in the three conditions above mentioned—one in the liquid, and two in the solid state.

Expansion in both states takes place owing to the increase in the spheres of activity of the atoms, and there is no reason to suppose that any change in this respect occurs owing to change of state.
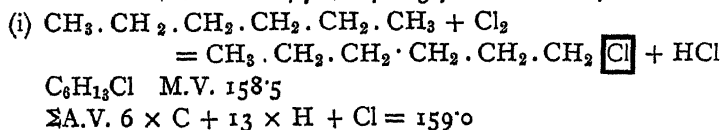
On comparing the atomic volumes in the free and combined states, we see that they are nearly the same at the same temperatures.

The Molecular Volume (and A.V.), a Function of the Chemical Constitution of the Molecules
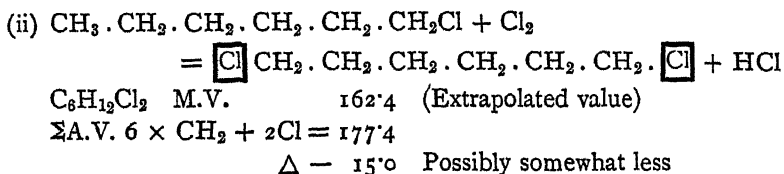
The Law of Additivity has been suggested by the foregoing Tables, and thus we notice the probability of the M.V.'s being also functions of the Constitution of the Substances. We start with a substance like Hexane, C_6H_{14}, and by a series of chemical changes illustrate the different varieties of structure.

I. *Straight Chains: The Di-substitution of Hexane.*

Hexane $CH_3.CH_2.CH_2.CH_2.CH_2.CH_3$ M.V. 139·93 observed.

ΣA.V. 6 × 14·7 + 14 × 3·7　　　 = 140·0 calculated.

(i) $CH_3.CH_2.CH_2.CH_2.CH_2.CH_3 + Cl_2$

　　　　 $= CH_3.CH_2.CH_2 \cdot CH_2.CH_2.CH_2 \boxed{Cl} + HCl$

$C_6H_{13}Cl$　M.V. 158·5

ΣA.V. 6 × C + 13 × H + Cl = 159·0

This compound has thus a straight chain, like Hexane.

(ii) $CH_3.CH_2.CH_2.CH_2.CH_2.CH_2Cl + Cl_2$

　　　　 $= \boxed{Cl}\, CH_2.CH_2.CH_2.CH_2.CH_2.CH_2. \boxed{Cl} + HCl$

$C_6H_{12}Cl_2$　M.V.　　　 162·4　(Extrapolated value)

ΣA.V. 6 × CH_2 + 2Cl = 177·4

　　　　　　　 Δ — 15·0 Possibly somewhat less

There is thus a large difference in volume between the two results. It has been shown by experiment that 1 : 1 di-substitution products are normal and involve no contraction. 1 : 2 compounds show a contraction of 3·0, and 1 : 3 a contraction of 6·0, and so on successively. (See paper read before Brit. Assoc. Portsmouth, 1911, Section B; and also *Chemical News*, vol. civ. 1911, p. 151 *et seq.*)

Such di-substituted compounds as the above, however, mark a structure different from that of straight-chain compounds. Owing to residual affinity, arising from the terminal chlorine atoms, the curvature of the Hydrocarbon chain is effected and a *Partial Ring* is formed.

$$\overset{\nearrow}{Cl}.CH_2.CH_2.CH_2.CH_2.CH_2.\overset{\nwarrow}{CH_2}Cl = \begin{array}{l} CH_2 - CH_2 - CH_2 - Cl \\ | \qquad\qquad\qquad\quad \| \\ CH_2 - CH_2 - CH_2 - Cl \end{array}$$
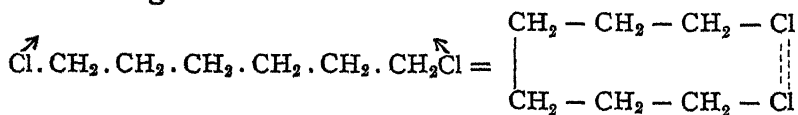
The difference, 15·0, is an expression of this change in constitution.

II. *A Closing of the Partial Ring.*

Complete Rings.

$$CH_2 - CH_2 - CH_2 - Cl + Na$$
$$|$$
$$CH_2 - CH_2 - CH_2 - Cl + Na$$

$$= \quad CH_2 - CH_2 - CH_2$$
$$|\qquad\qquad\qquad|$$
$$CH_2 - CH_2 - CH_2$$

Hexamethylene.

M.V. C_6H_{12} 116·3

6 × CH_2 = 6 × 22·2 = 133·2

$\triangle$ — 16·9
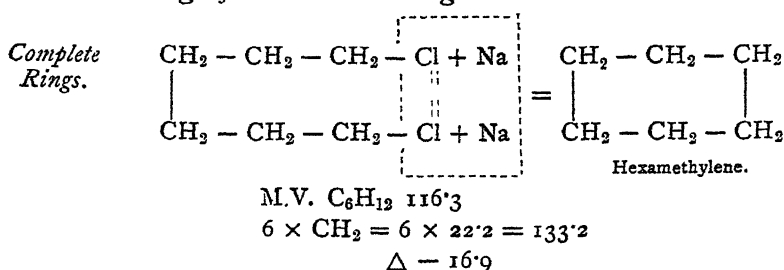
This large difference is an expression of the difference in structure between Hexamethylene and Hexane or Hexylene, C_6H_{12}.

III. *The Formation of Benzene from Hexamethylene.*

$$CH_2 - CH_2 - CH_2$$
$$|\qquad\qquad\qquad| \quad - 6H_2 =$$
$$CH_2 - CH_2 - CH_2$$

$$CH = CH - CH$$
$$|\qquad\qquad\qquad\|$$
$$CH = CH - CH$$

M.V. of Benzene C_6H_6 96·0

V. C_6H_{12} — V6H = 116·3 — 6 × 3·2 = 97·1 = C_6H_6

or Hexane C_6H_{12} 139·93
Hexylene C_6H_{12} 132·4   7·53
Dipropargyl C_6H_6 139·93 — 4 × 7·5
= 139·9 — 30·0 = 109·9

$\triangle$ = 109·9 — 96 = — 13·9 or 14·0 approx.
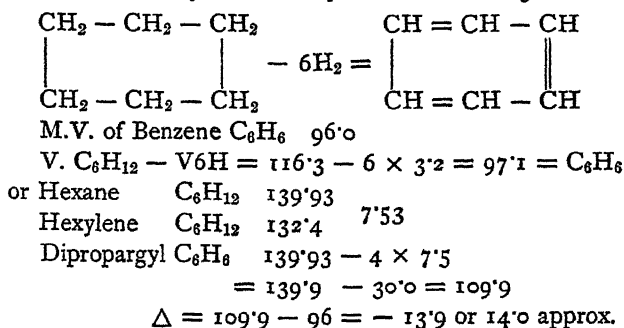
In this case again we notice a large contraction for ring structure.

IV. *Substitutions in the Benzene Ring.*

(i) Nitrobenzene $C_6H_5NO_2$

Volume Anomaly.

$$CH$$
$$/\!/ \quad \backslash$$
$$CH \qquad CH$$
$$| \qquad\quad \| \quad + HNO_3 =$$
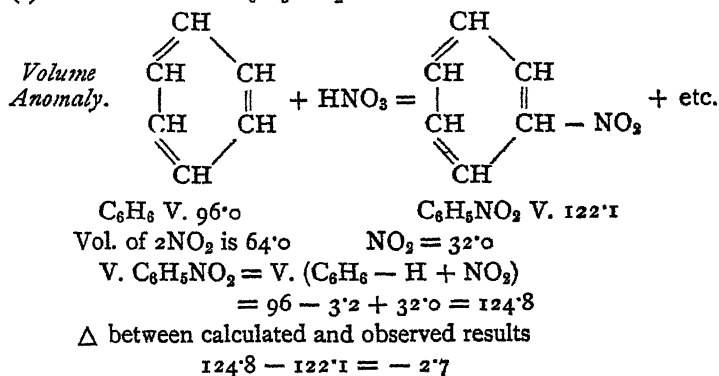$$CH \qquad CH$$
$$\backslash\!\backslash \quad /$$
$$CH$$

$$CH$$
$$/\!/ \quad \backslash$$
$$CH \qquad CH$$
$$| \qquad\quad \|$$
$$CH \qquad CH - NO_2 \quad + \text{etc.}$$
$$\backslash\!\backslash \quad /$$
$$CH$$

C_6H_6 V. 96·0 $C_6H_5NO_2$ V. 122·1

Vol. of 2NO$_2$ is 64·0   NO$_2$ = 32·0

V. $C_6H_5NO_2$ = V. (C_6H_6 — H + NO$_2$)

= 96 — 3·2 + 32·0 = 124·8

$\triangle$ between calculated and observed results

124·8 — 122·1 = — 2·7

This small difference expresses the effect of an interaction between the unsaturated — NO$_2$ group and the nucleus. This

44

has been called *the Volume Anomaly*, by analogy with the similar Optical Anomaly.   It is expressed graphically thus:

$$
\begin{array}{c}
CH \\
CH \quad CH \\
CH \quad C-NO_2 \\
CH
\end{array}
$$
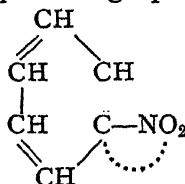
If the group be saturated, this anomaly disappears.

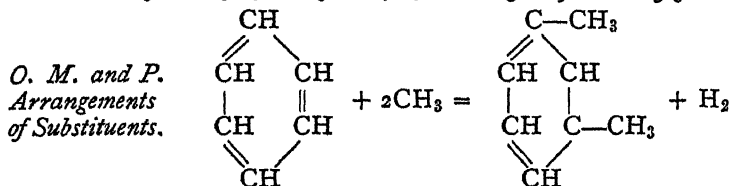(ii) Meta Xylene $1:3$ $C_6H_4(CH_3)$ M.V. 140·0.

We find that the CH_3 group is equal to 25·5.

Toluene $C_6H_5CH_3$ V. 118·3.

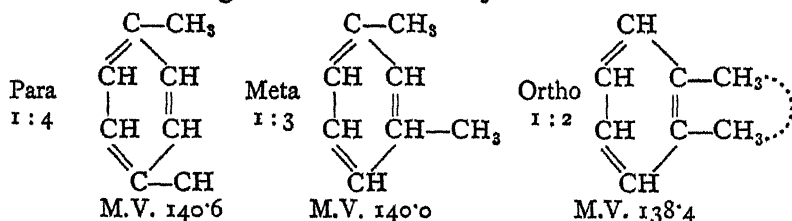$\times CH_3 . = C_6H_5 . CH_3 - C_6H_5 = 118·3 - 92·8 = 25·5$

O. M. and P.
Arrangements
of Substituents.

$$
\begin{array}{c}
CH \\
CH \quad CH \\
CH \quad CH \\
CH
\end{array}
+ 2CH_3 =
\begin{array}{c}
C-CH_3 \\
CH \quad CH \\
CH \quad C-CH_3 \\
CH
\end{array}
+ H_2
$$

Vol. C_6H_4 89·6
2CH₃   51·0

Calculated   140·6   Observed 140·0

Para Xylene is 140·6, and O Xylene 138·4.

The close approximation of the two groups CH_3 causes a contraction owing to residual affinity.

Para
1:4
$$
\begin{array}{c}
C-CH_3 \\
CH \quad CH \\
CH \quad CH \\
C-CH
\end{array}
$$
M.V. 140·6

Meta
1:3
$$
\begin{array}{c}
C-CH_3 \\
CH \quad CH \\
CH \quad CH-CH_3 \\
CH
\end{array}
$$
M.V. 140·0

Ortho
1:2
$$
\begin{array}{c}
CH \\
CH \quad C-CH_3 \\
CH \quad C-CH_3 \\
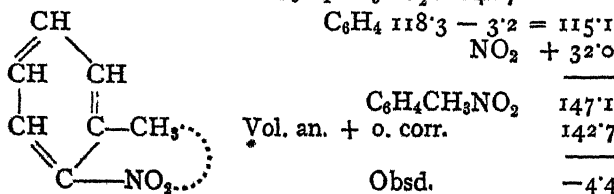CH
\end{array}
$$
M.V. 138·4

Calculated 140·6 on supposition of independence of groups.

The dotted line illustrates this effect.

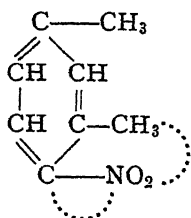Value for O Nitrotoluene $1:2$ $C_6H_4CH_3NO_2$ is 142·7

$$
\begin{array}{c}
CH \\
CH \quad CH \\
CH \quad C-CH_3 \\
C-NO_2
\end{array}
$$

C_6H_4 118·3 $- 3·2 = 115·1$
NO₂ $+ 32·0$

$C_6H_4CH_3NO_2$ 147·1
Vol. an. + o. corr.   142·7

Obsd. $-4·4$

Nitro m. Xylene $C_6H_3(CH_3)_2NO_2$ 1 : 3 : 4

O. Nitrotoluene	142·7
M. Nitrotoluene	144·3
	— 1·6
Vol. anom. — 4·4 + 1·6 = 2·8	
which is similar to that of nitro benzene.	

m. Xylene 1 : 3C₆H₄(CH₃)₂	140·0
less H	— 3·2
	136·8
NO₂	+ 32·0
	168·8
less vol. an.	— 2·8
	166·0
less o. struct.	— 1·6
Calculated Vol.	164·4
Observed	164·8

More simply

O Nitrotoluene

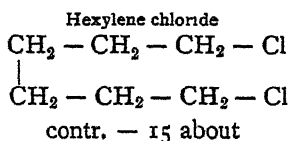|	
	142·7
less H	3·2
	139·5
+ CH₃	25·5
	165·0

The result that the constitutive features which are observed in the separate compounds are united in this compound.

For Nitro m. Xylene: (1) ring structure, (2) volume anomaly, and (3) ortho position. Each of these is indicated by a definite influence on the volume.

Besides all this, there is an additive effect due to its composition.

A similar striking series of reactions to the above may be made up from the following:

Hexane
$CH_3.CH_2.CH_2.CH_2.CH_2.CH_2.$ →
contr. o

Hexyl chloride
$CH_3 \; CH_2.CH_2.CH_2.CH_2.CH_2Cl$ →
contr. o

Hexylene chloride
$CH_2 - CH_2 - CH_2 - Cl$
|
$CH_2 - CH_2 - CH_2 - Cl$
contr. — 15 about

Hexymethylene
→ $CH_2 . CH_2 . CH_2$
| |
$CH_2 . CH_2 . CH_2$

Benzene
→ $CH = CH - CH$
| ||
$CH = CH - CH$

→ Benzoic Acid

$$CH$$
$$CH\quad CH$$
$$CH\quad C{-}COOH$$
$$CH$$

Methyl Benzoate

→

$$CH$$
$$CH\quad CH$$
$$CH\quad C{-}COOCH_3$$
$$CH$$

→ Methyl Salicylate

$$CH$$
$$CH\quad C{-}OH$$
$$CH\quad C{-}COOCH_3$$
$$CH$$

M.V.	126·9
C_6H_5	92·8
COOH	37·0
ΣV_A	129·8

M.V.	150·3
C_6H_5	92·8
COOH	37·0
CH_2	22·1
	151·9

Δ — — 2·9
Large volume anomaly owing to the reactive group — COOH.

Δ — 1·6
Anomaly has nearly disappeared. Ethyl Benzoate shows a value o for this.

Anomaly has again appeared owing to unsat. — OH group.

$$CH$$
$$CH\quad C{-}OH$$
$$CH\quad C{-}COOCH_3$$
$$CH$$

$C_8H_8O_3$

Phenol C_6H_5 — OH

C_6H_5	92·8
OH	11·1 [1]
Calculated	103·9
Observed	101·9
Δ for anom.	— 2·0

1 : 2 Cresol $C_6H_4CH_3$. OH

	121·8
1 : 4 Cresol	123·8
	— 2·0

For ortho. correction.

M.V. Methyl Salicylate	157·0
Dipropargyl	111·0
Ring	—15·0
Benzene C_6H_6	96·0
less H_2	—6·4
C_6H_4	89·6
OH	+11·1
	100·7
COOH	+37·0
	137·7
CH_2	+22·1
	159.8
less vol. anom.	— 2·0
	157·8
less o. corr.	— 2·0
Calculated	155·8
Observed	157·0

[1] o' has been taken to be 7·4, but in the aliphatic alcohols it is only 6·3. This is 1·1 less. It follows that the calculated volume of Methyl Salicylate should be on this basis = 156·9.

These examples show that the molecular volume is very sensitive to changes in constitution. Such a result does not seem possible except on the basis of compact structure.

Incidentally, these results confirm the Theory of the Atomic Structure of Matter and of the existence of Proximate Constituents, because molecular volumes show that parts of a molecule are outside the range of each other's action, while others evidently influence one another.

The Molecular Volumes (and A.V.) Functions of the Physical State of Compounds

It follows from the Theory of Compact Structure herein set forth that the Atomic Volumes are likely to reflect the Physical State of substances. The idea is that the nature of the intramolecular cohesive forces upon the particular character of which the different Physical Modifications depend are functions of the internal conditions and arrangements. Changes in the Molecular Volumes due to differences in mode of arrangement of the molecules are accompanied by changes in the atomic volumes. Thus the A.V.'s have been found to vary with temperature and pressure.

They also vary with *the Solid or Liquid* condition in which they are examined. If the assumptions which we have made be correct, then we are justified in speaking of *the Liquid and Solid Atom*, because an atom in a liquid is essentially different from an atom in the solid molecule.

This is emphasised by an examination of the abrupt changes in volume, which usually occur at the M.P. when change of state occurs, and illustrated by the volumes of the Pb-Sn alloys already referred to.

Free Atoms:

	Sb	Pb.
At M.P. (solid)	16·42	18·82
„ (liquid) . . .	16·91	19·44
Δ in liquefaction . . .	+ 0·49	+ 0·62

In Combination :

No. of atoms n.	Alloy.	M P.		+ Δ.	n × ·44.
		Solid.	Liquid.		
2	SnPb	35·02	35·40	+ 0·38	—
3	Sn₂Pb	51·36	52·65	+ 1·29	3 × 0·43
4	Sn₃Pb	67·63	69·81	+ 1·78	4 × 0·44
5	Sn₄Pb	84·10	86·34	+ 2·24	5 × 0·45
13	Sn₁₂Pb	216·8	221·9	+ 5·10	13 × 0·39

The Table shows conclusively that the augmentations in volume on liquefaction depend on the number of atoms. The expansion is the same for a Pb as for a Sn atom, for Sn = Pb = + 0·44 ; but in the free state they are different, for Sn = + 0·49 and Pb = + 0·62. A modification of this space augmentation per atom has occurred, in a similar manner to the variation or modification of the M.P. on combination. That there has been an alteration in the volume of each atom individually, and not an indirect variation due to the change in the amount of play space of the whole molecule, seems very probable.

A study of the amounts of heat absorbed by 100 grammes of the substance as the temperature has been raised from 100° to 360° has been shown by Spring (*Bull. Acad. Belg.* [3], 11, 355, 1886) to be greater than that absorbed by an amount of matter in the free state equivalent to the sum of the constituents. The excess varies with the composition. The results may be explained by assuming that the metals form unstable compounds, which, on being heated, break down into their constituents. The result of Magies' work on " Specific Heats " (*Bull. of Amer. Phys. Soc.*, April 27, 1901) greatly favours our view of the question, for even in solution salt molecules and ions are able to exert an attracting influence on molecules of the solvent for some considerable distance, so that possibly complex aggregations of molecules about the ions result.

If this be so, the Additive rule already noticed is independent of whether the substance is solid or liquid, or whether the atoms are associated or dissociated.

Modifications in Volume associated with Physical Changes in Compounds

It is extremely probable that many differences occur in the nature of alloys which are indicated by their thermal and

electrical properties. So too do molecular volumes show conclusively that changes have taken place. Several writers show this to be the case by summing up the A.V.'s in the free state and finding out the nature of the differences, M.V. $- \Sigma V_a$. This certainly indicates that changes have taken place, but we are left in the dark regarding the nature of such changes. This, no doubt, is because these writers are unwilling to make the assumption that

$$M.V. = \Sigma(A.V.)$$

Thus E. Vanstone, in a paper entitled "A Physico-chemical Study of the Mercury-sodium Alloys or Amalgams," and read before the Faraday Society, March 14, 1911, gives data which show the existence of the compounds Na_3Hg, Na_3Hg_2, $NaHg$, Na_7Hg_8, $NaHg_2$, and $NaHg_4$ as crystalline solids. Volumetric evidence is here given that they are divisible into two classes. The first four are additive as regards volume, and made up from the combined volumes of $Na = 21\cdot64$ and $Hg = 9\cdot04$. The free values are $Na = 23\cdot786$ and $Hg = 14\cdot76$. Na thus contributes $23\cdot786 - 21\cdot64 = -2\cdot146$ to the contraction, and Hg $14\cdot76 - 9\cdot04 = -5\cdot72$, per gramme atom.

The remaining compounds, $NaHg_2$ and $NaHg_4$, are different. The columns marked * are additional to those given by the author of the paper (E. Vanstone), and are based upon the principles set forth in the present one :

M.P. compound.	M.V.	$\Sigma(A.V_1)$*	$\Sigma A.V_2$ (free).	M.V $- \Sigma A$ V.	Calc.*
Na_3Hg . . .	73·97	73·96	86·12	− 12·14	− 12·14
Na_3Hg_2 . . .	83·01	83 00	100·87	− 17·86	− 17·86
$NaHg$ . . .	30·99	30·68	38·54	− 7·54	− 7 86
Na_7Hg_8 . . .	221·88	223·80	284·57	− 62·69	− 60·74

The observed and calculated values agree so well, that it cannot be doubted that the explanation given of the reason for the changes in volume is the correct one.

The combined volume of Na is very similar to that in the free state, but the free and combined volumes of mercury are very different.

In the next group the state of things is very different :

M.P. [$NaHg_2$] $43\cdot14 = 3 \times 14\cdot38$
M.P. [$NaHg_4$] = [$NaHg$] . $3Hg = 30\cdot99 + 3 \times 14\cdot38 = 74\cdot13$
Observed $74\cdot07$

In the first case Hg seems to impose its value on Na, and in the second there seems to be association of NaHg in the former with three atoms of mercury.

The M.P.'s show that $NaHg_2$ is quite different from all the others, because its M.P. is considerably higher than those of NaHg and $NaHg_4$, whereas it should be intermediate.

It is evident from this that considerable modifications of the free atomic volumes can occur, even in such loose combinations as alloys, and that in spite of such changes the Additive Principle holds.

(ii) *The Chlor-Brom-Iodides of Silver*, which have been studied by Rodwell (*Phil. Trans.* 1882, 1140), present some remarkable examples of the modification of volumes in conformity with a simple numerical relationship.

We give first of all the following, which illustrate the principle of addivity without great modification of the free values (Rodwell, *Phil. Trans.* 1882, 1160).

Compound.	V_m.	$\Sigma A.V.$
2CuI . AgI	107·1	107·1
2CuI . 2AgI	148·4	148·4
2CuI . 3AgI	189·6	189·7
2CuI . 4AgI	231·1	231·0
2CuI . 12AgI . . .	561·8	561·4

The combined volumes of CuI and AgI are respectively 32·9 and 41·3.

Free Volumes:

CuI 33·3   AgI 41·5

The volumes of both are diminished slightly in the above combinations, but the volumes are perfectly additive.

As before stated, great modifications can occur, either so as to conform with some simple spatial relation, or even, as in the case of the amalgams previously studied, in what seems an arbitrary manner.

They may be studied at 0° and at their respective M.P.'s.

The data for the free simple salts are:

Compounds.	V_0	V_{mp}	M.P.	
AgCl	26·07	29·17	451	Rodwell, *P.T.*
AgBr	30·14	33·60	427	1182 1125
AgI	41·52	44·59	527	

If we tabulate the Chlor-Brom-Iodides we find :

Compounds.	V_o	$\Sigma A.V.$	V_{mp}	M.P.
AgI . 2AgBr . 2AgCl .	147·6	153·94	162·9	383
AgI . AgBr . AgI . .	92·56	97·73	101·8	331
2AgI . AgBr . AgI . .	123·25	139·25	140·7	326
3AgI . AgBr . AgI . .	173·6	180·77	183·5	354
4AgI . AgBr . AgI . .	215·2	222·29	223·9	380

The volumes at 0° show considerable changes, as judged by the differences $V_o - \Sigma A.V.$, but these cannot be made out at this point. Probably there is considerable heterogeneity in the composition of the substances.

At the M.P.'s the results are, however, surprisingly regular.

Rodwell's Chlor-Brom-Iodides at the M.P.

Substance.	M.P.	V_{mp}	n	n × 20·36.
1 AgI . 2AgBr . 2AgCl . .	383	162·9	8	8 × 20·36 = 162·9
2 AgI . AgBr . AgCl . .	331	101·8	5	5 × 20·36 = 101·8
3 2AgI . AgBr . AgCl . .	326	140·7	7	7 × 20·36 = 142·5
4 3AgI . AgBr . AgCl . .	354	183·5	9	9 × 20·36 = 183·2
5 4AgI . AgBr . AgCl . .	380	223·9	11	11 × 20·36 = 223·9

$$\Delta\ (1) - (2) = AgBr + AgCl\ \ 61·1 - 3 \times 20·36 = \ \ 61·1$$
$$\Delta\ (4) - (2) = 3AgI\ \ \ \ \ \ \ \ \ \ \ 122·1 - 6 \times 20·36 = 121·1$$

Thus Ag I = 2 × 20·36 Ag Br = 2 × 20·36 and AgCl = 1 × 20·36.

These three Halogen Compounds of Silver in the above complex compounds possess volumes which stand to each other in the simple relationship of

$$Ag\ I : AgBr : AgCl = 2 : 2 : 1.$$

This occurs at the M.P. We note that

Vol. of AgI at

0° d ·5673 is 41·42

max. dens. 163° d ·5771 is 40·72 = 2 × 20·36

mol. den. 527 d ·5522 is 42·56

This compound diminishes in volume from 0° to 163°, and then increases to the M.P.

It is thus remarkable that, in combination at the M.P., the volume of AgI is the same as its volume at the maximum density when free. Not only is this the case, but AgI imposes

its volume on AgBr and AgCl. The considerable differences met with at 0° are thus easily understood.

The half of 20·36 (10·18) is very similar to the volume of Ag in the solid state, so that we must attribute to Cl, Br, or I this volume or one twice as great. Since Ag and the Halogens are typically monovalent, we have perhaps here an example of the operation of Barlow and Pope's Valency Law.

The Effect of Physical Modification in Solid State on the A.V.

In the solid state we find that differences in physical modification affect the atomic volumes. This is well illustrated by a number of Complex Felspars, studied by Day and Allen of the U.S. Geological Survey (*The Isomorphism and Thermal Properties of Felspars*, Part I.).

Volumes of Certain Complex Felspars

Felspar Anorthite An.	M.P.	M V. (cryst.)	ΣV.	M.V. (glass).	ΣV.
{Al,Ca(SiO₄)₂} $\{Al,Ca(SiO_4)_2\}$	1532	100·76	—	103·19	—
Ab₁An₅	1500	605·9	605·3	626·1	626·4
Ab₁An₂	1463	302·6	302·5	316·5	316·7
Ab₃An₁	1419	201·9	201·7	213·8	213·5
Ab₃An₁	1367	302·4	302·4	323·9	323·9
Ab₃An₁	1340	402·9	403·7	434·2	434·3
Albite Ab	—	100·92	—	110·37	—
(AlNaSi₃O₈) $(AlNaSi_3O_8)$					

In the case of both crystalline and glassy varieties the additive law strictly holds—the volumes of the Simple Felspars being preserved in the Complex Felspars. In the glassy state we may suppose that amorphous structure is the true one, and in the crystalline varieties we have orientation of the molecules. This arrangement of the molecules, although it involves a certain amount of compression as compared with those which show want of arrangement, yet does not prejudice the principle of additivity. If molecular interspaces existed this would not be so. We must suppose that in the amorphous condition the structure is compact in the sense already stated, because the vibrating atoms, held together by the forces of affinity, fully occupy the space. When arrangement of the molecules takes place, owing to the action of intermolecular forces, it may well be that the molecules are in the most favourable position for

the action of these forces, so that there results a certain compression. This in the first instance affects the Simple Felspathic constituents, but ultimately the constituent atoms also.

It follows that the individual atoms have different volumes in the two modifications, owing to the difference in the constraint imposed by the attracting forces.

We can speak of a *crystalline atom* and a *glassy atom*, for they are different. Crystalline and glassy modifications of substances have their origin in the peculiarities of intermolecular forces. These are in reality residual affinities due to the atomic constituents of the molecules and their special arrangements. *We are, consequently, led to look for the ultimate cause of physical modifications in the nature and arrangement of the atoms.*

In conclusion we see that a study of the Molecular Volumes of Substances gives us, as it were, an external view of the structures. From the peculiarities noticed, by a process of analysis of the data for known substances, we proceed inductively to arrive at conclusions concerning the internal conditions and the modes of arrangement of the atoms in the molecules. By the opposite—or deductive—method R. Kleeman (*Phil. Mag.* vi. 19, 840-46) has recently studied the question. This author concludes that the range of action of the molecular forces is equal to the distance between the molecular centres. The molecules are thus strongly attracted. In opposition to these, the kinetic heat-forces act. By supposing that the intervening space is occupied by matter, then owing to the extensive motion of the vibrating atoms the resistances to compression which Richards has studied would be represented by the mechanical resistance occasioned by this motion. The molecular kinetic forces are so far reduced as compared with vapours that only slow diffusion is possible.

ORGANIC DERIVATIVES OF METALS AND METALLOIDS

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TAKEN in its widest sense, the title of this paper [1] embraces a very large and miscellaneous series of substances divisible into several distinct classes. For since carbon is the essential element of all organic compounds, there should fall within the category of organic derivatives of metals and metalloids all those combinations which contain carbon in direct association with these elements. The scope of the present paper is, however, restricted to a consideration of the compounds containing not merely carbon but the carbon of hydrocarbon radicals. This restriction at once excludes two very important classes of substances which would otherwise deserve special reference. The first of these classes is that of the metallic carbides, an outcome of Moissan's famous researches on the electric furnace, of which calcium carbide is the best known example. The other class includes the metallic carbonyl derivatives which were discovered by Mond, who made use of the remarkable properties of nickel carbonyl in the technical production of pure nickel.

The substances discussed in the sequel contain a metal or metalloid combined with one or more hydrocarbon radicals, and for the purpose of this paper the hydrocarbons themselves, compounds consisting entirely of carbon and hydrogen, may be illustrated by the following two types: the paraffins with methane CH_4, ethane C_2H_6, and propane C_3H_8, as simplest members, and the aromatic hydrocarbons represented by benzene C_6H_6.

The paraffinoid or alkyl radicals are methyl CH_3, ethyl C_2H_5, propyl C_3H_5, and generally C_nH_{2n+1}, obtained by removing one hydrogen from the paraffin hydrocarbon itself. These radicals

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[1] This paper formed the subject of the opening address to the Dublin University Experimental Science Association delivered on November 11, 1913.

do not exist in the free state, but can pass from one compound to another in chemical interchanges.

The typical benzenoid or aryl radical is phenyl C_6H_5, obtained by the abstraction of one hydrogen from the aromatic hydrocarbon, benzene. This radical, again, is only known in combination.

Perhaps in passing I should attempt to define the inorganic portion of my title. The metals are those elements which can function as simple cations in electrolysis and which do not furnish volatile hydrides, *i.e.* compounds with hydrogen. The metalloids are a small group of elements having certain metallic characteristics and, in addition, the property of yielding vaporisable hydrides like the non-metals. Arsenic, antimony, and tellurium may be regarded as metalloids, and possibly also boron and selenium, although the last two are much more closely allied to the non-metals than to the metals.

In many instances chemical research has progressed along utilitarian lines. The employment in medicine of various plant extracts has encouraged investigations on alkaloids and other active products of vegetable life. The art of dyeing has led to the study of natural and artificial colouring matters, lakes, and mordants. But the activities of pioneers have never been restricted by purely utilitarian considerations, and if science is to continue its healthy and beneficial growth it is to be hoped that these activities will always be afforded the widest scope. Many laboratory investigations, at first apparently quite devoid of any practical utility, have led to results of fundamental importance from both the theoretic and practical standpoints. The pioneering experiments of Cavendish on the fixation of atmospheric nitrogen is a classical example, and I hope to show that the early study of organic derivatives of metals and metalloids is another case in point. Of no branch of human activity can it be predicted with greater certainty than of chemistry, " Cast thy bread upon the waters and thou shalt find it after many days."

1. Early Researches

So singular are the properties of the first discovered organometallic compounds that in taking up their study chemists appeared to be turning their backs on the realities of ordinary terrestrial phenomena. It would tax the genius of a Jules Verne
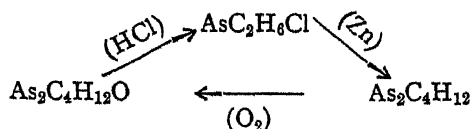
or a Wells to conceive a world in which these substances might form the materials of everyday life. Many of them are intensely poisonous, others are decomposed by traces of moisture, and others again are spontaneously inflammable or even explosive in air.

Cacodyl Derivatives

The first worker in this field was Bunsen, who during the period 1837—1843 undertook the study of organic derivatives of arsenic. It had long been known that by distilling a mixture of white arsenic (arsenious oxide) and potassium acetate a fuming oily liquid was obtained having very poisonous properties and a most disagreeable odour. This uninviting product, known as Cadet's liquid, was examined systematically by Bunsen, who showed that the pungent constituents of the mixture were two substances containing arsenic.

The main constituent contained the metalloid associated with carbon, hydrogen, and oxygen; the compound present in smaller amount consisted of the three elements, arsenic, carbon, and hydrogen. Both compounds were extremely poisonous.

Bunsen's analyses showed that the oxygenated compound had a composition indicated by the formula $As_2C_4H_{12}O$. The non-oxygenated compound had the empirical formula AsC_2H_6, but the vapour density gave its molecular formula as $As_2C_4H_{12}$. The former of these substances when distilled with hydrochloric acid yielded a volatile oil with the molecular formula AsC_2H_6Cl, and this compound, when heated with zinc in an inert atmosphere, lost its chlorine and became converted into the compound $As_2C_4H_{12}$.

$$As_2C_4H_{12}O \xrightarrow{(HCl)} AsC_2H_6Cl \xrightarrow{(Zn)} As_2C_4H_{12}$$
$$As_2C_4H_{12}O \xleftarrow{\hspace{2cm}} As_2C_4H_{12}$$
$$(O_2)$$

On examining the foregoing formulæ it will be seen that there is a group $[AsC_2H_6]$ common to all. Such a group is now called a compound radical, and this particular group was among the first compound radicals to be definitely recognised.

At first the group was called by Bunsen *alkarsin*, but later, at the suggestion of Berzelius, the name of *cacodyl* was adopted. This discovery of a compound metal afforded at the time a striking confirmation of the radical theory according to which organic substances are composed of these groups or compound

radicals combined with elementary radicals. Berzelius wrote of Bunsen's work, " The research is a foundation stone of the theory of compound radicals of which cacodyl is the only one the properties of which in every particular correspond with those of the simple radicals."

The analogy between the compound metal, cacodyl, and two of the metallic elements, sodium and thallium, may be illustrated as follows :

Cacodyl	Cacodyl oxide.	Cacodyl chloride
$As_2C_4H_{12} = [AsC_2H_6]_2$	$(AsC_2H_6)_2O$	AsC_2H_6Cl
Kd_2	Kd_2O	$KdCl$

Metal	Metallic oxide	Metallic chloride
$2Na$.	Na_2O	$NaCl$
Tl_2	Tl_2O	$TlCl$.

From the large series described by Bunsen, two other cacodyl derivatives may be selected for special mention. On distilling cacodyl oxide with mercuric cyanide a well-defined crystalline substance, *cacodyl cyanide*, was obtained. This product is of interest, as consisting of a combination of two of the first compound radicals (cacodyl and cyanogen, CN) to be definitely recognised.
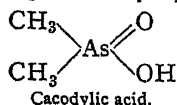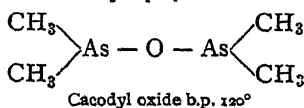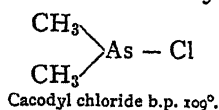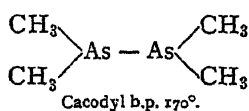
$$Kd_2O + Hg(CN)_2 = HgO + 2Kd.CN.$$
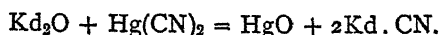
Cacodyl cyanide is a terribly poisonous substance, a few grains left to evaporate in a large room speedily attack the occupants, producing tingling and numbness of hands and feet, giddiness and finally unconsciousness. In addition to this disagreeable property, the vapour of the compound is explosive, and in attempting to determine the vapour density Bunsen lost the sight of one eye. Nevertheless, he persisted in the investigation and left on record a complete description of this deadly substance.

Cacodyl itself is spontaneously inflammable in air, but if allowed only a moderate amount of free oxygen, or preferably if oxidised with moist mercuric oxide, it changes successively into cacodyl oxide Kd_2O and then to an extremely soluble compound $KdO.OH$ which, having acidic properties, is termed *cacodylic acid*. When compared with the cacodyl derivatives already mentioned, it may seem extraordinary that this oxidised compound, although containing 54 per cent. of soluble arsenic, is nevertheless non-poisonous. Bunsen first observed this difference in 1843, and his observation remained fallow until seventy

years afterwards, when in 1913 Ehrlich unfolded to the Seventeenth International Congress of Medicine assembled in London his wonderful story of the therapeutic application of the organo-arsenic compound "salvarsan" or "606."

Cacodylic acid, in the form of its sodium salt, has been suggested for medicinal use, but at present it is largely superseded by arsenical preparations based on atoxyl.
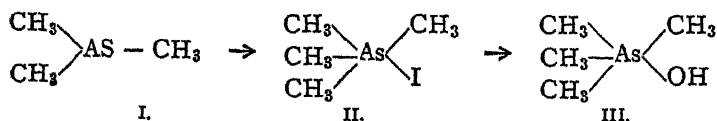
It should be pointed out that at the time of Bunsen's researches the hydrocarbon radicals themselves had not been recognised. Subsequent researches by Kolbe, Frankland, Cahours, v. Baeyer and others elucidated the inner constitution of cacodyl, and it is now known that this compound radical consists of tervalent arsenic associated with two methyl radicals:

$$\begin{array}{cc} CH_3\diagdown \\ \quad\quad >As - As< \quad \diagup CH_3 \\ CH_3\diagup \quad\quad\quad \diagdown CH_3 \end{array} \qquad \begin{array}{c} CH_3\diagdown \\ \quad\quad >As - Cl \\ CH_3\diagup \end{array}$$

Cacodyl b.p. 170°.   Cacodyl chloride b.p. 109°.

$$\begin{array}{cc} CH_3\diagdown \\ \quad\quad >As - O - As< \quad\diagup CH_3 \\ CH_3\diagup \quad\quad\quad\quad\quad \diagdown CH_3 \end{array} \qquad \begin{array}{c} CH_3\diagdown \quad\quad\diagup O \\ \quad\quad >As< \\ CH_3\diagup \quad\quad \diagdown OH \end{array}$$

Cacodyl oxide b.p. 120°   Cacodylic acid.

In all these compounds but the last the arsenic is tervalent; in cacodylic acid it is quinquevalent.

Cahours found that an alloy of arsenic and sodium when heated with methyl iodide yielded cacodyl and another arsenical compound, *trimethylarsine* (I) containing arsenic associated with three methyl groups. This substance gives rise to a large number of derivatives, of which I shall only mention two.

The direct addition of methyl iodide yields a salt-like compound, *tetramethylarsonium* iodide (II.), and from this product by the action of moist silver oxide one obtains the base *tetramethylarsonium hydroxide* (III.), which is a strong caustic alkali having properties resembling those of potassium hydroxide:

$$\begin{array}{c} CH_3\diagdown \\ \quad\quad >AS - CH_3 \\ CH_3\diagup \end{array} \rightarrow \begin{array}{c} CH_3\diagdown\quad\quad\diagup CH_3 \\ CH_3->As< \\ CH_3\diagup\quad\quad\diagdown I \end{array} \rightarrow \begin{array}{c} CH_3\diagdown\quad\quad\diagup CH_3 \\ CH_3->As< \\ CH_3\diagup\quad\quad\diagdown OH \end{array}$$

I.   II.   III.

### *Zinc and Mercury Alkyls*

A few years after Bunsen's investigations E. Frankland found that on heating the alkyl iodides with zinc, especially when the metal is rendered more active by the addition of a small amount

of sodium, the iodine and alkyl radicals became separately attached to zinc :

$$2C_2H_5I + 2Zn = ZnI_2 + Zn(C_2H_5)_2.$$
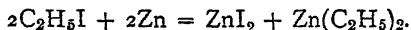
Zinc ethyl is a liquid boiling at $118°$ and solidifying at $-28°$; it is spontaneously inflammable in air, and is decomposed violently by water. It is a remarkably energetic substance and a most valuable reagent in research, as it reacts with a great variety of inorganic or organic materials.

The organo-mercury derivatives next discovered were prepared either through the agency of zinc alkyls or directly by the action of alkyl iodides on sodium amalgam :

$$Hg(2Na) + 2C_2H_5I = 2NaI + Hg(C_2H_5)_2.$$
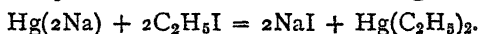
With zinc ethyl and mercuric chloride an intermediate compound is formed :

$$2HgCl_2 + Zn(C_2H_5)_2 = ZnCl_2 + 2Hg(C_2H_5)Cl.$$
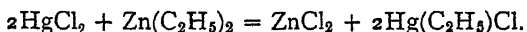
This mercury ethyl chloride gives rise to an iodide in which moist silver oxide replaces iodine by hydroxyl. The product, mercury ethyl hydroxide, is a strongly caustic base like potassium hydroxide ; it liberates ammonia from ammonium salts, and precipitates alumina and other metallic oxides from their soluble salts.

By the agency of the zinc and mercury alkyls it has been found possible in many instances to combine the alkyl radicals with other metals, but the reaction is by no means general, and some metals have not as yet yielded organo-metallic derivatives. It might therefore be profitable at this stage to consider in which cases favourable results are obtained.

2. The Position of Elements in the Periodic Scheme in Relation to their Capacities for Forming Organic Derivatives

The periodic classification due to Newlands and Mendeleef is now too well known to need detailed description. It is based on the principle that the physical and chemical properties of the elements are periodic functions of their atomic weights. Starting with the elements of least atomic weight, excluding hydrogen, it is found that during two periods recurrence occurs at the ninth element, subsequently the periodicity becomes doubled, but nevertheless all the elements can be arranged in eight vertical series, each of these vertical series being divisible

45

into two natural families, the successive members of which occur alternately.

The most rational way of representing the arrangement is not on a sheet, but on a cylinder or octagonal prism. A spiral or solid helix is traced down the cylinder or prism, each turn of the screw corresponding with the addition of eight or ten elements arranged in their appropriate vertical columns.

The doubling of the periodicity indicated by Mendeleef leads to the arrangement of two natural families in each vertical series. Let us examine a pair of these related families as regards their capacities for yielding organic derivatives.

(a) The Silicon Family

It will be most convenient to start with the fourth vertical series. Here we find, as the initial member, carbon itself, the essential element of organic compounds. Following this element is silicon, which exhibits certain points of resemblance, but also many points of difference.

Silicon is followed successively by titanium and germanium, and the question arises which of these is to be placed in the same family as silicon.

Germanium and tin, the next metal of the series, resemble silicon in forming feebly acidic hydroxides existing in colloidal forms; their oxides have the same general formula RO_2 as silica, and they yield volatile chlorides RCl_4 decomposable by water.

Titanium and zirconium also resemble silicon in many of their naturally occurring compounds; they likewise form feebly acidic gelatinous hydroxides and yield volatile chlorides and bromides decomposed by water.

Silicon evidently has affinities with both series, but the best criterion of relationship is the capacity for forming organic derivatives. This non-metal readily yields organic derivatives, and a large number of these compounds have been described by Friedel, Ladenburg, Emerson Reynolds, and more recently by Kipping.

Titanium and zirconium have, however, evaded all attempts to combine them with hydrocarbon radicals; they are found directly associated with carbon in their carbides, but these compounds are excluded from the present consideration.

Turning to the germanium-tin series, we find that all the elements of this group yield organic derivatives, and this pro-

perty forms the distinctive family trait for this group of elements, in which we may, by straining a point, include carbon, since this element is known to possess in the highest degree the property of combining with hydrocarbon radicals. Hence the fourth vertical series of the periodic scheme may be divided into two families, the former of which yields organic derivatives but the latter does not:

| Silicon family	. C	Si	Ge	Sn	Pb
Titanium family	.	Ti	Zr	Ce	Th

It will be sufficient to consider the following two series of alkyl derivatives to see the family likeness of the five members of the silicon family:

C	Si	Ge	Sn	Pb
$C(CH_3)_4$	$Si(CH_3)_4$	—	$Sn(CH_3)_4$	$Pb(CH_3)_4$
b p $+95°$	$30-31°$	—	$78°$	$110°$
—	$Si(C_2H_5)_4$	$Ge(C_2H_5)_4$	$Sn(C_2H_5)_4$	$Pb(C_2H_5)_4$
b p	$153°$	$160°$	$181°$	200

The first member of the series, $C(CH_3)_4$, regarded from our present standpoint as an organic derivative of carbon, is a hydrocarbon of the paraffin series, but it does not occur in mineral oils. Like its homologues, the organo-metallic compounds of tin and lead, it is produced through the agency of zinc methyl, the other reagent in this instance being the dichloride of the well-known solvent acetone, $(CH_3)_2CO$:

$$(CH_3)_2 CCl_2 + Zn(CH_3)_2 = ZnCl_2 + C(CH_3)_4.$$
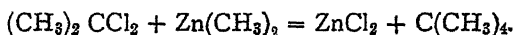
Similarly silicon tetramethyl and tetrethyl are produced by the interaction of zinc alkyls and silicon tetrachloride. Tin tetramethyl is obtained from methyl iodide and an alloy of tin and sodium. Lead tetramethyl and tetrethyl are prepared by the general process from the zinc alkyls and lead chloride. It is interesting to note the exaltation in the valency of lead produced in these condensations:

$$\overset{\text{II.}}{2PbCl_2} + 2Zn(CH_3)_2 = Pb + 2ZnCl_2 + \overset{\text{IV.}}{Pb(CH_3)_4}.$$
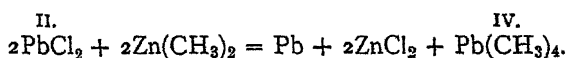
Silicon, tin, and lead yield also organic derivatives containing aryl radicals.

By surrounding silicon or tin with four dissimilar radicals, three being organic groups, it has been demonstrated that the product is a racemic combination containing two optically active components related to one another as object and image.

The case of silicon was worked out by Kipping on the sulphonic acid of benzylethylpropylsilicyl oxide :

$$\left[SO_3H . C_6H_4 . CH_2 . \overset{C_2H_5}{\underset{C_3H_7}{Si}} . \right]_2 O$$
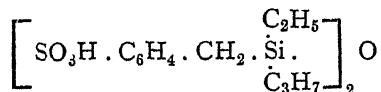
by crystallising the salts of this acid with optically active *d*- and *l*-methylhydrindamines. Pope and Peachy demonstrated the case of tin with the compound methylethyl-*n*-propyl-stannic iodide, the dextrorotatory component being isolated through the agency of *d*-camphorsulphonic acid.

This resolution of silicon and tin asymmetric compounds into optically active components shows that these compounds have the tetrahedral structure characteristic of carbon compounds, and that in all probability the above series of tetra-alkyl derivatives are all constituted on the tetrahedral type.

Such relationships as these afford ample justification for including carbon, silicon, germanium, tin, and lead in the same natural family.

The points of difference between carbon and silicon are also illustrated by investigations of the organic derivatives of the latter. Kipping and Robison showed that silicones differed from the ketones in having polymerised molecules; they worked this difference out in the case of benzyl ethyl silicone, which has a trimeric molecule

$$\left[\overset{C_6H_5 . CH_2}{\underset{C_2H_5}{}} \!\!\! >\!\!SiO \right]_3$$
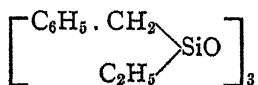
unlike the non-polymerised molecule of the ketones RR'CO. This fact helps to explain the difference between the refractory solid oxide, silica $[SiO_2]_x$, and the gaseous carbon dioxide CO_2.

(b) *The Aluminium Family*

The third vertical series is similarly divisible into two natural families, and taking again the capacity for forming organic derivatives as the important criterion of relationship, we find that boron, the initial member of the series, falls into line with aluminium and its homologues in yielding organic derivatives :

Aluminium family .	. B	Al	Ga	In	Tl
Rare earth ,,	. .	Sc	Y	La, etc.	

The other family containing scandium, yttrium, and some other twelve or thirteen elements of the rare earth series

including lanthanum, do not give rise to organo-metallic derivatives. In this vertical series, therefore, as in the fourth, the two families show very different capacities for forming compounds containing hydrocarbon radicals.

The boron compounds with alkyl radicals are produced by the general method from zinc alkyls:

$$2BCl_3 + 3Zn(C_2H_5)_2 = 3ZnI_2 + 2B(C_2H_5)_3.$$
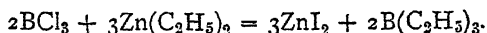
The corresponding boron trimethyl is made by similar means from ethyl borate.

These alkyl boron compounds are possessed of somewhat remarkable properties, one might even say inconvenient properties, regarded from the standpoint of the conventional theories of valency. The existence of the great majority of boron compounds can be readily explained on the assumption that the element is uniformly tervalent, corresponding with the chloride BCl_3 and the oxide B_2O_3. But if this degree of combining power represented all the chemical affinity possessed by boron, then the boron trialkyls should be as inert as the paraffins, for example, tertiary pentane (carbon tetramethyl, $C(CH_3)_4$), in which, as we have already seen, the carbon is surrounded by four methyl radicals situated at the apices of a regular tetrahedron containing the carbon atom at its centre. The boron trialkyls behave, however, as highly unsaturated compounds; they combine additively with ammonia, and are readily absorbed by the caustic alkalis. The compound $[B(CH_3)_3, NH_3]$ is possessed of considerable stability, melting at 51° and boiling at 110°. The compound with caustic potash has the composition $B(CH_3)_3, KOH$.

If instead of regarding valency as being always entirely integral we consider it as partly fractional and depending to a large extent on the possibilities of arrangement, we can obtain an explanation for the existence of these additive compounds.

The most symmetrical mode of arranging three methyl groups round a central boron atom is at three points 120° apart on a great circle of the boron sphere of influence. This would also be the most symmetrical way of arranging the atoms in a molecule of ammonia. This arrangement is, however, less symmetric than the tetrahedral structure which could be produced by adding another associating unit to either of these molecules. The boron and the nitrogen of the ammonia have

sufficient residual affinity to make this addition possible, and accordingly rearrangement occurs in both molecules with the setting up of the more symmetric tetrahedral configurations:
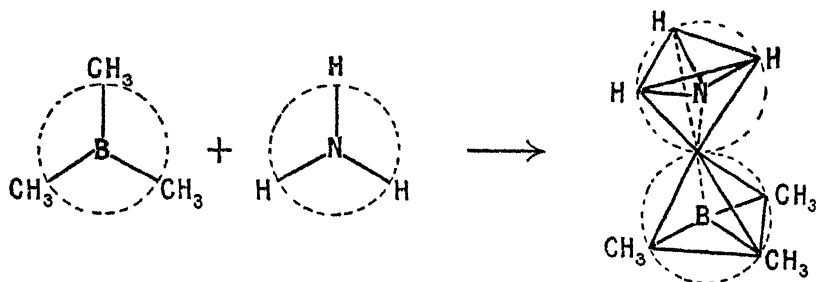

In this additive compound the three methyl groups are held in position by the principal valencies of the boron atom, while the ammonia group is held in position by the mutual action of the residual affinity of the boron and nitrogen atoms.

The addition of ammonia to tertiary pentane, $C(CH_3)_4$ would destroy and not increase the existing symmetry of the molecule, and hence this combination does not occur. In the struggle for existence among chemical compounds the most symmetrical types tend to survive.

Boron combines with aryl radicals, and phenylboron dichloride results from the interaction of boron trichloride and mercury diphenyl. This dichloride is decomposed by water, yielding phenylboric acid, $C_6H_5 . B(OH)_2$, which is an antiseptic far more powerful than boric acid. Both acids are volatile in steam.

The alkyl derivatives of aluminium obtained by the action of the metal on mercury alkyls are spontaneously inflammable in air and are at once decomposed by water:

$$Al(C_2H_5)_3 + 3H_2O = Al(OH)_3 + 3C_2H_6.$$
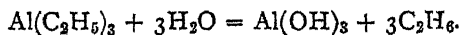
Aryl derivatives have not been obtained.[1]

Gallium and indium are extremely rare metals, and hitherto only alkyl derivatives of the latter have been studied.

Thallium readily yields both alkyl and aryl derivatives:

$$TlCl_3 + Zn(C_2H_5)_3 = ZnCl_2 + Tl(C_2H_5)_2Cl.$$
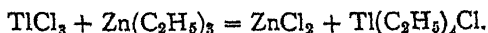
The product, thallic diethyl chloride, can be converted into

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[1] Aluminium triphenyl has recently been prepared (*Ber.* 1912, **45**, 2828) from aluminium foil and mercury diphenyl as a very unstable solid decomposed by water and not distillable even *in vacuo*.

thallic diethyl hydroxide, $Tl(C_2H_5)_2 . OH$, a strongly alkaline base.

Thallium diphenyl bromide, $Tl(C_6H_5)_2Br$, is an example of an aryl derivative obtained through the agency of the Grignard reaction, a process which is explained in the following section.

(c) The Glucinum Family

In the vertical periodic series containing the typical bivalent metals we come across a family possessing in a very marked degree the property of yielding organic derivatives. This is the glucinum family, including zinc and mercury, the organic derivatives of which have already been discussed.

The other natural family of this series contains the metals of the alkaline earths and radium :

Gl	Mg		Zn		Cd		Hg	
		Ca		Sr		Ba		Ra

The organic derivatives of calcium have been mentioned as substitutes for the corresponding compounds of magnesium in the Grignard reaction, but do not appear to have met with any considerable degree of success. Very little is known concerning the organic derivatives of strontium and barium, the metals of the alkaline earths showing little or no tendency to combine with hydrocarbon radicals. Radium is the final member of this family, and it therefore seems unlikely that organic derivatives of this remarkable element will be readily obtained.

In the glucinum family cadmium shows the least tendency to unite with hydrocarbon radicals. Cadmium dimethyl and diethyl have been obtained, but only in very small yield; they are fuming liquids spontaneously inflammable in air and energetically decomposed by water.

The glucinum compounds are prepared by the interaction of this metal and mercury alkyls. Glucinum dimethyl and diethyl are fuming liquids decomposed by water but not spontaneously inflammable in air.
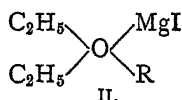
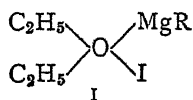*The Grignard Reagents.*—To the early workers in this field the organic compounds of magnesium did not appear to be very promising materials for synthetic purposes. But as the result of modern researches, all carried out within the last thirteen years, the organo-magnesium derivatives have proved to be the most general synthetic agents hitherto discovered in organic chemistry.

In 1899 Barbier found that a mixture of magnesium and

methyl iodide in the presence of dry ether behaved towards certain ketones in the same way as zinc methyl. Grignard took up the study of this reaction in 1900 and discovered the important reagents which now bear his name.

It is possible to obtain as white solids the so-called individual magnesium compounds, composed only of magnesium and alkyl groups. But in the presence of dry ether this solvent enters into reaction and the Grignard reagents are additive compounds of ether with the individual magnesium alkyl or aryl halide, $MgRI, (C_2H_5)_2O$.

V. Baeyer regards these compounds as derivatives of quadrivalent oxygen (I.), and Grignard proposes the alternative formula (II.):

$$C_2H_5 \diagdown \diagup MgR \qquad\qquad C_2H_5 \diagdown \diagup MgI$$
$${>}O{<} \qquad\qquad\qquad {>}O{<}$$
$$C_2H_5 \diagup \diagdown I \qquad\qquad C_2H_5 \diagup \diagdown R$$
$$\text{I} \qquad\qquad\qquad\qquad \text{II.}$$

Whichever of these two configurations be accepted, it will be seen that the same general principle is at work, namely the grouping of four radicals round a central atom—in this case oxygen—with possibly a development of tetrahedral symmetry.

Similar compounds are known in the case of zinc, $Zn(CH_3)_2$, $(C_2H_5)_2O$, and Tschelinzeff has isolated magnesium compounds with two and four molecular proportions of ether and this solvent may in certain cases be replaced by tertiary amines, $MgRI, NX_3$.

These Grignard reagents are not spontaneously inflammable in air, and being readily soluble in many anhydrous organic solvents are much more easily handled than the inflammable zinc alkyls. Accordingly, these reagents have already received a very wide application, and in the hands of Acree, Béhal, W. H. Perkin, jun., Zelinsky and numerous other investigators have facilitated many valuable syntheses which could not otherwise have been effected. Perkin's synthesis of terpineol may be cited as a prominent example of the use of magnesium methiodide.

In this paper attention will be confined to the use of the Grignard reagents in preparing organo-metallic and metalloidal derivatives.

(d) The Alkali Metals

In the first vertical series of the periodic table we find the well-defined family of alkali metals which are typically univalent

metals. The tendency for these metals to unite with hydrocarbon radicals is very slight. Organic derivatives of sodium and potassium were indicated by Frankland and Wanklyn but not isolated as individual compounds. Here, as in the case of magnesium, residual affinity plays an important part in increasing the stability of the organic derivatives. Sodium ethyl, although not isolated as such, has been obtained in an additive compound with zinc ethyl containing the two metals in atomic proportions, $Na . C_2H_5, Zn(C_2H_5)_2$.
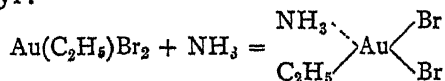
The metals of the alkalis resemble those of the alkaline earths as regards their feeble affinity for hydrocarbon radicals.

(e) The Gold-Platinum Group

Alternating with the alkali metals in the first vertical series, we find the currency metals, copper, silver, and gold. Although researches are even now in progress, only one of these, namely gold, has been definitely combined with hydrocarbon groups. This combination was successfully accomplished by Pope and Gibson in 1907, by acting on auric bromide with the Grignard reagent, magnesium ethiodide :

$$AuBr_3 + 2MgC_2H_5I = Au(C_2H_5)_2Br + MgI_2 + MgBr_2.$$
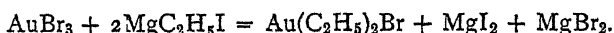
The product, diethylauric bromide, which was obtained in colourless needles, reacted with bromine to yield ethylauric dibromide, $Au(C_2H_5)Br_2$, a ruby-red crystalline compound combining additively with ammonia, and in this respect resembling boron trimethyl :

$$Au(C_2H_5)Br_2 + NH_3 = \begin{array}{c} NH_3 \diagdown \\ \diagup \\ C_2H_5 \end{array} Au \begin{array}{c} \diagup Br \\ \diagdown Br \end{array}$$
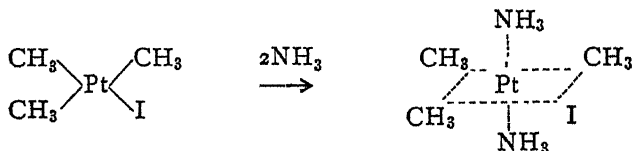
In this reaction the general tendency to form the compound with four associating units is again apparent.

The metal platinum, closely allied in many respects to gold, occurs in the periodic scheme as the final member of the metals of the eighth vertical series, which contains nine metals arranged in three sets each with three members. Pope and Peachy have successfully applied the Grignard reagent to platinic chloride dissolved in dry ether :

$$PtCl_4 + 3Mg(CH_3)I = (CH_3)_3PtI + MgI_2 + 2MgCl_2.$$

Trimethylplatinic iodide (bright yellow crystals) is converted by moist silver oxide into trimethylplatinic hydroxide

(colourless needles), a basic substance insoluble in water, but soluble in nitric acid to the corresponding nitrate, $(CH_3)_3$ Pt . NO_3. The original iodide combines additively with two molecules of ammonia, a combination which in all probability involves the formation of a molecule having octahedral symmetry :

$$
\begin{array}{ccc}
CH_3 \diagdown \quad \diagup CH_3 & 2NH_3 & \quad\quad NH_3 \\
\quad\quad \diagup Pt \diagdown & \longrightarrow & CH_3 \text{----} Pt \text{----} CH_3 \\
CH_3 \diagup \quad \diagdown I & & CH_3 \quad I \\
& & \quad\quad NH_3
\end{array}
$$

At present very little is known regarding the organic derivatives of the other metals of the platinum family. Both in this series and in the first vertical series, including the alkali and currency metals, it is significant that it is the metal of highest atomic weight—platinum in one case, gold in the other—which has combined most readily with alkyl radicals.

<div align="center">GENERALISATIONS</div>

<div align="center">1. INFLUENCE OF ATOMIC WEIGHT ON THE STABILITY AND EASE OF FORMATION OF ORGANIC DERIVATIVES</div>

In the natural families of elements thus far considered the capacity for forming organic derivatives appears to increase with the rise in atomic weight. Gold and platinum, the final members of their respective groups, have just been cited as a case in point. Thallium, the final member of the aluminium family, is the only one yielding readily both alkyl and aryl derivatives. Iodine, although a non-metal, may be quoted, as it furnishes iodinium bases such as $I(C_6H_5)_2$. OH containing two phenyl or other aryl groups, a property which is not possessed by the halogens of lower atomic weight.

<div align="center">*Organo-Mercuric Compounds*</div>

Mercury, the final member of the glucinum family, affords a striking illustration of the great capacity for combining with organic groups possessed by metals and metalloids of high atomic weight. This metal possesses a very marked affinity for carbon, and enters into combination with a large number of organic substances of very varied type.

In many instances the attachment of mercury to carbon can be effected merely by boiling the organic substance with mercuric acetate in a suitable solvent. This is notably the

case with phenols and aromatic amines. The mercury enters the amine molecule in two stages, the mercuri-acetate group first attaches itself to the amino-nitrogen, and then swings into the aromatic nucleus either into para- or the ortho-position in accordance with the law governing substitution in the benzene series. This process can be repeated, and in the case of *meta*-toluidine as many as three mercuri-acetate groups can be introduced into the molecule:

$$CH_3 . COOHg \underset{\underset{HgOOC . CH_3}{CH_3}}{\overset{NH_2}{\bigcirc}} -HgOOC . CH_3$$
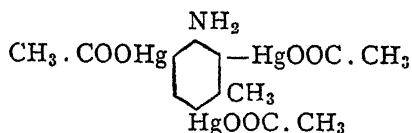
This compound contains 93 per cent. of mercury and is extremely soluble in water.

By heating mercuric acetate with aromatic compounds at high temperatures, products are obtained containing mercury attached to two organic radicals.

Meta-nitrobenzoic acid and mercuric acetate yield such a compound which on reduction furnishes an amino-acid, the sodium salt of which has the following formula:

$$Hg[C_6H_3(NH_2) . CO_2Na]_2.$$
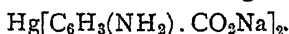
2. THE MASKED OR HIDDEN CONDITION OF METALS AND METALLOIDS IN THEIR ORGANIC DERIVATIVES

The above sodium salt of the mercury derivative of meta-aminobenzoic acid is soluble in water, but owing to its double attachment to carbon the mercury present in the salt does not show its ordinary analytical reactions. Before the mercury can be detected by the usual tests for the metal, its attachment to the two aromatic rings must be destroyed. This masked condition of the mercury extends to the physiological action of the compound, which is thirty times less toxic than mercuric chloride. The substance has marked bactericidal and spirochætocidal properties, and can be tolerated in large doses. A rabbit weighing about 5 lb. was not injured by a one-gram dose of this sodium salt.
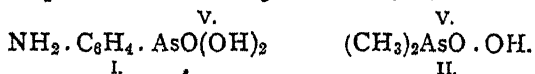
Mercury compounds with aromatic amines are likely to prove of therapeutic value, as the amino groups have the property of entering into combination with certain constituents of the parasitic organisms binding drug and organism together, while the poisonous metal does its work on the bacterium or spirochæte.

Organic Arsenic Derivatives

In 1865 Béchamp discovered among the products of the interaction of aniline and arsenic acid a compound which he supposed was an anilide of arsenic acid, that is, a substance in which carbon and arsenic are not joined directly but through the intermediary of oxygen. Experiments on animals showed that this compound was much less toxic than the ordinary inorganic compounds of arsenic, and it was found safe to administer forty times as much arsenic in the form of the supposed arsanilide as in potassium arsenite (Fowler's solution). The compound, which was accordingly called *atoxyl*, came into increasing demand as the result of the discovery that it had considerable germicidal powers and could be used in the treatment of sleeping sickness and other diseases of protozoal origin.

In 1907 Ehrlich and Bertheim showed that atoxyl was a true organo-metalloidal compound, the arsenic being directly attached to carbon. The compound therefore furnishes another striking example of the masked or hidden state of metals and metalloids in their organic derivatives, this intimate state of combination with carbon leading to a modification in the analytical and physiological reactions of these elements.

Although destroying trypanosomes *in vivo*, atoxyl has no effect on these organisms *in vitro*. A preliminary change, which takes place in the tissues of the host, appears to be necessary before the drug becomes effective. The arsenic in atoxyl (I.) is in the same saturated quinquevalent condition as it was in Bunsen's non-poisonous cacodylic acid (II.)

$$\overset{\text{v.}}{NH_2 . C_6H_4 . AsO(OH)_2} \qquad \overset{\text{v.}}{(CH_3)_2AsO . OH.}$$
$$\underset{\text{I.}}{} \qquad . \qquad \underset{\text{II.}}{}$$

The substance which actually destroys the trypanosomes in the body of the host is in all probability a compound of tervalent arsenic. Following up this hypothesis, Ehrlich after many trials ultimately arrived at the compound, salvarsan (III.), or "606," this number indicating the series of substances which had been examined before success was attained:
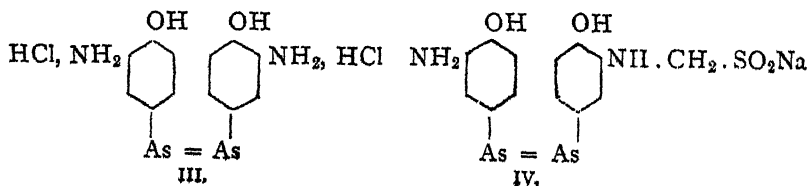
Salvarsan inhibits the growth of trypanosomes in a test-tube as well as in the body of the host. It contains the two contiguous *ortho*-aminohydroxyl groups which serve as "haptophores" for attaching the molecule to the parasitic organism. Ehrlich compares the compound to the poisoned arrows used by savages, the amino-phenol complex being the barbed arrow-head, the two benzene nuclei serving as the shaft of the arrow, while the two unsaturated arsenic atoms are the poison smeared on the arrow.

The drug is made up in the form of its dihydrochloride (III.), and the practical difficulties attending its use are its great oxidisability and the careful preparation needed to secure a neutral solution, which is especially necessary when the substance is administered intravenously. To obviate the latter difficulty a modified drug has been devised, known as *Neosalvarsan* (IV.), which is prepared by treating salvarsan with formaldehyde sulphoxylate, the result being that one or two $CH_2 . SO_2H$ groups become attached to aminic nitrogen, so that the product becomes distinctly acidic and capable of forming a stable neutral sodium salt (IV.).
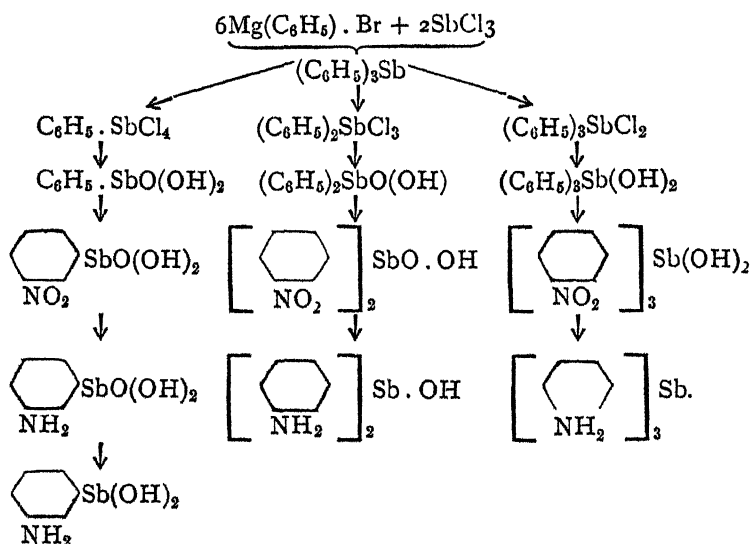
Organic Derivatives of Antimony

Antimony has long been administered therapeutically in the form of its salts, especially as potassium antimonyl tartrate, the well-known tartar emetic. It would be of great interest to ascertain what modification in the action of the metalloid would be effected by combining it with hydrocarbon radicals.

Many organic derivatives of antimony are known containing alkyl or aryl radicals, or both. I do intend discussing these beyond showing the steps by which quite recently the antimony analogue of atoxyl has been reached. We may expect the antimony analogue of salvarsan to follow, although its advent has not yet been recorded.

The best general method of attaching aromatic radicals to antimony is through the Grignard reaction, as, for example, with antimony trichloride and magnesium phenyl bromide, this condensation leading to *triphenylstibine*. From this product three series of aromatic antimony compounds can be obtained containing one, two, and three phenyl radicals attached to antimony. By chlorination followed by hydrolysis the corre-

sponding organic stibinic acids are produced, and these three substances were shown by Miss Micklethwait and the author to yield *meta*-nitro derivatives, from which, on reduction, aromatic amino-compounds containing antimony were obtained. These amines had some trypanocidal action, but were also very irritant on injection. Their production is shown in the following diagram, which illustrates the genesis of the series from the Grignard reagent and antimony trichloride:

$$6Mg(C_6H_5) . Br + 2SbCl_3$$

$$(C_6H_5)_3Sb$$

$C_6H_5 . SbCl_4$	$(C_6H_5)_2SbCl_3$	$(C_6H_5)_3SbCl_2$
$C_6H_5 . SbO(OH)_2$	$(C_6H_5)_2SbO(OH)$	$(C_6H_5)_3Sb(OH)_2$

Owing to the fact that the oxidised antimony radical induces nitration in the meta-position, all the foregoing amines are meta-derivatives, differing in this respect from atoxyl, which is a para-compound. But within the last few months the isolation of para-aminophenylstibinic acid has been accomplished. The starting-point is acetyl-*p*-phenylenediamine (V.), which when diazotised combines, as was shown by P. May, with antimony chloride; this double salt (VI.) when gently warmed with dilute alkali and copper powder yields *antimony-atoxyl, sodium para-aminophenylstibinate* (VII.):
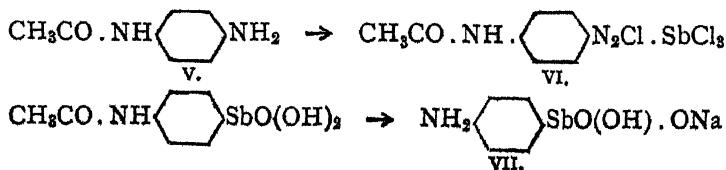
Some of these aromatic amino-derivatives of antimony may find therapeutic application, and already *triphenylstibine sulphide*, $(C_6H_5)_3SbS$ ("sulphoform"), has been used in the treatment of skin diseases.

3. Influence of Valency on the Stability of Organo-metallic and Metalloidal Compounds

Reference has already been made to the influence of valency in the formation of organic compounds of metals and metalloids. There is no well-authenticated case where a univalent metal furnishes an organic derivative capable of existence as an individual compound. Sodium ethyl is only known in combination with zinc ethyl. When a metal has several valencies the tendency is for the organic compound to contain the metal in its highest state of valency. Mercury, thallium, gold, and lead exhibit this tendency, as is shown by their organic derivatives already cited. A series of comparative experiments with camphor and the elements of the arsenic family show the same tendency at work. Sodium camphor was condensed with the trichlorides of phosphorus, arsenic, and antimony containing the non-metal or metalloid in the tervalent condition, the products as shown by the following table, contained these elements in the quinquevalent state:

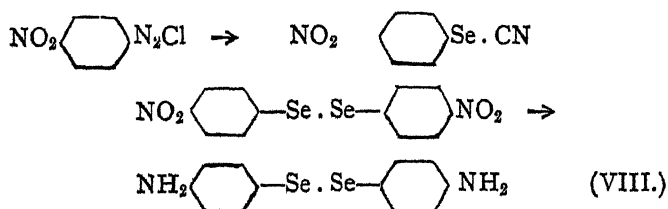*Condensation Products from Sodium Camphor and the Trichlorides of the Phosphorus Group*

Products.	Phosphorus trichloride.	Arsenic trichloride.	Antimony trichloride.
Dicamphoryl derivatives.	$(C_{10}H_{15}O)_2PO . OH$, dicamphorylphosphinic acid, stable in concentrated aqueous alkali hydroxides; decomposed by fused alkali hydroxides.	$(C_{10}H_{15}O)_2AsO . OH$, dicamphorylarsinic acid, stable in hot dilute aqueous alkali hydroxides; decomposed by very strong solutions of these alkalis.	——
Tricamphoryl derivatives.	——	$(C_{10}H_{15}O)_3As(OH)_2$, tricamphorylarsinic acid, is as stable towards alkalis as the above dicamphoryl derivative.	$(C_{10}H_{15}O)_3SbCl_2$, tricamphorylstibinic chloride, slowly resolved by water into $(C_{10}H_{15}O)_3Sb(OH)_2$, tricamphorylstibinic acid, very unstable, decomposed by dilute aqueous sodium hydroxide and even by boiling water.

This very general tendency affords confirmation for the view that valency is largely a question of arrangement in space.

The atomic volume of carbon is less than that of other elements, and accordingly when a metal or metalloid unites directly with carbon there is room for the maximum number of associating units.

Organic Derivations of Selenium and Tellurium

Considerable attention is now being given to the study of organic compounds of selenium and tellurium owing to a recent statement made by v. Wassermann to the effect that combinations containing these elements had been noticed to induce diminution in the growth of malignant tumours. Bearing in mind the beneficial results obtained with atoxyl and salvarsan, it seems likely that the most promising field for research lies in the study of the aromatic derivatives of these two elements. The Grignard reaction is available for both, and recently it has been found that selenium can be introduced into aromatic nuclei through the agency of the diazo-reaction. The following series has been completed by Mr. Elliott of the Royal College of Science, Dublin. Starting with diazotised para-nitraniline, selenium is introduced by the action of potassium selenocyanide KCNSe :

$$NO_2\langle\bigcirc\rangle N_2Cl \rightarrow NO_2 \langle\bigcirc\rangle Se.CN$$

$$NO_2\langle\bigcirc\rangle -Se.Se-\langle\bigcirc\rangle NO_2 \rightarrow$$

$$NH_2\langle\bigcirc\rangle -Se.Se-\langle\bigcirc\rangle NH_2 \qquad (VIII.)$$

The organic selenocyanide when hydrolysed yields di-*p*-nitrophenyldiselenide; this on reduction furnishes di-*p*-aminophenyldiselenide (VIII.), an oxidisable base which, like salvarsan, can be utilised in the form of its more stable dihydrochloride.[1]

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[1] Alternating in the periodic scheme with the arsenic and selenium families, we find, as in other cases, two other groups of elements showing little or no capacity for yielding organic derivatives ; these are the vanadium family (with columbium and tantalum) and the chromium family (with molybdenum, tungsten, and uranium). It was formerly supposed that tungsten had yielded organic derivatives, but this statement has since been contradicted.

4. ORGANO-METALLIC AND ORGANO-METALLOIDAL RADICALS BEHAVING AS COMPLEX ALKALI METALS

A necessarily brief and imperfect survey has now been made of all the families of metals and metalloids capable of yielding organic derivatives. It will have been noticed that in several instances, for example with certain derivatives of arsenic, mercury, and thallium, it is possible to obtain complex alkaline hydroxides having the properties of caustic soda or potash.

The alkali metals, potassium, lithium, sodium, rubidium, and cæsium, are distinguished from most other metals by their univalency and by their property of yielding very soluble alkaline hydroxides. Moreover, they are distinguished from all other elements by having each in its own horizontal series the maximum atomic volume. When this property (atomic volume) is plotted for all the elements against the atomic weight as was done by Lothar Meyer, it is seen that the alkali metals occupy points of maxima on the curve.

The study of organic derivatives of metals, metalloids, and non-metals shows that one can synthesise a compound alkali metal by associating with many polyvalent elements sufficient alkyl or aryl radicals to reduce the principal valency to unity. If the remaining valency is satisfied by iodine, the result is a saline iodide in which the iodide ion can be replaced by hydroxyl, usually through the agency of moist silver oxide, giving rise to a basic hydroxide which in the majority of cases is soluble in water to a caustic alkaline solution. The following series of organo-metallic, organo-metalloidal, and organo-non-metallic hydroxides illustrates this principle :

$$N(CH_3)_4OH, \quad P(C_2H_5)_4OH, \quad As(C_7H_7)_4OH, \quad Sb(CH_3)_4OH,$$
$$Se(CH_3)_3OH, \quad Sn(C_2H_5)_3OH, \quad Pb(C_2H_5)_3OH, \quad Pt(CH_3)_3OH,$$
$$I(C_6H_5)_2OH, \quad Tl(CH_3)_2OH,$$
$$Hg(C_2H_5)OH \quad Hg(C_6H_5)OH.$$
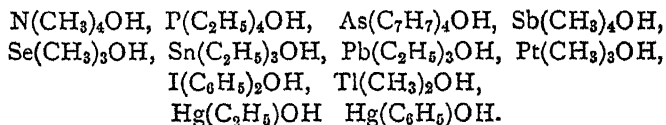
All these substances, with the exception of trimethyl-platinic hydroxide, are soluble in water, giving rise to strongly alkaline solutions, which absorb carbon dioxide, precipitate the heavy metals from their soluble salts, and saponify fats, thus behaving quite like the strong caustic alkalis, sodium and potassium hydroxides.

Providing that the univalent complex organo-metallic or

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metalloidal ion has a sufficiently large molecular volume, it resembles the bulky elementary ion of sodium or potassium in furnishing ionisable halides (chlorides, bromides, and iodides) and soluble strongly alkaline hydroxides.

Nowadays, when elementary atoms are regarded as having a composite structure, this synthesis of compound alkali radicals in the manner just indicated is a fact of great significance.

CONCLUSIONS

The investigations in the wide field of organic derivatives of metals and metalloids on which I have touched so very superficially have amply justified themselves in a variety of ways.

On the theoretic and doctrinal side of chemistry they have proved to be of fundamental importance in establishing the theory of compound radicals. They have greatly enlarged our conceptions of stereo-chemistry and the structure of molecules, and have thrown much additional light on the manifestations of chemical affinity and valency.

From the practical standpoint these researches have endowed chemists with the Grignard reagents and other synthetic agents of a most general type.

To the physician they have furnished several valuable series of synthetic drugs in which a close connection can be traced between chemical constitution and physiological action, and the combined chemical and clinical study of these materials has given rise to a new science—" Chemiotherapy."

At the outset an apparently fantastic development of chemical synthesis, these experimental researches have vindicated the cogency in chemistry of Bamberger's bold assertion, " Ohne Phantasie kommen wir nicht weiter."

PROF. JOHN MILNE

By CHARLES DAVISON, Sc.D., F.G.S

To three Englishmen, living almost in three different centuries, we are chiefly indebted for the advances which have culminated in the new science of seismology. JOHN MICHELL (1724–1793), one of the early Woodwardian professors at Cambridge, wrote the first important memoir on a great earthquake, that of Lisbon in 1755, and, in endeavouring to account for its various phenomena, foresaw some of the main lines on which the science has since developed. ROBERT MALLET (1810–1881), a Dublin engineer, with unfailing industry codified our knowledge of the nature of earthquakes and devised new methods of investigation which he applied to the Neapolitan earthquake of 1857. Much of Mallet's work remains, but his methods and theoretical views are to a great extent superseded, and the instruments which he devised for the registration of earthquakes are of little value. It was reserved for JOHN MILNE (1850–1913) to advance far beyond the limits to which Michell and Mallet attained. His influence and energy were such that, at the close of his life, when the study of earthquakes has attracted a host of workers and its practical importance is fully recognised, we may yet claim for him the chief share in the growth of the science.

Of the three, Milne received the training best adapted to his future career. Born at Liverpool on December 30, 1850, he was educated as a mining engineer under Warington Smyth at the Royal School of Mines. After gaining experience in the mines of Cornwall, Lancashire, and Central Europe, he spent two summers in ascertaining the mineral resources of Newfoundland, while his interest in geology was manifested by the valuable remains of the great auk which he brought home from Funk Island. In 1874 he acted as geologist in Beke's expedition to north-west Arabia ; and, a year later, received the appointment which determined the bent of his future life, that of consulting mining engineer and geologist to the Government of Japan.

It was characteristic of Milne's energy and wide interests

that he preferred to approach his new home by a solitary and toilsome journey overland. Crossing Asia, almost along the line of the present Siberian Railway, and making many geological observations on the way, he arrived at Tokyo after the lapse of nearly a year. On the first night spent in that city, he began his acquaintance with Japanese earthquakes. A strong shock made his house creak and pictures sway, and it is said that, from that moment, the main interest of his life was fixed.

In its early days, the Tokyo University depended largely on foreign aid. On his arrival in 1876, Milne found among its professors the late W. Ayrton as well as J. Perry and J. A. Ewing, all of whom became interested in the construction of accurately recording seismographs. Milne's opportunity, however, came with the destructive earthquake of February 22, 1880, when the neighbouring port of Yokohama was laid in ruins. In a country visited by a thousand earthquakes a year, the materials are too abundant for solitary workers, and Milne realised that it was only by the co-operation of many students and observers that substantial advances could be made. As the result of a public meeting due to his initiative, the Seismological Society of Japan —the first society devoted exclusively to the study of earthquakes and volcanoes—was founded in the spring of 1880, with Mr. J. Hattori as president and Milne as secretary.

In later years Milne often claimed that the formation of this society marks an epoch in the history of seismology, and all will admit the justice of the claim. Little, it was recognised, could be done without the aid of an accurate seismograph, the essential feature of which is that a part should remain at rest or nearly so while the ground to which it is attached is in constant motion. The problem was solved satisfactorily by members of the Seismological Society, and it is to them, and especially to Ewing, Milne, and Gray, that we are indebted for the first instruments deserving of the name of seismographs. The frequent earthquakes of Japan soon offered the materials for registration, and the diagrams of these early seismographs represented with precision the movements of the ground during earthquakes both great and small. The Seismological Society lasted for about twelve years, and ceased to exist in 1892, mainly because its work could be carried on more completely under official guidance and control. During the greater part of the time Milne might almost have said that he himself was the

Seismological Society. He certainly did most of its work. Of the sixteen volumes of *Transactions* published by the Society and of the four volumes of the *Seismological Journal* which he afterwards edited, he wrote not less than two-thirds. But his labours did not end with his actual contributions. It was under his guidance and led by his enthusiasm that many of the other papers were written, and that native investigators, and in particular the present eminent professor of seismology at Tokyo, were trained to carry on the work after his return to Europe.

The papers which Milne contributed to these twenty volumes vary widely in their subjects. There were indeed few branches of the science with which he did not at some time or other deal. Occasionally he would touch on the lighter side, such as the effects of earthquakes on animals and the emotional and moral effects of earthquakes on human beings. He made many experiments on artificial earthquakes caused by the fall of heavy iron balls or by explosions of dynamite. The minute, and sometimes almost incessant, tremors of the ground attracted much of his attention, and he was probably correct in attributing them, in part at least, to the pressure of the wind on the mountains of Central Japan. He soon recognised that the vibrations of a given earthquake varied in strength and period in different parts of Tokyo, and this led him to carry out what he called a seismic survey of that city. Of still greater importance were his survey of the whole of Japan and his determination of the districts in which the principal earthquakes originated. His mode of working was characteristic. Enlisting the aid of numerous observers in all parts of the country, he provided them with bundles of postcards, one of which with the necessary details was sent to him whenever an earthquake was felt. In this way he was able to determine the region beneath which each earthquake occurred, and thus to ascertain and map those parts of the country that were most liable to be shaken. But the method had other and more permanent results, for it led to the formation of the network of nearly a thousand observing stations which are now scattered over the empire of Japan. Of this valuable system Milne was able to avail himself in the last work which he published before leaving the country. The concluding volume of the *Seismological Journal* consists of his great catalogue of 8,331 Japanese earthquakes during the years 1885–92. The volume, however, is no mere list of dates. For each earthquake

it gives, in addition to other elements, the dimensions of the disturbed area and the approximate position of its centre. The illustrative map which Milne prepared shows the positions of these centres and incidentally reveals the great law of their distribution, namely, that earthquakes are most numerous on the side of Japan facing the Pacific Ocean, and especially beneath the ocean bed shelving steeply into the Tuscaroora Deeps.

Shortly after Milne began the study of earthquakes, he received aid from the British Association in the construction of seismographs and for other allied purposes. Money-grants are made by the Association to committees and never to individuals. But when the chairman of the committee lives in England, and the secretary who does the work in Japan, the committee becomes identified with the secretary. Thus, the fifteen valuable reports of the committee were the work of Milne alone. They gave brief, and readily accessible, summaries of the many investigations which he carried out in Japan.

Milne remained in Japan for nearly twenty years, and during this time collected an extensive library of earthquake-literature and furnished his observatory with numerous instruments mostly of his own design. The close of his residence in the country was ;marked by a deep feeling of animosity among the Japanese towards foreigners generally. Though it was by the action of Russia, France, and Germany alone that they were afterwards deprived of the principal fruits of the war with China, there can be little doubt that it was from political motives that almost the whole of his property was destroyed in February 1895. Early one Sunday morning, a fire broke out in a pile of wood in an outhouse and spread so rapidly that, in half an hour, Milne was standing in his night-dress looking at the smoking ruins of his home, with some papers and a few books at his feet to represent all that was saved of the accumulations of twenty years. Great as it was, the loss, though uncovered by insurance, was not wholly irreparable. The library was in part at least replaced, and Milne with his usual energy at once set about the construction of two new pendulums, so as to lose no time in renewing observations when he arrived in England.

Milne reached this country and began the third period of his life in July 1895. With his Japanese wife, he made his home at Shide Hill House, near Newport, in the Isle of Wight.

He at once constructed pillars on which to erect the pendulums he had brought from Japan, and in three weeks he began the long series of records which have made his name and observatory so widely known. As far back as 1883 he had predicted that with suitable instruments every great earthquake might be recorded in all parts of the globe. Within the next twelve years, observations in Germany and England, as well as in Japan, fulfilled the prediction and at the same time showed that the horizontal pendulum, in one or other of its various forms, was admirably adapted for the purpose. Milne preferred his own form of pendulum, with photographic registration ; and this form, with some improvements, still holds the field in British, as well as in some foreign, observatories.

During his last years in Japan, Milne's interest in the phenomena of local earthquakes gave place to that in the phenomena of what he called "world-shaking earthquakes"; and in his new home it was only natural that the later interest should prevail. At the first meeting of the British Association held after his return, the committee on the earthquake and volcanic phenomena of Japan, which had naturally ceased to exist, was merged in that on earth tremors, and the joint seismological committee took up the great task of organising a seismic survey of the world. As a similar task had also been undertaken at about the same time by the International Seismological Association, with its headquarters in Strasburg, Milne's work became almost, though not entirely, confined to British colonies. Beginning with his observatory at Shide, the network of stations extended year by year, until the number of stations contributing records to the Seismological Committee now amounts to thirty-four. In this country, in addition to Shide, there are ten other stations furnished with the Milne seismograph or similar instruments. In the British possessions they are to be found in Canada and British Columbia, in Ascension Island and the Cape of Good Hope, and in various parts of India, Australia, and New Zealand. Records are also sent to the committee from several observatories in foreign countries, in Spain, the Azores, and Syria, and from such distant island stations as Fernando Noronha and Honolulu. Since 1899 the records have been published twice a year in Circulars; while from 1896 onwards the results have been discussed in the valuable reports presented annually at the meetings of the

British Association. These reports are not entirely the work of the late secretary, for Milne always welcomed the co-operation of other members of the committee; but the portions of chief and abiding interest are those in which he determined the origins of the sixty or more world-shaking earthquakes recorded every year in the observatories associated with the committee. It is one of the most valuable results of recent work that such determinations should be possible whether the earthquakes originate in civilised countries, beneath the ocean, or under lands inhabited by illiterate and wandering tribes. They show that the destructive earthquakes of the world are confined to about a dozen seismic regions, the more important of which lie along the steeply sloping margins of the Pacific Ocean.

In the physical history of the globe, however, an interval of fourteen years is but as one day. No fact of seismology is more clearly established than the continual migration of seismic activity. From month to month, even from hour to hour, the centre of action ranges along the line or lines of fault which give birth to a series of earthquakes. In larger districts the same displacement occurs over greater distances and at longer intervals of time. Thus the interesting maps which Milne published annually in his reports show only the region in which the earth's crust is being deformed at the present time. In past centuries the seats of chief activity may have been very different. What they were Milne sought to determine in one of his latest contributions to seismology—his "Catalogue of Destructive Earthquakes, A.D. 7 to A.D. 1899." As complete probably as such a catalogue can now be made, it is inevitably defective. The total number of entries in it is 4,151, and many of the earthquakes recorded in it would fall far short of the intensity of a world-shaking earthquake. Yet if the latter occurred throughout the Christian era at the present rate of sixty a year, the total number would be more than 113,000; that is to say, ninety-six out of every hundred great earthquakes in the past nineteen centuries may remain for ever unknown to us. Nevertheless, Milne's second great catalogue of earthquakes possesses a value that will only be fully known after a careful analysis of its contents has been made.

From what has been already said it will be obvious that Milne was a student of earthquake phenomena rather than of

individual earthquakes. With one exception he never made a detailed study of any shock. The nature of earthquake motion in general, the relations between earthquakes and other phenomena, the peculiarities of their distribution in time and space, presented greater attractions to him than the investigation of an earthquake unit.

It has been said of Milne that he was a man who never perfected anything; and in a very limited sense this was true. He preferred sometimes to start an inquiry and to leave others to finish it. His two great catalogues, of Japanese earthquakes and of destructive earthquakes, are monuments of detailed and patient labour; but, in both cases, his own analysis was slight. He was content to provide the materials which others were to use in building. He was a man of large views. He cannot be held to have proved that the frequency of great earthquakes is connected with the small migrations of the earth's pole or that there is any bond between the occurrence of earthquakes in regions so remote as the east and west margins of the Pacific. But it is something to have imagined such relations, to have tested their reality as far as his materials would allow, and thus to provide promising subjects of inquiry for a future of wider knowledge.

Several chapters of seismology, if they do not owe their origin to Milne, were largely written by him. His part in the construction of seismographs was a prominent one, and the extent and precision of our knowledge as to the nature of earthquake motion were to no slight extent the result of his labours. He was the first to realise that the vibrations of a great earthquake may be recorded in any part of the globe, the first also to carry a worldwide seismic survey into execution. What all this means, we shall only fully understand when the accumulation of many records shall enable us to unravel the mystery of the nature of the earth's interior.

The practical applications of his science always possessed a charm for a man so human as Milne. He devised a form of seismograph for registering the vibrations of railway-trains, and for discovering any defects that may exist in the engine or permanent way. His study of the fracture of deep-sea cables by earthquakes and other earth-movements is unique. But it was by his design of houses, bridges, etc., that will withstand the rough and sudden touch of earthquakes that he has chiefly earned

the gratitude of the present and future generations.   It may not always be possible to use the best sites for dwelling-houses—the neighbourhood of a great harbour may prevail over other considerations—but it is at least possible, by following the principles which Milne has laid down, to lessen very materially the destructive power of a great shock.

After a brief illness, Milne died on July 31.   By his will, he has left all his seismographs and his books and papers relating to earthquakes to the British Association, together with a sum of £1,000 subject to the life-interest of his wife.   He has also left behind him property, intangible it may be, but still more valuable. He has left an organisation for the study of earthquakes that is practically co-extensive with the British empire.   It is satisfactory to learn that this work of Milne's creation will not be allowed to lapse, that it will be continued as far as possible by other if less capable hands.   A more worthy memorial to our late leader in seismology we could not offer than by continuing and extending his work in the way that he would probably have done had he remained among us.

THE CORPUS LUTEUM, ITS STRUCTURE AND FUNCTION

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INTRODUCTION

LONG ago it was known that in the mammalian ovary sometimes a well-marked bright yellow body, the corpus luteum, appeared, easily recognisable with the naked eye; and its nature and function were the subjects of much speculation. In recent years a great deal of attention has been paid to this structure, and some light has been thrown on its histology and function. It is hoped it will be of interest therefore to set out the facts and theories recently brought forward regarding this body, which we now recognise as a ductless gland, and the part it plays in the chemical co-ordination of the body.

It is now generally known that in the female mammal the ovary is in a state of activity during the years intervening between puberty and senescence. This activity varies in intensity, and becomes very strongly marked at recurring intervals. The time elapsing between two periods of maximum activity varies from about a year (*e.g.* Monotremes, Marsupials (9), etc.) to about a month (*e.g.* Primates, etc.). The ovarian changes are frequently accompanied by well-marked alterations in the external appearance or behaviour of the animal, and these latter have long been recognised by the breeder under the name of "heat" or "rut." The climax of the period of ovarian activity is, in general, marked by the liberation of a ripe ovum or ova, and this is immediately followed by the formation of corpora lutea.

Before passing on to consider the corpus luteum itself it is necessary to glance, albeit quite briefly, at the structures present in the ovary prior to the setting free of the eggs.

The mammalian ovary consists of a mass of connective tissue, the stroma, which contains some plain muscular fibres

and numerous blood vessels. It is surrounded by a single layer of columnar cells, the germinal epithelium, and has embedded in it a large number of ova in all stages of growth, each surrounded by an epithelium. The ovum, together with its enveloping epithelium, is termed a Graafian follicle, and such follicles are present in all stages of development from the early "primordial" follicles up to those that are mature. Here only the latter need be considered.

The ripe follicle is a vesicular structure containing a large central cavity filled with a fluid, the liquor folliculi, which appears in section as a lightly staining coagulum. The ovum is situated within the cavity and usually towards one side. The wall of the follicle is formed by an epithelium several cells deep, known as the membrana granulosa, and a special part of the membrana, the discus proligerus, surrounds the ovule. In fairly small follicles mitotic figures are by no means uncommon in the cells of the membrana granulosa, but they appear to be entirely absent in the ripe follicle.

The thickness of the membrana granulosa varies in different parts of the follicle. It may be six to ten or even more cells thick on the inner side or at the point where it meets the discus proligerus, while at its outer edge it is thinner, and at the point where it will rupture, i.e. the stigma, it is not more than two or three cells thick. The outer limit of the membrana granulosa is marked by a clear homogeneous basal membrane. Outside this again the follicle is surrounded by a fibrous structure, the theca folliculi, derived from the ovarian stroma. This is divisible in the Eutheria into two parts, an inner coat of more or less granular cells, the theca interna, which is very vascular, and an outer coat of more fibrous nature, the theca externa. In one of the Marsupials (*Dasyurus*), however, as Sandes (19) pointed out, the two layers of the theca folliculi cannot be distinguished one from the other.

Usually several of these follicles attain maturity at the same time and burst, discharging their contained ova, together with a certain amount of the liquor folliculi and some blood. The cause of this rupture is obscure, and although it has been suggested that it is due to a rise of blood pressure or to the stimulation of erectile tissue in the ovary, a satisfactory explanation is not yet forthcoming.

The mammals fall into two classes, according to the manner

of their ovulation, which may be either spontaneous or not. According to Ancel and Bouin (1) in animals in which ovulation is not spontaneous, *i.e.* it requires the additional stimulus of coition to provoke it, only one kind of corpus luteum is to be found, the "*corpus luteum gestative*," and this body, however produced, always becomes fully grown and is of long duration. In the remaining mammals, where ovulation occurs spontaneously, two varieties of corpora are encountered. Firstly, during each pregnancy a "*corpus luteum gestative*" is formed similar to that in the preceding group. Secondly, in non-pregnant females at each heat period corpora lutea are formed. These do not become full-grown and have but a transitory existence and are termed "*corpora lutea périodiques.*" This does not appear to apply universally however, for in a Marsupial (*Dasyurus* (15)),[1] although the ovulation is spontaneous, it is not possible to distinguish between the corpora lutea in the pregnant and non-pregnant females.

It is perhaps better to retain the terminology in use before the work of Ancel and Bouin, as it applies equally well to either group of mammals. Thus:

The *corpus luteum verum* is the structure that forms in the ruptured follicle in the ovary of a female when pregnancy follows ovulation.

The *corpus luteum spurium* is the structure that forms in the ruptured follicle in the ovary of a female when ovulation is *not* followed by pregnancy.

Still another term is to be found in older works, namely *corpus luteum atreticum.* It is applied to an *un*ruptured follicle undergoing atrophy, *i.e.* an atresic follicle, and as it is not comparable in structure with either of the foregoing it will not be dealt with here.

Growth and Structure of the Corpus Luteum Verum

Immediately after the discharge of the ovum the follicle shrinks considerably. In some animals the cells of the membrana granulosa come together so as to close the point of rupture by a plug of epithelial cells (Bouchon épithélial), while in others apparently the opening may persist for some time. The interior of the body which is now a corpus luteum

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[1] This has since been shown to be the case in certain other marsupials (16A).

contains a large space filled with liquor folliculi and in some cases a certain amount of extravasated blood. All parts of the follicular wall now take part in the changes leading to the formation of the fully grown corpus luteum. The membrana propria is burst through in a large number of places by active irruptions of the theca folliculi, which sends shoots of connective tissue towards the centre. With these connective tissue sprouts, new blood vessels enter the corpus luteum, and in their immediate neighbourhood the membrana propria becomes lost, although in other places it persists for some time. Simultaneously with this invasion the cells of the membrana granulosa gradually become transformed into lutein cells and start to fill up the central cavity.

On account of the ingrowths from the theca, the membrana granulosa cells become divided up into groups, so that the young corpus luteum presents a lobulated appearance. Very soon, however, the irruptions burst through the membrana cells and reach the central cavity, where they form ultimately a plug of connective tissue.

In the ripe follicle the epithelial cells are small and crowded together as if under pressure. During the invasion of the thecal ingrowths these cells undergo a great hypertrophy both of the nucleus and the cell body and become much less tightly packed together. Their cytoplasm also becomes more and more granular, the granules giving to the cells that intense yellow colour so characteristic of this body, and the granulation, which becomes more marked as the formation proceeds, certainly suggests a secretory activity on the part of the lutein cells.

The matrix of the fully developed corpus luteum, then, contains two distinct kinds of cells, the lutein cells and the connective tissue network and central plug. Opinion is divided as to the origin of both of these, and it will be convenient to deal with each of them separately.

With regard to the lutein cells two main theories have been advanced. On the one hand, Von Baer in 1827 (22) suggested that the entire corpus luteum was derived from connective tissue and that the membrana granulosa took no part in its formation, and was either discharged with the ovum or degenerated *in situ*. On the other hand, Bischoff in 1842 (4) stated that the lutein cells were formed by the hypertrophy of the cells of the membrana granulosa and not from the theca, which only supplied the

connective tissue. Both views have received a considerable number of supporters, but largely owing to the work of Sobotta (20) the balance is in favour of Bischoff's view. Van der Stricht (21) has put forward yet another theory, that the lutein cells were derived not only from the cells of the membrana granulosa, but also in small part from the cells of the theca interna. On the whole, then, it appears that the lutein cells are derived from the cells of the membrana granulosa, although in some cases perhaps some of them may also be derived from the theca interna. Moreover, although certain investigators (*e.g.* Sobotta and Van der Stricht) have described the very occasional appearance of mitotic figures in the lutein cells of the growing corpus luteum, there is no doubt that by far the greater number of these cells are simply the transformed and hypertrophied cells of the membrana granulosa.

All investigators agree that the connective tissue network is derived from the theca folliculi, but differ in describing the parts taken by its constituent layers. It is stated that the network is derived from the theca interna alone or from this and also from the theca externa. In the Marsupial, *Dasyurus*, one cannot distinguish between these two layers of the theca. The part played by each layer is not yet decided or may perhaps vary in different species.

The blood vessels of the corpus luteum appear to take their origin from the vessels of the theca interna and have walls composed of a single layer of flattened endothelial cells. They form a network of cavities and resemble the "sinusoids" in the liver and kidney of an Amphibian. Definite blood vessels with the structure of venules or arterioles are not found in the corpus luteum.

The duration of this body is not known accurately; it takes but a few days to form and is generally stated to reach its maximum development about the middle of pregnancy, and after that period to decrease gradually, lasting about three months in the rabbit, and also in *Dasyurus*. During the later stages of its degeneration it loses its characteristic yellow colour and becomes fibrous and white, whence it is known as the corpus albicans. It finally undergoes fatty degeneration and is absorbed by the aid of leucocytes, while according to some observers a number of its cells become transformed into interstitial cells.

The Corpus Luteum Spurium

According to Ancel and Bouin, the corpus luteum spurium does not assume the characters of the corpus luteum verum and has but a transitory existence. It is stated, however, that in man the false corpus is similar in structure to the true corpus and originates in the same way, although it has but a short life and in two months has entirely disappeared. In other animals also it does not appear possible to find any difference in structure between the two corpora lutea. Among the Eutheria, then, the corpus luteum spurium is similar to the corpus luteum verum, but may not last for so long a time. The Marsupial, *Dasyurus*, also has the two varieties of corpora lutea which cannot be distinguished in size or structure, and, moreover, there does not appear to be such a marked difference in the duration of the two bodies (15).

The foregoing statements apply on the whole to the corpus luteum in the Eutherian mammals, although, as has been pointed out, they apply equally well to *Dasyurus*. According to certain authors, the corpus luteum does not exist or remains rudimentary in vertebrates whose eggs develop outside the mother and in the aplacental mammals (*i.e.* Monotremes and Marsupials). In so far as this remark applies to the Monotremes and Marsupials it is not correct, for *Platypus* certainly possesses well-marked corpora lutea, as do also the Marsupials. In *Dasyurus*, the only Marsupial that has been investigated as yet, the corpus luteum is similar to that of a Eutherian mammal.[1]

The Nature of the Corpus Luteum

It has been pointed out previously, but may be reiterated here, that the corpus luteum has a very glandular appearance. Indeed, in the fully formed structure the lutein cells strongly resemble the cells of an ordinary gland in a state of secretory activity and are epitheloid in origin and character. Then, too, the whole body has a very efficient blood supply, by means of which the blood is brought into close proximity with the lutein cells.

Many workers have remarked that the ovary, apart from the fact that it is the seat of origin of the female reproductive cells, is a structure of great importance in the metabolism of the animal as a whole. Knauer (10) indeed, as the result of a long

[1] This is also true of a number of other Marsupials; *vide* (16A).

series of experiments, came to the conclusion that it is only so long as there is a functional ovary present, that is one that is capable of producing ripe ova, that it influences the general metabolism of the body.

To Prennant (17), in 1898, belongs the honour of being the first to suggest that the corpus luteum was an actively secreting gland of the variety we now call ductless glands, *i.e.* a gland in which the secretion, instead of being conveyed to a definite place by a duct, is transferred from the cells of the gland into the blood stream. Three years later Regaud et Policard (18) demonstrated that by means of a special method of staining, specific droplets of secretion can be found in the lutein cells. These droplets are coloured readily by Weigert's stain, and although similar to are yet different from fat globules. Cohn (5), again, working on the ovary of the rabbit, found that the Plessen-Rabonowicz method of staining brought out in the lutein cells vesicles surrounded by a kind of capsule and containing a substance resembling fat but not identical with it. Other authors have described an osmophile substance in these cells, and, again, granules of a doubly refracting substance.

As a result of chemical analysis various substances are stated to be present, a substance *sui generis*, lutein, cholesterin, or again lipoids with or without phosphorus, and also ethers of cholesterin. In his latest paper Van der Stricht gives a full review of the chemical aspect of the problem and the reader is referred thereto for further details.

The evidence available is strongly in favour of regarding the corpus luteum as a ductless gland that produces a specific secretion which may be a lipoid or a mixture of lipoids.

Thus we see that in the mammalian ovary at recurring periods a highly specialised glandular structure producing a specific secretion is developed which, after a period of activity very short compared with the life of the animal, disappears only to be replaced subsequently by another similar body. Further, it is noteworthy that it is present at those times when either the animal becomes pregnant or there is a possibility of pregnancy. In other words it appears immediately after ripe ova have been discharged from the ovary. It is perhaps not unnatural therefore that we should look for some connection between it and the changes marking the beginning of pregnancy. The extent and nature of these connections will now be considered.

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The Functions of the Corpus Luteum

Various views have been advanced as to the functions of this gland, and it is proposed to deal briefly with some of the older ones before passing to the two most recent.

The first is that the corpus luteum forms a plug of tissue to fill up the cavity of the ruptured follicle and so compensate for the disturbance in the circulatory system caused by the rupture. A second view, really a modification of the former, is that it provides a soft tissue which favours the growth of the succeeding follicles and allows of the regrowth of blood vessels instead of leaving hard fibrous scar tissue. These views have met with little support. Although just after rupture the corpus luteum is smaller than the follicle, it ultimately grows and becomes a great deal larger and also possesses an elaborate and highly specialised histological structure. Again, no blood vessel with the structure of an arteriole or venule is ever found in the corpus. In view of these facts the foregoing explanations of its functions are obviously insufficient.

Another theory that has received a considerable amount of support owes its origin to Beard (3). In an interesting paper in 1897 he suggested that the function of the corpus luteum was to suppress ovulation during pregnancy by causing the degeneration of the nearly ripe follicles and retarding the maturation of the others. He stated also that it disappeared prior to parturition in order to allow of ovulation at that time, but it only effected a temporary suppression in the absence of pregnancy, hence the difference in duration between the corpus luteum verum and the corpus luteum spurium. In a large number of animals, however, the corpus luteum has not disappeared before parturition. The suggestion is open to several other criticisms, however. Follicles may mature in an ovary that has large and active corpora lutea. The rabbit can ovulate shortly after parturition, indeed by some observers it is stated to do so spontaneously at that time, but well-marked corpora lutea are still present in the ovary. Again, in the rabbit and sometimes also in the sheep and ferret and probably in other mammals the follicles do not burst spontaneously, and in the absence of copulation degenerate on their own account without ever coming under the influence of a corpus luteum. In certain mammals only one batch of follicles reach maturity during the breeding

season, so that only one heat period or œstrus occurs instead of a succession of such periods as in the higher mammals; these animals are termed monœstrous. Monœstrous mammals have a long quiescent period, in some cases lasting about eleven months, and the corpora lutea have disappeared months before another set of follicles starts to mature, and so the inhibitory influence throughout this period cannot be regarded as due to the corpus luteum. As then follicles can mature, while corpora lutea are present and in the absence of such bodies the follicles in some cases degenerate and in others do not mature, this theory of Beard is also unsatisfactory.

Loeb (12) investigated this point in guinea-pigs. He first ascertained the time elapsing between two successive ovulations, the first following coition and the second spontaneous, and found that in no case was it less than fifteen days and in the majority it was more than nineteen. A second series, in which the corpora lutea were extirpated during the first day of their growth, was examined, and it was found that after sixteen days all those in which the extirpation had been complete had ovulated. On the other hand, in a certain number of cases in which the extirpation was incomplete and some of the corpus luteum tissue was left, ovulation had not occurred eighteen or twenty days after. It has been claimed that these experiments support Beard's hypothesis that the function of the corpus luteum is to prevent ovulation. What they do show, however, is that the removal of these structures tends to hasten the next ovulation. The criticisms advanced above apply also in this case and in the monœstrous mammals a suppression of ovulation takes place without the intervention of corpora lutea. So that although it may be said that the presence of a corpus luteum tends to retard ovulation it cannot be claimed that this is its main function. .

Two hypotheses now remain, which, in the light of our present knowledge, appear more satisfactory, and it is proposed to deal with them separately.

UTERINE CHANGES IN EARLY PREGNANCY

The first concerns the changes in the uterus during the first stages of pregnancy, and owes its origin to Gustav Born. It is that the corpus luteum provides an internal secretion which assists in the attachment of the embryo to the lining of the

uterus, *i.e.* to the uterine mucosa. The experimental examination of this hypothesis was first made by Fraenkel and Cohn (6), who found that the removal of both ovaries from rabbits at various times during the first six days of pregnancy brought about its cessation. Since then a number of observers have conducted similar experiments on various animals, dogs, rats, and guinea-pigs. Although different times were obtained in the separate species, all the results confirm that for rabbits, namely, that if the two ovaries be removed during the first part of pregnancy, it is invariably stopped. Removal of the ovaries at a later stage did not have any effect.

It is clear, then, that the presence of, at any rate, one ovary is absolutely necessary to the implantation and maintenance of the embryo during the early stages of that process.

These investigations were pressed still further, and Fraenkel (7) tried the result of removing the corpora lutea by electric cautery. Control animals were employed in which some of the corpora lutea were left untouched. It was found that the complete removal of all the corpora lutea from both ovaries resulted in the termination of pregnancy if performed within the first six days, whereas, so long as some of these structures were left, pregnancy as a general rule pursued its normal course. Similar experiments have been repeated by other workers, and the results fully confirm the previous ones.

Further evidence of a less direct nature, but also bearing on this point, is forthcoming. It is found that in the rabbit, if ovulation be provoked by sterile coitus, the formation of corpora lutea takes place, and simultaneously there occur an enlargement and vascularisation of the uterus. After the thirteenth day repression begins in the uterus, and by this time also the corpus luteum is on the wane.

The placenta, by means of which the embryo is attached to the uterine wall, is composed of two parts, one of embryonic and one of maternal origin. Loeb (13) found that he could produce deciduomata, the maternal part of the placenta, by making a series of transverse cuts in the uterus, by injecting paraffin wax, and by inserting pieces of glass or platinum. These structures, however, could only be produced from one to ten days after ovulation, that is, only during the time that young, active corpora lutea are present in the ovary, but can be produced during that time even though the discharged ova

be excluded from the uterus. As the uterine growth did not take place in the absence of mechanical stimulation, it appears as if the formation of the maternal part of the placenta is due to two causes: Firstly, the presence of corpora lutea in the ovary conditions the possibility of its formation, and secondly, a mechanical stimulus, supplied normally by the fertilised ovum, calls forth the response.

The experimental evidence therefore shows that there is an intimate connection between the corpora lutea and the early uterine changes. The nature of this connection will now be discussed.

During the above inquiry into the production of deciduomata it was noticed that if the corpora lutea were all removed the first day or two after ovulation no response could be obtained. If they were removed six days afterwards the response, although obtained, was not so marked as if the ovaries were left intact, so that the effect of the corpora lutea is cumulative. Again, portions of the uterus transplanted to the sub-cutaneous tissue also produced deciduomata if the transplantation was carried out from five to seven days after ovulation. If when the uterine tissue was transplanted the ovaries were also extirpated, it was found that the response was not nearly so marked as if the transplantation alone had been effected but the ovaries not interfered with. These transplanting experiments appear to exclude the possibility of the stimulation being nervous in nature, and the fact that it is cumulative, even when the uterus is removed to another part of the body, suggests that the stimulus is a chemical one carried by the blood.

To sum up briefly, then: The presence of a corpus luteum is essential to the uterine changes connected with the implantation of the ovum ;and its maintenance during the early stages of pregnancy. Under the influence of the corpus luteum the uterine mucosa becomes so sensitised that it will form the maternal part of the placenta in response to a certain stimulus normally provided by the fertilised ovum, but which may be replaced by a mechanical one. Some evidence is also available to show that the stimulus is a chemical one carried by the blood.

THE MAMMARY GLAND GROWTH IN EARLY PREGNANCY

The second of the two hypotheses concerns the relation existing between the corpus luteum and the mammary glands.

The credit of being the first to conduct "an experimental inquiry into the factors which determine the growth and activity of the mammary glands" belongs to Lane-Claypon and Starling (11), who in 1906 tried the effect of the injection of extracts of ovaries, placentæ, and fœtuses. They came to the conclusion that the growth of the mammary gland during pregnancy is due to the action of a specific chemical stimulus produced in the *fertilised ovum*. Similar experiments have been repeated without a uniformity of the results.

These authors further state that the source of the stimulus during early pregnancy may be located in the chorionic villi, *i.e.* a part of the placenta. Halban (8), who gathered a great deal of clinical evidence, also considers the placenta as the tissue in which the chemical stimulus inciting growth in the mammary glands is produced. This view, however, is open to criticism. In *Dasyurus*, a Marsupial (14), the main growth of the glands has occurred before the attachment of the embryo, which does not take place until late, and therefore before the formation of the placenta. In the Monotremes, the egg-laying mammals, of course no placenta is ever formed: although in both these cases there is a growth of the mammary glands. A full functional development of these glands may be experienced by virgins, and therefore without any stimulus from a fœtus. In hunting kennels also it is not very uncommon for a bitch that has failed to become pregnant to experience such a growth of the breasts that she is able to suckle the pups of another mother who for some reason or other is unable to do so herself. In *Dasyurus* the mammary glands in the non-pregnant female undergo a growth identical with those in the pregnant female, and reach a state of development comparable with that in the mother thirty-six hours after the birth of the young.

Although it is not improbable that the presence of a fœtus and its attachment may influence the very great growth and activity of the mammary gland in the pregnant female, the foregoing facts show that neither fertilised ovum nor placenta is necessary, and therefore neither of these can be regarded as primarily responsible for the production of the stimulus. Halban himself, although looking to the placenta in pregnancy, admits that at puberty, at the menstrual periods, and in pathological cases, that is to say, cases of abnormal growths in the ovary, it is in the ovary that the stimulus inciting growth in the mammary

glands is produced. It is hard to see why if the ovary may be effective at one time it should not also be effective at another.

Several authors have pointed out that the removal of both ovaries before puberty prevents its onset with the accompanying growth of the mammary glands. It is well known too that there is a growth of these glands at puberty and again at each œstral period. In connection with the latter an interesting case has been cited of a woman with supernumerary mammary glands which not only enlarge but actually secrete milk at each œstral period. All these enlargements of course synchronise with periods of ovarian activity which include ovulation and the formation of corpora lutea.

In the course of their experiments Lane-Claypon and Starling noted that if the ovaries and uteri were removed from a rabbit before the fourteenth day of pregnancy the mammary glands return to a state of rest without giving milk. If the same operation were performed after that time milk was expressible within two days, that is to say, the glands had reached a stage of development at which they could become functionally active. But the corpora lutea have also reached their maximum growth by the fourteenth day and after that time begin to diminish.

All these points, considered in the light of what is already known of the connection between the uterine changes and the corpora lutea, suggest the probability of a relation between these bodies and the mammary glands.

Turning now to consider the condition in the Marsupial, *Dasyurus viverrinus*, we find that the adult resting gland consists of six main ducts lined with an epidermis four or five cells thick. Some distance below the epidermis each duct breaks up into a number of branching tubules, lined with an epidermis two cells thick. The growth of the gland falls into two distinct phases. Firstly, the stage of actual formative growth during which the glandular cells lining the mammary tubules increase rapidly by mitotic division and in which the total number of cells is increased many times. As a result of this growth the mammary tubules become much more ramified and throughout their length hollow bud-like outgrowths, the primitive alveoli, are formed. Towards the end of this period the lumina of the tubules and their outgrowths begin to enlarge and the cells lining them begin to arrange themselves in a single layer. Secondly, a stage of enlargement, during which the

epithelial cells no longer multiply and mitoses are absent from them, but each individual cell increases markedly in size and commences to secrete. The cells of alveoli and tubules become arranged in a single layer, and as a result of their secretory activity the alveoli and ducts become greatly distended. It is this second stage that is the more noticeable in external examination.

An interesting correlation is seen between the formation and growth of the corpus luteum and the growth phase of the mammary glands. As soon as the corpus luteum is formed the mammary gland starts to grow. This growth is noticeably increased after the body is fully formed, a stage characterised by the presence of plentiful granules in the lutein cells. Later the corpus luteum reaches its maximum and remains constant, and shortly after the cells of the mammary gland cease to multiply. This correlation, which holds good whether the female be pregnant or not, suggested, taking into account the other evidence, that it was very probable that the corpus luteum, in addition to its other functions, is intimately connected with if not indeed the point of origin of the stimulus inciting growth in the mammary glands.

Other evidence regarding the relation of the two structures was already in existence when the foregoing conclusion was independently arrived at. Ancel and Bouin (2) showed that if corpora lutea are produced in the rabbit, either by copulation with a male previously rendered sterile or by artificial rupture of the follicle, a growth of the mammary gland follows. This growth, very noticeable about the fourth day, ceases about the fourteenth day and regression then takes place.

Similar experiments have been performed (16) which confirm these results in their essential points. It was found, however, that the rupture of the ripe follicles was not invariably followed by the formation of corpora lutea, but this provided a strong piece of negative evidence. The results may be summarised as follows. 1. If the rupture of the follicle was followed by the formation of corpora lutea there was also a growth of the mammary glands. The amount of growth in fourteen or fifteen days was about equal to that in the normal pregnant animal of twelve days. 2. On the other hand, if the follicular rupture was *not* succeeded by the formation of corpora lutea, there was *no* growth of the mammary glands, although the operation performed was precisely similar in the two cases.

As in the case of the uterine changes, so also here the experimental evidence points strongly to a connection between the corpora lutea and mammary gland growth. Again also the facts indicate that the connection is a chemical one.

The nerves supplying the mammary gland in the goat were cut without interfering with the growth of the gland during pregnancy or consequent lactation. In ,the dog the spinal cord was severed above the point of origin of the ovarian nerves, and a similar lesion resulting from an accident is also recorded in a woman, and in neither case were the growth and activity of the mammary gland interfered with. These seem to exclude the possibility of any nervous stimulation. In the guinea-pig,. however, single mammary glands have been transplanted to the subcutaneous tissue behind the ear without the growth during pregnancy being interfered with, so that the stimulus. in these cases must have been a chemical one carried by the blood.

The use of the term hormone has been carefully avoided because as yet no definite evidence has been adduced to show that the corpora lutea produce a specific secretion which when poured into the blood stream directly influences the mammary glands. As has been indicated above, the presumptive evidence is strongly in favour of such a direct chemical stimulus, but the experiments of injecting corpus luteum extract that have been tried up to the present have yielded only negative results. This. failure, however, may simply be because the technique was at fault.

It has been attempted in the foregoing to set out the evidence now available to show that the corpus luteum is a well-marked,. glandular body with a very definite and characteristic histological structure that periodically forms and disappears in the mammalian ovary—that it secretes a substance of lipoid nature. While present in the ovary corpora lutea may retard subsequent ovulation. Its principal work appears to be that by means of a chemical stimulus acting directly or indirectly it controls the uterine changes necessary for the attachment of the embryo in the early stages of pregnancy and also incites the formative growth of the mammary glands during the same and at other times.

The corpus luteum, then, is a gland that is present in a well-developed condition in the three sub-classes of the Mammalia,.

and we must conclude therefore either that it took its origin *de novo* in that class or that its representative is to be found in lower classes of the Vertebrata. Little is known with certainty concerning the follicular changes following ovulation in the lower vertebrates, for the accounts given by the few workers who have investigated these phenomena do not agree. Wallace (23) describes an enlargement of the epithelial cells accompanied by a more or less marked invasion of the connective tissue and blood vessels in certain fish, although according to some preceding workers no such hypertrophy occurs. It is also stated that a somewhat similar series of changes takes place in certain reptiles, and so here perhaps we may have the morphological forerunner of the corpus luteum.

For the origin of its two main functions described above we shall have to search within the limits of the class Mammalia itself, for both are concerned with processes that are characteristically and exclusively mammalian. Indeed the formation of a placenta is a process that itself originates among the mammals, for it is not found in the lowest sub-class, the Monotremata. These animals (Monotremes) possess corpora lutea, however, which therefore furnish an example of a gland taking on a further and new function within the same class of animals. Moreover, the mammary glands are found in all three sub-classes of the Mammalia and are so characteristic that from their presence the name of the whole class is derived.

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THE INFLUENCE OF THE SCIENTIFIC MOVEMENT ON MODERN POETRY

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" POETRY," says Leigh Hunt, "is the utterance of a passion for truth, beauty, and power, embodying and illustrating its conceptions by imagination and fancy, and modulating its language on the principle of variety in uniformity. Its means are whatever the Universe contains; and its ends, pleasure and exaltation. Poetry stands between nature and convention, keeping alive among us the enjoyment of the external and the spiritual worlds; and, next to Love and Beauty, which are its parents, is the greatest proof to man of the pleasure to be found in all things, and of the probable riches of infinitude. . . . Poetry," he continues, " begins where matters of fact or of science cease to be merely such, and it exhibits a further truth; that is, the connection science has with the world of emotion, and its power of producing imaginative pleasure. Inquiring of a gardener, for instance, what flower it is we see yonder, he answers, 'a lily.' This is a matter of fact. The botanist pronounces it to be of the order *Hexandria monogynia*. This is a matter of science. It is the 'lady' of the garden, says Spenser; and here we begin to have a poetical sense of its fairness and grace. It is 'the plant and flower of light,' says Ben Jonson; and poetry then shows us the beauty of the flower in all its mystery and splendour." This was written some eighty years ago, when science was in its infancy, or rather in that embryo stage in which all science is purely descriptive, but it shows us clearly enough that, even in those early days of scientific thought, the best literary minds of the time saw clearly that in matters of poetry, as in matters of fact and of science, we see "the same feet of nature treading in different ways"; that the most scornful and dullest disciple of fact should be cautious how he betrays the shallowness of his philosophy by discerning no poetry in its depths. It is indeed

not too much to say that science and poetry are twin sisters, so intimately bound up are they each with the other.

That there is some real connection between science and poetry—some direct and real influence of what we' call the scientific movement upon poetry—is easily discernible and becomes very evident when we remember, as we must, that although the poet, like all genius, is born, not made, that is, although genius is largely independent of time and place, he is nevertheless and necessarily, and to no inconsiderable extent, a product of his age, both as regards form and content. For example, Mrs. Browning, Tennyson, Robert Browning, Matthew Arnold— to take a few at random of the greatest of the Victorian poets— have indelibly impressed the poetry of their era. But it must not be forgotten that their greatness—even though it be the greatness of genius—does not alter the fact that what they thought and what they wrote was in large measure determined for them by the circumstances and ideas of the time in which they lived. This fact, though obvious, is often obscured in the minds of those people who talk of great men as being not of an age, but for all time. It is undoubtedly true that great poets, philosophers, artists, statesmen, and great men of all kinds, if they are only great enough, are for all time ; but it is equally true that they are, in a sense, of their own age first of all. Shakespeare is perhaps the first example, as well as the best, that comes to one's mind. His literary form, the atmosphere of his poetry—what one might call the mental dialect in which it was written; that is, the presuppositions which he carried with him to his work and which he owed entirely to his education and environment— many of his characteristic interests are Elizabethan. His morality, too, is essentially that of the age of the later Tudors —so much, indeed, is this the case that there are not wanting Mrs. Grundys at the present day who would willingly banish some of his plays altogether from the stage. Again, the drama was the most popular form of literature ; and he wrote mainly dramas. Subjects from English History possessed a special interest for his audience and had a special fascination for writers of his time ; and he wrote English historical plays. The fact, therefore, that Shakespeare is for all time does not prevent his being, in a very real sense, of an age, and of his own age. Similar considerations apply to all writers of every age and clime. However great a man may be, the world is

greater, and we can only hope to understand the man and his work in so far as we view him in his relation to the spirit and thought of his age. This being so, it is easy to see that the influence of science on poetry is not a mere figment of a perverted imagination, but is as real as it is lasting; for the spirit of the nineteenth and twentieth centuries was, and is, essentially scientific; and by "scientific" one must not be understood to mean "materialistic," which is a philosophical and not a scientific term, and so outside the scope of this article. By the scientific spirit is meant merely submission to the conception of universal law. The greatest intellectual triumph of the early part of the nineteenth century was the establishment of the two laws of the uniformity of Nature and of universal causation on a firm basis of experience, and the consequent elimination of the supernatural from natural phenomena. Such an intellectual revolution could not fail in leaving its mark upon contemporary literature, for the simple reason that it altered our whole conception of the relation of man to the Universe.

This influence has sometimes been deplored on the ground that science means the disappearance of mystery and of superstition and so takes away from the poet a great deal of his raw material. The idea is quite a wrong one. It is true, indeed, it is universally admitted that natural science has had the greatest possible influence in the extirpation of superstition; but this in no way limits the activity of the poet. The error is based on a confusion in thought. Superstition is not a necessary nor a permanent possession of mankind; it is but a phase—although an inevitable one—in human history. During the period of infancy—alike of the race as of the individual—in which the power of thought is but slightly developed, and the reason hardly more than a rudiment, the form given by this faculty of thought to the impressions of the senses is very imperfect and only superficially correct. But during his development man acquires a considerable knowledge of himself, and his knowledge of himself has, and must necessarily have, an immense influence on his comprehension of the world. He embodies all his feelings, his desires, his fancies into the sensible world, and imagines that everything around him is living, feeling, and desiring as he is. He, in fact, does and can only regard phenomena in terms of his own consciousness. He does the

same in religion. It was once said by a Frenchman—I forget his name—" In the beginning God created man in His own image, and since then man has returned the compliment by creating God in his." The jibe—for jibe it was—is really the expression of a fundamental truth : among all the lower and early religions, even as in our own, the conception of God is essentially anthropomorphic like primitive man's conception of Nature. This inner world, which man thus creates for himself, is a world of poetry and is very different from that which he afterwards acquires from his thoughts, but nevertheless this childish comprehension of the world is in peculiar harmony with things as they appeared to our ingenuous ancestor. We may say, in fact, that if poetry could be the prevailing sentiment in the world, the life of man would be one harmonious whole, but at the same time his comprehension of the world would be vague and dreamy. He would not be fully conscious, if at all, of the rational connection between phenomena. He has to be led, so to speak, by knowledge—that is, by science, which is only organised knowledge—to the point where thought and poetry will no longer be opposed. This intellectual development is forced upon man by the very constitution of Nature. Nature does not permit man to bury himself in a world of poetry and he is prevented from doing so by external influences ; objects intrude themselves which require his constant consideration. Irresistible impressions and thoughts appear in prominent distinctness, and oblige him to look at things in a new manner. This induces one of two opposite sensations : either joy and satisfaction at the new idea that he finds revealed to his ken, or discontent at the encroachment which has been made into his habitual view of the world. Either will have a direct influence on the expression of his thoughts and ideas in poetic form. It is the latter idea that dominates those who deplore the disappearance of mystery and superstition on the plea that it means a serious loss to the poet, and hence to the world. It is not so. The muse of the poet is the eternal beautiful—and, surely, truth is the highest expression of the beautiful ; as Browning has it :

> Ah ! world as God has made it ! Truth is beauty,
> And knowing this is love, and love is duty.

I think we may say, then, that the poet not only cannot, but

ought not even to attempt to, get away from contemporary life
and activity on the mistaken idea that he serves the eternal
beautiful; or, rather, under a misapprehension as to what the
eternal beautiful is. Whatever men think, do, suffer, hope is
the poet's theme. Poetry therefore must change with life and
grow with thought and will never suffer from a scarcity of
subject-matter until life and thought alike have disappeared. It
is true that only recently have we begun to awaken to this
conception of the function of poetry. In the middle of the
nineteenth century a materialistic anti-poetic movement swept
over the intellectual world to the great detriment of poetic
expression. One must always be thankful that such a move-
ment was transient, even evanescent, in its nature and in its
effects on poetry; but this movement again was an inevitable
phase in the intellectual development of mankind, and to it we
owe our present firm belief in the universality of law and our
rejection of the old theological ideas of the causeless, the
arbitrary, the capricious in the government of Nature. It arose
out of an attempt to correct the false perspective of previous
generations, and like all attempts at re-adjustment erred through
its very exaggeration; we find the same exaggeration to-day in
the ideas of such writers as Tolstoi and G. B. Shaw. We see
then that there are two ways of regarding the natural world—
first, as it appears to the bodily eye and to the normal untutored
imagination; secondly, as we know it actually is, having sought
out the truth of its phenomena, and the laws which underlie
their beauty or repulsiveness. The former, although purely
empirical, was formerly the raw material on which the poet
worked; the latter is due to that spirit of inquiry which we call
the scientific movement.

The materialism of the middle nineteenth century was due
to an attempted transition from one point of view to the other—
an attempt, however, which from its very violence carried the
intellectual pendulum far beyond its point of equilibrium into a
position in which it was as unstable as it was before. Thus
Huxley in a lecture on "Scientific Education" in 1869 deplored
the fact that "at present, education is almost entirely devoted to
the cultivation of the power of expression and of the sense of
literary beauty." The spirit of the conflict is aptly summed up
by another writer who says: " The truth is that our school-girls
and spinsters wander down the lanes with Darwin, Huxley, and

Spencer under their arms ; or if they have Tennyson, Longfellow, or Morris, read them in the light of Spectrum Analysis or test them by the economics of Mill and Bain." Such a conflict, however inevitable, was necessarily short-lived. Many of the greater minds of the time regretted it and looked forward to the day when reconciliation should come and " so far from being unfriendly to the poetic imagination, science will breathe into it a higher exaltation." The poets themselves recognised the necessity of the struggle, while looking forward with calmness, serenity, and certain hope to reconciliation and mutual help. The poets' feelings did not belie them—we are growing to recognise the fact that poetry is largely the expression of thoughts, ideas, feelings, many of which are founded and generated by science. The essays of Tyndall and Huxley are, the question of form apart, poems in themselves ; and there are both philosophers and poets who feel that no absolute antagonism can exist between them. The mission of science is to struggle against the unknown ; while in letters it is enough to give an expression, and in art a body, to the conceptions of the mind or the beauties of Nature. In other words, science kindles the imagination with new conceptions and new beauties which it has wrested from the unknown, and thus becomes the ally of poetry.

On the other hand, although, as has been pointed out, science is the ally of poetry, it must not be forgotten that poetry is, in no less real a sense, the ally of science. The intuition of the poet often anticipates scientific discoveries ; we see this in fiction in the novels of Victor Hugo, Jules Verne, and in those of H. G. Wells. In poetry a single example will suffice: when the theory of Evolution had been definitely established in science it was regarded merely as the process by which man and the Universe in general had arrived at their present condition, and inferences from it with regard to the future were put forward in a purely tentative manner, merely as suggestions containing perhaps a certain amount of speculative interest. The poet, however, with his prophetic insight seized on the theory and pushed it at once to its logical conclusion ; he

> Dipt into the future far as human eye could see ;
> Saw the vision of the world and all the wonder that would be ;

and many of his speculations are now accepted as beliefs by men of science, if not as scientists at any rate as thinkers.

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One must not forget,in passing, the independence of the poet—as the scientist has his own independent field of work, the investigation of the laws of Nature, so also has the poet. In his case it is the expression of the spirit of Nature. It is where these two spheres of activity—the investigation of the laws of Nature and the expression of the spirit of Nature—overlap that we find the common ground of science and poetry; but whereas science reaches it by an analysis of natural phenomena, poetry attains it by that direct intuition which is the poetic characteristic *par excellence*. Then again this common ground, where science and poetry meet and join hands, is an elusive thing, which though easy to recognise is by no means easy to describe without taking personal factors into account; it is so largely a matter of temperament. It has been said that " visible beauty exalts our emotions far more than a dissection of the wondrous and intricate systems beneath it. The sight of a star or of a flower, or the story of a single noble action, touches our humanity more nearly than the greatest discovery or invention could ever do and does the soul more good." The passage is a striking one and expresses a belief that is all too general; but though true in a sense it contains a confusion in thought which it is not difficult to point out. Although a noble action may appeal to us and to our human sympathy more strongly than the latest scientific discovery, say that of aërial locomotion, it is because its very nature is human, it pertains of the very essence of humanity; while the latter is something extraneous, without which the world would probably be as well off. The world was no less beautiful in the days before artificial means of locomotion came into being, but we can scarcely conceive a world so ugly, so repulsive, so utterly devoid of beauty that nobility of thought, word, and deed was unknown; we cannot conceive a humanity so inhuman as to be devoid of human sympathy and utterly unresponsive to nobility and beauty of thought and action. The former part of the argument, however, belongs to quite another plane, and I think one may say safely that "the sight of a star or of a flower" does *not* "touch our humanity more nearly," or anything like as nearly, as a " dissection of the wondrous and intricate systems " that lie beneath the object; or as an analysis of the processes underlying its growth and development; for by the very act of dissection or analysis new beauties and fresh wonders are revealed to our ken which far surpass those laid bare by a

mere sight of the object. The poets themselves realise this;
when Tennyson said,

> Flower in the crannied wall,
> I pluck you out of the crannies ;—
> Hold you here, root and all, in my hand,
> Little flower—but if I could understand
> What you are, root and all, and all in all,
> I should know what God and man is,

it was not the mere sight of the beauty of the flower that touched
him, but the thought that that same little flower was an integral
part, however small a one, in a more wondrous whole we call
Nature; it was the innumerable beauties laid bare by thought
and reflection founded on investigation and knowledge that
touched the poet's nature and enriched our language with a
poetic gem of such surpassing beauty; and it is the same know-
ledge in us, likewise based on a study of Nature's laws and
processes, that alone enables us to recognise its beauty.

But enough of the general nature of the influence of the
scientific movement on poetry. One comes naturally to
inquire into some of the more direct effects of scientific
ideas on the form and expression adopted in modern poetry.
Such an inquiry can only be carried out by an appeal to the
poets themselves and by a study of their works. For this
purpose we will take Tennyson as our chief example, since it is
better to make a detailed examination of one poet than a diffusive
and cursory glance at many. Moreover, besides being one
of the greatest and one of the most typical of modern poets,
Tennyson was a scientific observer of no mean order and his
scientific knowledge, if not profound, was at least exact and of
unusual width; while he enjoyed the extra advantage (for our
present purpose, at any rate) of being a friend of Darwin's—of
all modern scientists the one most deficient in, I had almost said
devoid of, poetic feeling; a defect which no one deplored more
than he did himself.

Perhaps the first real scientific idea introduced into poetry
was the idea of vastness—vastness of space, and later still the
vastness of time. One might at first think that the conception
of mere immensity is, emotionally speaking, a barren one; it is
not so in reality. Who does not know, who has not felt the awe
and wonderment, the subdued reverence with which we gaze up
at the starry heavens in the darkness and silence of the night!

How insignificant and even humble we ourselves are compelled to feel, even against our wills, when we realise how small a part we, and our earth on which we live, play in the totality of things! Or, to change the point of view, how happy we feel, what a quiet sense of pleased satisfaction we get when we realise that the part we play, even though it be so small, is an essential one and that we form an integral part of that scheme of things we call the Universe. This sense of awe is admirably expressed by Byron in his dramatic poem *Cain* ; Cain is being borne through space by Lucifer and is overcome with awe as millions of stars seem to flash past him and he loses sight of earth—

> O thou beautiful
> And unimaginable ether! and
> Ye multiplying masses of increased
> And still increasing lights! what are ye? What.
> Is this blue wilderness of interminable
> Air, where ye roll along, as I have seen
> The leaves along the limpid streams of Eden?
> Is your course measured for ye? Or do ye
> Sweep on in your unbounded revelry
> Through an aërial universe of endless
> Expansion—at which my soul aches to think—
> Intoxicated with eternity?
> O God! O Gods! or whatsoe'er ye are!
> How beautiful ye are! how beautiful
> Your works, or accidents! or whatsoe'er
> They may be! Let me die as atoms die
> (If that they die), or know ye in your might
> And knowledge! My thoughts are not in this hour
> Unworthy what I see, though my dust is ;
> Spirit, let me expire, or see them nearer ;

Lucifer. Art thou not nearer? Look back to thine earth!
Cain. Where is it? I see nothing save a mass
  Of most innumerable lights.
Lucifer.   Look there!
Cain. I cannot see it.
Lucifer.   Yet it sparkles still!
Cain. That! Yonder!
Lucifer.   Yea.
Cain.   And wilt thou tell me so?
  Why I have seen the fire-flies and fire-worms
  Sprinkle the dusky groves and the green banks
  In the dim twilight, brighter than yon world
  Which bears them.

The idea of the vastness of space was first introduced into science by Copernicus and was afterwards extended by such

intellectual giants as Galileo, Kepler, and Newton. To the immensity of space was added the immensity of time by Darwin and his co-workers. Perhaps no scientific idea has been so fruitful of results in its effects upon philosophy and religion, for it was a discovery which at once caused a vague disquietude in the minds of men. The universe ceased suddenly to be homocentric. Man seemed to become at once utterly insignificant; a mere speck of animated dust; a parasite of one of the meanest of the planets. As Tennyson says:

> What are men that He should heed us? cried the King of sacred song;
> Insects of an hour, that hourly work their brother-insects wrong,
> While the silent Heavens roll, and suns along their fiery way,
> All their planets whirling round them, flash a million miles a day.

And again, in that awful, gloomy, pessimistic poem *Despair*, he says:

And the suns of the limitless Universe sparkled and shone in the sky,
Flashing with fires as of God, but we knew that their light was a lie—
Bright as with deathless hope—but however they sparkled and shone,
The dark little worlds running round them were worlds of woe like our own—
No soul in the heaven above, no soul on the earth below,
A fiery scroll written over with lamentation and woe.

And yet again, in a more cheerful vein, he writes:

> For tho' the Giant Ages heave the hill
> And break the shore, and evermore
> Make and break, and work their will;
> Tho' world on world in myriad myriads roll
> Round us, each with different powers,
> And other forms of life than ours,
> What know we greater than the soul?

The immensity of time, too, is realised:

> Many an æon moulded earth before her highest, Man, was born,
> Many an æon too may pass when earth is manless and forlorn.

It is small wonder indeed that the greatest minds have been frightened and have recoiled in blank dismay from the conception of such immensity as this! "Man began to wonder how far he could still maintain moral laws and ideals of life formulated under other and different conditions, conditions when he was able to regard himself not only as the centre but as the

object of creation. 'The heavens declare the glory of God and the firmament showeth His handiwork,' said the Psalmist; 'the heavens declare no glory but that of Newton and Kepler' seemed to be the conclusion of modern science; and Laplace, in his great treatise *Mécanique Céleste*, admitted that in his system he could find no place for a God." The situation has been aptly summed up by Haeckel when he said, "We have learnt to look upon the sun shining out of a godless heaven upon a soulless earth." It is quite evident that our apprehension of man's littleness and the greatness of the Universe has a disquieting effect upon the human mind and tends to point out the futility of moral effort and the absurdity of mental speculation.

All these questions and problems were raised and suggested by this one scientific idea. The answers, if answers there are, must be sought in philosophy and in the works of the poets; this is neither the place nor the time for such philosophical disquisitions—the object of this article is to point out that such questions were raised by science; we must go to the poets for an answer.

The next idea which has been introduced by science is the conception of law and order in the Universe. This question has already been touched upon, so we may pass it over here, and proceed at once to a consideration of its greatest offspring— that master thought of the nineteenth century the idea of Evolution. This idea, more perhaps than any other scientific conception, has had most influence on modern thought; it, more than anything else, has altered man's outlook on life. The reason is obvious—it revolutionised men's conception of life, and their chief interest, whether scientist, poet, or philosopher, must necessarily be life. It meant the substitution of a dynamic for a static conception of the Universe; it meant the replace- ment of the idea of a product by the idea of a process. The idea itself is by no means the exclusive property of science; on the contrary, the earliest, widest, and most satisfactory expression of it was given in philosophy. Nevertheless, the idea would not have had a tithe of the influence it now possesses if it had not been put on a firm scientific foundation by Darwin and his successors.

It is really very difficult for us to realise fully the whole effect of Evolution on modern thought; we are so used to the idea, having grown up, as it were, in its shadow, that it has

almost become a part of our mental constitution; it has become one of the presuppositions which the human mind carries with it in its onward march. We can scarcely imagine the intellectual outlook of people, including the poets, who lived before the inception of this idea. We have to imagine an intellectual atmosphere in which the law of the uniformity of Nature and the law of universal causation were only accepted to a very limited degree. Law itself was hardly more than partially recognised. Theories of the special creation of species held general sway, and immutability, rather than mutability, was regarded as their main characteristic. In man the idea of development, of social and intellectual progress, was of academic rather than of practical interest. Many, indeed, believed that retrogression had set in; that the highest attainment of humanity had occurred in some Golden Age of the past. If there was any social progress at all it was looked upon as the result of an artificial social machinery; the idea that that social machinery itself was the result of a natural development of the race was only dimly perceived, if at all. Evolution changed all this. Society must be regarded as continuous from age to age—it is an organism, not a manufacture. The idea of the individual being "the heir of all the ages" was seen to be merely the expression of a fact. Moreover, the philosophy of development is essentially a hopeful one—it finds for a large amount of pain and evil a place and a significance more satisfactory to the reason than most of the arbitrary theological explanations of previous generations, and affords a natural incentive to moral effort.

Of all modern poets Tennyson was the one who perhaps made most use of this conception,[1] though all were under its influence. In fact Tennyson's principal point of contact with science was his acceptance of evolution as a fact. All his philosophical and nature poetry is written from this point of view. His most emphatic references to evolution are in the two poems *Locksley Hall* and its sequel. In *Maud* too we find it—

He (man) felt himself in his force to be Nature's crowning race.
As nine months go to the shaping an infant ripe for his birth,
So many a million of ages have gone for the making of man;
He now is first, but is he the last? is he not too base?

[1] For a more extended treatment of this part of the subject see Mr. Masterman's book on Tennyson, to which I must acknowledge my indebtedness.

And again in *The Princess* :

> This world was once a fluid haze of light,
> Till toward the centre set the starry tides,
> And eddied into suns, that wheeling cast
> The planets : then the monster, then the man ;
> Tattoo'd or woaded, winter-clad in skins,
> Raw from the prime, and crushing down his mate ;
> As yet we find in barbarous isles, and here
> Among the lowest.

This evolutionary faith runs all through Tennyson's works; *In Memoriam* is permeated with it. In one part of this poem he speaks of the succession of types in Nature and speaks of a gradual development from age to age, man being but an intermediate link in the chain of progress to higher and higher types :

> Star and system rolling past,
> A soul shall draw from out the vast
> And strike his being into bounds,
>
> And, moved thro' life of lower phase,
>   Result in man, be born and think,
>   And act and love, a closer link
> Betwixt us and the crowning race
>
> Of those that, eye to eye, shall look
>   On knowledge ; under whose command
>   Is Earth and Earth's, and in their hand]
> Is Nature like an open book ;
>
> No longer half-akin to brute,
>   For all we thought and loved and did,
>   And hoped and suffered is but seed
> Of what in them is flower and fruit.

It is significant that though this was written before the *Origin of Species* was published, the ideas expressed are practically identical with those in Prof. Ray Lankester's Romanes Lecture on "Nature and Man"—the latest word that science has yet said on man's position in the Universe. After the appearance of the *Origin* Tennyson's grasp of the principle of Evolution became much firmer. Henceforth two main points in the theory seem to have struck him with special force. One was the slowness of the change combined with the fact that though slow there seems to be no logical limit to its power;

so that man may develop into something much higher than he is—

> Man as yet is being made, and ere the crowning Age of ages,
> Shall not æon after æon pass and touch him into shape.

Or, on the other hand, he may pass away altogether:

> Many an æon moulded earth before her highest, Man, was born,
> Many an æon too may pass when earth is manless and forlorn.

The other point that seems to have struck Tennyson was the possibility of a reversion. This becomes much more marked in his later poems in which he looks back on life; they are naturally less optimistic than the earlier ones. In the earlier poems he regards man as being an intermediate link in a chain of an ever-progressing development. Later he is less confident, and his lack of confidence is due to his wider experience and completer knowledge of the complex relationships of life. In *Locksley Hall, Sixty Years After*, we find this conception of reversion quite clearly stated:

> Evolution ever climbing after some ideal good,
> And Reversion ever dragging Evolution in the mud.

He then goes on and gives the idea in its full significance:

> All the full-brain, half-brain races, led by Justice, Love, and Truth;
> All the millions one at length, with all the visions of my youth?
>
> All diseases quench'd by Science, no man halt, or deaf, or blind;
> Stronger ever born of weaker, lustier body, larger mind?
>
> Earth at last a warless world, a single race, a single tongue—
> I have seen her far away—for is not Earth as yet so young?
>
> Every tiger madness-muzzled, every serpent passion-killed,
> Every grim ravine a garden, every blazing desert till'd,
>
> Robed in universal harvest up to either pole she smiles,
> Universal ocean softly washing all her warless isles.
>
> Warless? when her tens are thousands, and her thousands millions, then—
> All her harvests all too narrow—who can fancy warless men?
>
> Warless? war will die out late then. Will it ever? late or soon?
> Can it, till this outworn Earth be dead as yon dead world the moon?

And this reversion, he fears, has even now set in, for he continues—

> Is it well that while we range with Science, glorying in the Time,
> City children soak and blacken soul and sense in city slime?

There among the gloomy alleys Progress halts on palsied feet,
Crime and hunger cast our maidens by the thousand on the street.

There the master scrimps his haggard sempstress of her daily bread,
There a single sordid attic holds the living and the dead.

There the smouldering fire of fever creeps across the rotted floor,
And the crowded couch of incest in the warrens of the poor.

And yet in spite of all this he realises that it is a possibility only and no more, and that we are really quite ignorant as to the future—

Far away beyond her myriad coming changes Earth will be
Something other than the wildest modern guess of you and me.

Earth may reach her earthly-worst, or if she gain her earthly-best,
Would she find her human offspring this ideal man at rest?

On the whole, however, he agrees that Evolution instils hope into the human heart; his last word is one of exhortation, and he ends the poem by pointing out the necessity of hoping and striving:

Follow you the star that lights a desert pathway, yours or mine,
Forward, till you see the highest Human Nature is divine.

Follow Light, and do the Right—for man can half-control his doom—
Till you find the deathless Angel seated in the vacant tomb.

Forward, let the stormy moment fly and mingle with the Past.
I that loathed have come to love him. Love will conquer at the last.

Tennyson, as we have seen, kept pace with the advances of modern thought. He of all the poets made most use of the results arrived at by modern science without making his work at all prosaic, or anything other than the highest and the best. It is, in fact, this advance of Tennyson in keeping pace with the strides of modern science and modern thought that makes him the best example one can offer of the influence of science on poetry; from an examination of his work we can trace the development of his mind with increasing years and that knowledge, wider experience, and fuller understanding that advancing years alone can bring.

It has been said that Browning had a firmer grasp of the principle of Evolution, and that the science and philosophy of the time probably made a deeper impression on him than it did on Tennyson. Be this as it may, at any rate it is less apparent,

for Browning never advanced beyond the position taken up in his first really great poem, *Paracelsus*, which he published at the age of twenty-three. It is remarkable that although this poem was written a quarter of a century before Darwin's *Origin* was published, yet it contains what is perhaps one of the most precise, complete and satisfactory expressions of the principle of Evolution that has ever been put forward. As far as regards his position towards this theory and towards contemporary thought in general, his mind was as fully developed at this time as it was in any of his later poems, and his whole conception of the Universe was ruled by this one idea. Thus at the close of the poem, the speaker Paracelsus shows how God is immanent in all Nature and how finally all leads up to man; and yet how "man is not Man as yet," but must develop into something far higher and nobler. The whole poem is wonderfully conceived and still more wonderfully expressed; it is one of the wonders of the English language; one of those precious things of literature that humanity cannot afford to be without. It is also interesting from another point of view. It shows us that Browning arrived at the conception of evolution, not from science alone, but from the whole of contemporary thought, whereas Tennyson arrived at it mainly from the scientific side. It points out to us exactly the nature of any influence that science may have had on modern poetry. Science is not an extraneous thing which casts a halo, like some divine effulgence, over everything that comes within its influence. It is merely a mode of thought. It is one of the forms in which thought expresses itself. Philosophy is another, and so also to a large extent are poetry and art. All these are merely expressions of thought; merely forms in which is expressed man's outlook on life and on the Universe. As such they are bound to influence each other, to overlap, as it were, and collectively they represent that "spirit of the age" which we are so prone to objectify and make the standard by which we judge and are judged, and by which, to use Hegel's phrase, we "re-evaluate all values" as human exigencies demand. This "spirit of the age," which is thus in reality but another name for modern thought, is itself a product of the human mind—like science, poetry, and art—and must therefore change with progress. This brings one naturally to the idea of the relativity of human knowledge and the impossibility of setting up absolute standards. This

conception is a product of evolutionary science, in fact it follows from it as a necessary corollary and is perhaps the latest scientific idea that is old enough to have influenced poetry in any definite manner.

The more one thinks about it the more is one convinced that the scientific movement must necessarily from its very nature have had a profound and lasting influence on modern poetry. In tracing such influence one can only generalise, pointing out tendencies and directions that the thoughts of men—poets in particular—have taken. The real meeting-point of the poet and the scientist is in the imagination and the emotions of men. We have too long been accustomed to regard these as being the exclusive happy hunting ground of the poet and as being but a sterile desert to the scientific investigator except in so far as he regards them objectively as parts of that Nature which it is his function to study. This, in fact, was the feeling in the materialistic philosophy of last century, but it has happily given place to another. Science can play on the imagination and emotions of men to an extent scarcely inferior to that of poetry, and it is only by so doing that science can become and remain a living thing and of real and lasting interest to mankind.

CRITICISMS OF PSYCHICAL RESEARCH

I.—By J. ARTHUR HILL

MR. SHELTON'S paper in SCIENCE PROGRESS for January may perhaps give erroneous impressions regarding certain points in psychical research. Without in the least wishing to be censorious, or to adopt anything but the most friendly attitude, I venture to make a few remarks on the paper in question; following up those remarks with a review of the main features of the subject, in the hope that the interest of some few hitherto-indifferent men of science may be enlisted in the work upon which we are engaged. In the first and more critical part I will be as brief as possible, and hope that brevity will not be taken as discourtesy.

Mr. Shelton says, with commendable candour, that about psychical-research evidence he "knows little and cares less." He has read (some years ago) F. W. H. Myers's *Human Personality*, "that monumental volume," which as a matter of fact is two volumes, unless he means the abridged edition, which is not particularly "monumental," if "large" is the meaning intended. This, plus "common sense" and "some knowledge of psychology," represents his equipment for attacking a very distinguished man of science who has worked at psychical research—experimentally, and not merely by reading—for the last thirty years. It is usually found, in scientific and all other matters, that those who are ignorant of a subject are not capable of expressing wise opinions on it.

Mr. Shelton has found nothing in Myers's book or "elsewhere" which could "carry conviction to, or even merit serious consideration by, any one not naturally predisposed to form the spiritualist conclusions." Well, as to what merits serious consideration, that is a matter of individual opinion; but I wish to say that though I am not a spiritualist, and am not predisposed to form spiritualist conclusions (for I do not want a future life), I have nevertheless found in Myers's book, and elsewhere, very much that seems to me worthy of the most serious considera-

tion. Of course mere reading probably does not convince anybody. It certainly would not convince me. It is experiment, personal investigation, that is required. But if a man *will not* investigate—if he will persist in sitting in an armchair, reading books and complaining that they do not convince him—what can we do with him? We can only exhort him to be more scientific, to give up talking, and investigate for himself. I am reminded of a clerical acquaintance of mine who "could see no evidence for evolution." There are none so blind as those who *won't* see.

Mr. Shelton says that a well-known conjurer "has never yet failed to reproduce every phenomenon credited to 'spirits' that has been brought before him." A very wild statement! Mr. Maskelyne (who I suppose is meant) can do remarkable things, on his own stage and with all his concealed apparatus, but I am not aware that he has offered to reproduce the phenomena of Florence Cook in the house of the President of the Royal Society. I should like to see the conjurer who could produce a Katie King in my house (still more in the house of an F.R.S.), with half a dozen of my intimate friends present, with a good light, and the key of the locked door in my pocket.

As to telepathy, which is "not proven" (that is a matter of opinion, depending partly on what is meant by "proven"), we know well enough that it is a possible alternative (as regards some of the evidence) to the survival hypothesis, and that, if it is a fact, it *may* be material or etherial in its process. But I agree very cordially that it is "rash folly" to admit an ether-wave telepathy except as a mere guess—a guess, moreover, which the details of the evidence seem to render probably mistaken. Is it not equally rash folly for Mr. Shelton to say that, when telepathy has explained all it can, "the residuum ceases to be worth investigating"? No doubt this is so, to one who "knows little and cares less" about the subject. But there are others who think that even small residua do not cease to be worth investigating. Rayleigh and Ramsay discovered argon by following up the small residual difference between atmospheric nitrogen and nitrogen obtained from other sources. And as telepathy is "not proven," the spiritistic residuum is not proven to be small. It may turn out very large. It depends on the scope of telepathy. And this is a matter for investigation.

As to Mr. Shelton's suggestion that Sir Oliver Lodge should

"see what Rome has to teach him," because the Roman Catholic Church has had a lot of experience—well, I suppose this is a joke. Scientific method is a modern thing; the stringent psychical-research canons of evidence are only about thirty years old; the evidence of pre-scientific days does not come up to our standard. The Virgin of the Pillar, at Saragossa, is said to have restored a worshipper's amputated leg. Spanish theologians regard this as a specially well-attested case.[1] But the "evidence," such as it is, would leave an S.P.R. investigator unmoved, and I tremble to think with what ferocious joy the late Mr. Podmore would have hewed it in pieces before the Lord.

The Roman Catholic Church says that our investigations "are better not attempted." I rather think she has said something like that to every science in its turn. She tried to discourage Galileo—tried rather strenuously, we may remember, for there is some evidence (not conclusive) that he was put to the torture. Fortunately science has now won its freedom from ecclesiastical control.

Now to the more congenial positive side. First as to general considerations.

The question, "Does this or that alleged but not generally accepted thing really happen?" is to be answered by observation and inference. It is a question of evidence. No scientific man believes without evidence, but, on the other hand, neither does he say *a priori* that any alleged occurrence is impossible. J. S. Mill in his *Three Essays on Religion*, and Huxley in his *Hume* and elsewhere, sufficiently demolished the "impossibility" dogma. Says the latter, in *Science and Christian Tradition*:

"Strictly speaking, I am unaware of anything that has a right to the title of an impossibility, except a contradiction in terms. There are impossibilities logical, but none natural. A 'round square,' a 'present past,' 'two parallel lines that intersect,' are impossibilities, because the ideas denoted by the predicates 'round,' 'present,' 'intersect,' are contradictory of the ideas denoted by the subjects 'square,' 'past,' 'parallel.' But walking on water, or turning water into wine, are plainly not impossibilities in this sense" (p. 197).

In matters of alleged objective fact, it is a question of

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[1] Lecky's *Rise and Influence of Rationalism in Europe*, vol. i. p. 141.

evidence. The incomprehensible and incredible may turn out true, when we have learned more. The African king would not believe that water could ever take the form of solid lumps, for he had always seen it liquid. An elderly agricultural labourer said to a friend of mine a few years ago, concerning the alleged electric trams of the town : "Don't talk silly! How can they go without 'osses ? " The Greeks did not believe the circumnavigators of Africa when these latter said they had seen the sun in the north. Even so romantic an historian as Herodotus declined to accept such an obvious "traveller's story." " I for my part do not believe them," he says (*History*, book iv). Yet all these unbelievers were in error, because of their ignorance. They should have said : " I do not know ; I suspend judgment until I learn more." The lesson of scientific experience is that when a thing seems inexplicable, or when two theories clash, what is wanted is more investigation, more facts. The discovery of radioactivity has enabled physicists to concede the geologists' claim concerning the age of the earth ; indeed we now want more facts to help us to see why the earth is not hotter than it is ! Further knowledge always tends to fit things in, though until we see just where to fit them, the facts are naturally distrusted. The hypnotic trance was long looked on as a delusion of Elliotson's, and Esdaile's, and it was even hazarded that the Calcutta natives who underwent severe operations at the hands of the latter were *shamming* anæsthesia ! But the *a priori* objections of the ignorant had to give way before the hail of further facts. For example, Mr. Mayo Robson performed evulsion of the great toe-nail, and removal of part of the first phalanx (for exostosis) on a hypnotised patient of Dr. Bramwell's in Leeds, March 25, 1890, and no pain was felt. Another patient had sixteen teeth extracted : no pain, no corneal reflex, and the pulse slowed during the operation. About sixty medical men were present by invitation, to see these and other operations. Anæsthesia in the hypnotic trance of a good subject is now a medical commonplace. Explanation may not yet be fully attained, though we are as near it as we are to explanation of ordinary sleep ; but at least the system of orthodox science had to make room for the new facts. May it not be the same with other psychical phenomena ? I think it will.

Psychical research covers a wide field. It is rather un-

fortunate that the popular interest in " spirits " causes attention to be focussed on the Society's activities in the survival direction; for it is quite possible that its investigations in, *e.g.*, hypnotism, multiple personality, etc., if pushed farther, might yield data more important to our conceptions of human personality than the more immediately attractive phenomena of definitely spiritistic type. Anyhow, let me emphasise the fact that the Society exists to investigate, without prejudice, all apparently supernormal faculty, not merely those alleged phenomena which point directly to survival. The Society has no creed, except perhaps the belief that there is something worth investigating; and consequently no one has any right to speak for it as regards the conclusions reached—the facts and theories which are or are not established. Each member must speak for himself; and, as I am perhaps a fairly average member, half-way between Dr. Bramwell who does not believe in telepathy, and Dr. Ochorowicz who (after long and laborious investigation) has arrived at belief in various queer telergic and teleplastic phenomena, it may not be out of place if I indicate my own attitude towards the main features of the subject.

TELEPATHY

I believe that communication between mind and mind, through channels other than the known sensory ones, is a fact. My belief is based on the voluminous and carefully recorded evidence in the forty volumes of *Proceedings* and *Journal* S.P.R., *plus* my own experience. I have carried out long series of experiments with distant friends—not professional mediums, and not spiritualists—with impressive if not conclusive results. There is always a mixture of hit and miss in these experiments, and it is difficult to know how much to allow for chance coincidence. However, by using a shuffled pack of cards, and drawing one for each attempt, the chances can be mathematically determined. It may here be mentioned that, in a series carried out by Sir Oliver Lodge, the odds can be shown to be ten millions to one against the results being due to chance.[1] Mr. Shelton may say that telepathy is " not proven," but I think that in certain walks of life such odds as ten millions to one would indicate what I believe is known as a " dead cert."

[1] *Survival of Man,* p. 65.

The experimental evidence is the best adapted to exact statement and safe inference, but a provisional telepathic hypothesis is indicated—as the doctors say—by other phenomena such as are often observed in trance mediums and normal clairvoyants. Few if any serious investigators have remained unconvinced that some supernormal agency or mode of function is occasionally concerned in these curious happenings. Says the late William James, who investigated these things, off and on, for about thirty years, without accepting any particular theory:

"Knowing these trances at first hand, I cannot escape the conclusion that in them the medium's knowledge of facts increases enormously, and in a manner impossible of explanation by any principles of which our existing science takes account. . . . The trances I speak of (Mrs. Piper's) have broken down for my own mind the limits of the admitted order of nature. Science, so far as science denies such exceptional facts, lies prostrate in the dust for me; and the most urgent intellectual need which I feel at present is that science be built up again in a form in which such facts shall have a positive place."

That expresses the feelings of many of us.

Trance-Phenomena and Normal Clairvoyance

It is fairly common for a trance-control to give information about the sitter's deceased relatives, quite beyond what any amount of inquiry would account for. So long as the information given is within the knowledge of the sitter, telepathy is a possible explanation; and even if he has no conscious recollection of it, the knowledge may exist in his subliminal memory—where "forgotten" things go—and may be "telepathing" itself from that dim abode, or may be accessible to the medium's foraging mind. This is a permissible guess, but nothing more. Sometimes, however, evidential communications are received from *soi-disant* spirits who are quite unconnected with the sitter, and who were, indeed, unknown to him in life; and these messages have been verified by inquiry of the spirit's relatives, who did not even know of the medium's existence. This requires an extension of telepathy—if telepathy is urged at all—far beyond what experiment justifies. I cordially agree with Mr. Shelton about the rash folly of experimentally unsupported speculation

Premature guesses often retard discovery, by turning our eyes in wrong directions. I doubt very much if the telepathy guess is the true explanation of these cases.

And sometimes telepathy seems almost or quite excluded. A *soi-disant* spirit has been known to refer to something which, so far as could be ascertained, was known to no living mind, *e.g.* something written in an MS. note-book just before death, and not looked at by surviving relatives until the mediumistic communication came, alluding to the book and the entry as a test of identity.[1] Nevertheless, though telepathy seems excluded, this does not give us proof of the spiritistic hypothesis. There are several alternatives. It may be a case of *deferred* telepathy— *i e.* the person may have "telepathed" the information before she died, and the medium (a non-professional one) may have picked up its reverberations, or indeed may have received it at once and stored it up for later emergence. Or it may be that objects which have been handled and thought about by human beings somehow retain a sort of dim mentality or memory of their owner, and can afford information about the latter to any one possessing the necessary sensitiveness. Personally, I am convinced by my own experiments that something of the sort is true. A medium whom I have known for many years can describe living people, and can often give the most intimate details of their lives, by handling a lock of hair or a worn garment taken by some other person ; and the explanation is not telepathy from the sitter, for the evidence given often goes far beyond the sitter's knowledge. And if objects do thus carry some sort of memory, an old glove may enable a medium to produce any amount of evidence about its deceased owner. How it comes about, the medium does not know, nor do I. Perhaps dead people's memories stick together for a while, in the psychical world, without any self-conscious survival, as the physical body sticks together for a while in the physical world ; and the worn object may somehow tell the medium's subliminal where to cast its line to fish up some of these recollections. But this is only ingenious guesswork, devised as an alternative to "spirits." I state it in order that all sides of the question may be seen. We cheerfully admit that

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[1] *Proceedings S.P.R.*, vol. xvii. p. 183. The whole report (by Mrs. Verrall, classical lecturer at Newnham, and translator of Pausanias) should be read. It is an admirable example of what reports of this kind should be.

coercive proof is not possible—it never is in inductive science—and that alternative hypotheses may always be devised. No one need be afraid of having to believe against his will!

These trance-phenomena are closely paralleled by the "normal clairvoyance" of a medium well known to me for many years. This man, apparently quite normal, and certainly not in trance, will sometimes reel off correct descriptions and names of one's deceased relatives as fast as they can be taken down in short-hand; also intimate family details of the sitter's history which he could not have obtained by detective work; also, sometimes, things which the sitter did not know, and never had known—so far as he was aware—but which, on inquiry, turned out true. My friends and I have carried out long series of experiments with this medium, introducing strangers from distant towns—non-spiritualists, people with no interest in these matters—and devising various other tests. We began as unbelievers, but the facts beat us. Something out of the common is at work, of that we are sure. What it is we do not know. Perhaps it is partly telepathy, but some of the evidence seems to go beyond that.[1]

AUTOMATIC WRITING

Of late years the main interest of the Society has centred in the automatic scripts of certain persons, mostly of high social and academic position, and not spiritualists or mediums in any current sense of that objectionable and question-begging word, who receive messages purporting to come from the surviving minds of some former leaders of the S.P.R., notably Edmund Gurney, Richard Hodgson, F. W. H. Myers, and Henry Sidgwick. Once more we may say that much of this may be due to subliminal fabrication plus telepathy, so I waive the portion which is possibly thus explicable. But the more recent developments cannot be ascribed to any telepathy that I can believe in. In the cross-correspondences, fragmentary and incomprehensible messages came through Mrs. Verrall at Cambridge, Mrs. Holland in India, Mrs. Forbes in the North of England, and Mrs. Piper in America, but when the bits were pieced together by the Society's Research Officer, they were found to "make sense," and sense characteristic of the ostensible

[1] For details of this and other cases I may mention my *New Evidences in Psychical Research* (Rider, London) and *Spiritualism and Psychical Research* (T. C. & E. C. Jack's "People's Books").

sender. Here, then, is no mere sticking together of unconscious memories in a cosmic reservoir; for the evidence suggests intelligence, initiative, and will on "the other side." Admittedly, here again, the evidence does not amount to knock-down proof. It never can. We can always say, "there must have been some fraud or error somewhere," even though we cannot find it. Lavoisier had settled it in his own mind that there were "no stones in the sky," therefore the celestial origin of meteoric stones was palpably wrong. The scholastic philosopher had read several times through Aristotle, and had found no mention of sun-spots; therefore the astronomer who thought he saw them must be mistaken. We moderns are not yet emancipated from prejudice, but we may at least learn from such instructive examples that it is better to investigate for ourselves than to deny *a priori* the observations of others.

VERIDICAL SENSORY AUTOMATISMS

These are not producible to order, but they can be studied when they do turn up, like volcanic eruptions and earthquakes, which are similarly beyond our control.

It happens fairly often that when a person is undergoing some particularly stressful experience some friend at a distance becomes more or less aware of the fact by experiences varying from vague emotions to full-blown hallucinations. When a full record is written out and placed in the hands of some responsible person *before* verification, this constitutes evidence of something supernormal, particularly if the percipient had no reason to expect or imagine any occurrence of the kind, and was therefore not predisposed. Usually this signifies telepathy, and is sufficiently interesting as such. But sometimes it seems to signify more. It often happens that the apparent sender of the telepathic message is found to have *died* just about the time. And, of course, even if the hallucination (or whatever it may be) occurred after the time of death this would not prove survival, for it may be "deferred" telepathy, the dying person having sent his psychical wave-message out before dissolution. There may be cases, however, in which this seems unlikely, as when the death is so sudden that the individual has next to no time for thinking about it. If I see an apparition of my brother, with a bleeding wound in his right temple, and if it turns out

that he was killed by a bullet in the right temple a few hours before I saw the apparition, it certainly suggests the activity of his surviving mind. It does not prove it, for it is impossible to prove that death was instantaneous, which, indeed, it probably never is. He might, therefore, have time to send the message before death occurred. But such cases warn us to be careful about too airily disposing of everything of the kind by a reference to telepathy. The illustrative case just given actually happened to Captain Colt. He was in Scotland when he saw the apparition, and his brother was killed in Russia. Two more points are worth noting : (1) Captain Colt had asked his brother to let him know by "appearing" to him if possible should anything happen to him; (2) he saw the figure in a kneeling position, and that was the posture in which the body was actually found.[1]

The S.P.R. has made laborious collections of such cases, instituting, *e.g.*, a census in which 17,000 persons were questioned. The report of the Committee, which worked at the data for several years, concludes with the short but weighty statement that " between deaths and apparitions of the dying person a connexion exists which is not due to chance alone." This was signed, among others, by Prof. Sidgwick, who was— according to William James—" the most exasperatingly critical mind in England."

The Report, in vol. x of *Proceedings S.P.R.*, is worth careful study. All the possibilities of error that could be thought of by acute and experienced investigators were duly considered and allowed for. Some hasty critics have revealed their ignorance of the Report by advancing various elementary objections which the Committee had already exhaustively dealt with. *Verb. sap.*

PHYSICAL PHENOMENA

Of these there are various alleged kinds. Small objects— stools, chairs, tables sometimes—are said to move without discoverable application of force. Percussive sounds (" raps ") are said to be produced in some similarly inexplicable fashion. Objects are said to be brought from a distance (*apports*) by supernormal means, as when a bell appears in the *séance* room without any door having been opened—the bell being usually

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[1] Myers's *Human Personality and its Survival of Bodily Death*, vol. ii. p. 348.

located on a shelf in another room. In this particular case the experimenter at once went to the other room to investigate. His two boys were working there. He asked where the bell was. One of the boys looked up at the shelf, and said, astonished, that it was there a few minutes ago. The experimenter was Sir William Crookes, now President of the Royal Society, who also testifies to raps, movement of objects without contact, and materialisation. The medium who gave him the greatest range of results was D. D. Home, who, contrary to Browning's assertion, now disproved, was never caught in trickery, or anything like it. Sir William's materialisations, however, were mostly produced by the medium Florence Cook. All the experiments were carried out in Sir William Crookes's own house or that of a friend, and all the sitters were his close relatives or friends. Usually he did not decide which room to use for the *séance* until the last minute, so that preparation by a hypothetical trickster was rendered impossible. It is useless to discuss this evidence in detail, but any one who will read it with a really open mind will probably find it rather impressive.

The performances of the Rev. Stainton Moses seem to have equalled those of Home. Unfortunately, Mr. Moses gave sittings to his own friends only, and the evidence is therefore less good. But he was certainly a very highly respected man—a teacher of English for eighteen years at University College School, after throat trouble compelled relinquishment of his curacy—and no evidence of fraud or anything incompatible with complete integrity has ever been brought against him.

The most famous physical medium of modern times, however, is Eusapia Palladino, who is still living. For thirty years this Neapolitan peasant woman has provided material for the psychological *savants* of Western Europe to puzzle over. She was certainly caught tricking in the Cambridge sittings of 1895, held by Dr. Hodgson, Sir Oliver Lodge, Mr. Myers, and Prof. Sidgwick; also, perhaps, in some recent sittings in America. It is admitted, even by those who believe in her genuine powers, that she cheats sometimes, perhaps in a trance-state, which absolves her of moral responsibility. But it is a fact, on the other hand, that she had in 1894 convinced Sir Oliver Lodge, Mr. Myers, and Prof. Richet (the recent recipient of the 1913 Nobel prize for physiology) of her supernormal faculty. After the Cambridge sittings, Mr. Myers further confirmed his good opinion by some

more sittings in Paris. Most impressive of all (to those who, like Mr. Shelton's conjurer, have no opinion of F.R.S.'s), she completely upset the scepticism, in a series of eleven sittings at Naples in 1908, of three of the ablest investigators now living, two of them expert conjurers, and all of them old hands at the game of showing up fraudulent mediums.[1]

But, once more and for the last time, conclusions are not to be arrived at by proxy. We cannot get convictions second-hand. Each must investigate for himself. The mind is naturally inhospitable to statements alleging occurrences which have no parallel in its own experience. And this natural conservatism is a good thing. It saves us from superstition and foolish credence of various kinds. I greatly prefer excessive scepticism to excessive credulity, and should be sorry to think that any one believed these things on my authority. We do not expect to produce belief by our reports; we do not even wish to do so. The most that we expect or wish to do is to "modify the atmosphere," to dissolve away negative assumptions, to change popular opinion from a state of ignorant denial to a state of open-minded tolerance and suspense of judgment; while at the same time insisting on adherence to careful scientific methods and on ruthless rejection of anything that is not based on solid, carefully amassed, and tested evidence. To quote James again :

"Is it then likely that the science of our own day will escape the common doom, that the minds of its votaries will never look old-fashioned, to the grandchildren of the latter? It would be folly to suppose so. Yet, if we are to judge by the analogy of the past, when our science once becomes old-fashioned it will be more for its omissions of fact, for its ignorance of whole ranges and orders of complexity in the phenomena to be explained, than for any fatal lack in its spirit and principles."[2]

Oliver Cromwell once said, when getting rather impatient with some bigoted theologians : "For God's sake, gentlemen, consider that you may just possibly be mistaken." I would say to orthodox scientific men: "For Truth's sake, gentlemen, consider that Hamlet's famous remark to Horatio, though now too hackneyed for quotation, may nevertheless be true."

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[1] *Proceedings S.P.R.*, vol. xxiii. pp. 309 *et seq.* : Report by Baggally, Carrington, and Feilding. See also Carrington's book, *Eusapia Palladino and her Phenomena*, and Morselli's *Psicologia e Spiritismo*.

[2] *Proceedings S.P.R.*, vol. xii. p. 10.

MR. J. ARTHUR HILL, who has the opportunity to place before the readers of SCIENCE PROGRESS evidence concerning the survival of human personality beyond the grave, would have done better to have used the space at his disposal in presenting his evidence, instead of paying so much attention to my few cursory remarks published in the last issue. By so doing he would have been able to give a clearer idea of what the evidence is supposed to be, and he would not have given an entirely false impression of the content of my article. Readers of Mr. Hill's paper would imagine that I had written a paper in criticism of psychical research, whereas my article was a criticism of Sir Oliver Lodge's presidential address, concentrated mainly on the scientific side, and the object of the paper was to show that, on that side, there was a valuable contribution to the philosophy of science which was liable to be forgotten because criticism had been concentrated on the few remarks Sir Oliver did make on survival after death and on other matters of religion. The details of the evidence for psychical research I did not attempt to discuss. Certainly I expressed the opinion that the evidence did not convince me, but the point of my remarks consisted, not in discussion of the evidence, but in a statement of the methodological principles which would apply to any attempt to prove anything of the kind from the scientific standpoint.

Concerning my remarks, and Mr. Hill's criticisms, the following short explanation will suffice. The book of the late F. W. II. Myers referred to was the original edition in two volumes. The term monumental was intended to apply, not so much to the length, as to mass of material contained therein, and to the industry, ability, and research shown by the author. The conjuror *was* Mr. Maskelyne, and the reference was to a challenge by him to reproduce, *under similar conditions*, any physical phenomena credited to "spirits" which he had the opportunity of witnessing. I believe the challenge has not been withdrawn, and Mr. Hill would do well to refer to Mr. Maskelyne for further information. I seem to remember also that Mr. Maskelyne *was* present on one occasion at a séance with Eusapia Palladino.

Whether Mr. Hill has said anything likely to carry con-

viction, or to induce any one not at present interested to think there is evidence worthy of investigation, is a question on which I do not think it necessary to express an opinion. I am perfectly well content to leave that matter, as Mr. Hill has stated it, to the readers of Science Progress.

The suggestion concerning the Roman Catholic Church is emphatically not a joke. It is, perhaps, the most serious statement in the preliminary remarks of my article, before I reached the more strictly scientific side. The suggestion had reference not only to survival after death. Sir Oliver Lodge made remarks on survival after death, on Theism, and, if my memory does not fail me, also on miracles. I strongly objected to the introduction of such matters into a presidential address to the British Association. I did so, in the first place, because I thought that he would have done better to have concentrated on the scientific side. I did so, in the second place, because I thought that his patronising attitude towards the exponents of orthodox Christianity was somewhat inconsistent with his statement of belief in some of their most important fundamental doctrines, as if the belief in them were a remarkable new discovery of his own. He seemed entirely ignorant how powerful, and how logical, is the case for orthodox Christianity, and particularly for Roman Catholicism, if once you admit the premises. To those who have a strong interest in religion, as Sir Oliver Lodge appears to have, and who are personally convinced on the three dogmas of God, immortality, and miracles, I repeat, the most logical course is to go and see what Rome has to teach them.

But all this is more or less a side-issue. I am not, and make no pretence to be, an authority on ghosts, on religion, or even on telepathy. Concerning the latter, it is sufficient for me that Mr. Arthur Hill's "dead cert" has not convinced a prominent member of his own Psychical Research Society. And, moreover, if you succeed in proving it, as Mr. Hill has admitted, a mechanical explanation is available. The subject is of considerable scientific interest, but it lies within the sphere of experimental psychology rather than within that of my own subject—logic and methodology, and general philosophy. Any one who wishes to continue the discussion concerning ghosts, would do well, so far as they refer to me, to note my statements concerning the methods of interpreting such

evidence as is available, rather than my opinion concerning its value.

What I desire most emphatically to repeat is that the most important part of my article, as is stated in the article, is to be found in the latter part. And it is the scientific side of Sir Oliver Lodge's address to which I wished to attract attention. In my criticism of his remarks, I expressed views on metageometry, on the principle of relativity, on the principles of mathematical method and their application to scientific theories, which are somewhat at variance with those commonly held among men of science. On several disputed points, I was glad to note that Sir Oliver Lodge held similar views, and I greatly regretted that the value of his support was discounted by the introduction of what could hardly be described as legitimate scientific matter. I would repeat that, in my opinion, to concentrate criticism on the "spiritualistic" side is an injustice to Sir Oliver Lodge. That, however, is a matter for Sir Oliver rather than for me. With regard to my article, I followed him, on that side, with great reluctance, because it was my duty, as a critic, to deal with the address. But, so far as I am personally concerned, I would most emphatically say that, to concentrate criticism on that side of my article, is an injustice to me

REVIEWS

Encyclopædia of the Philosophical Sciences. Vol. I. Logic. By ARNOLD RUGE, WILHELM WINDLEBANK, JOSIAH ROYCE, LOUIS COUTURAT, BENEDETTO CROCE, FERDERIGO ENRIQUEZ, and NICOLAJ LOSSKIJ. Translated by B. ETHEL MEYER. [Pp. vi + 268.] (London : Macmillan & Co., 1913.)

THE title-page is strongly reminiscent of "Widdecombe Fair." The volume consists of an article on the scope and purpose of Logic by each of the authors named. Each presents the subject in a slightly different personal aspect. All the articles are well written. The object of the series of volumes is expressed by the editor in a few well-chosen words : ". . . each volume will consist . . . of original and relatively exhaustive discussions of fundamental aspects of each main subject." This is carried out thoroughly well. Another ideal, which the present volume purports to subserve, and which is also said by the editor to be the mission of philosophy, is to correct the surface tendency of present-day human thought towards divergency. "The field of the thinker's inquiry is becoming ever narrower, and the function of the practical man ever more particular. . . . The theoretic and speculative intercourse of civilised peoples is always becoming more intimate and full. . . . The promoters of the Encyclopædia have set themselves the most difficult, but also the most significant task of giving expression to this unity by means of the very freedom and variety of the writers whom they have enlisted in the service."

The reviewer is not disposed to deny the truth of the view expressed in the first part of the quotation, having himself, on several occasions, asserted the same thing. The last part is an unwarrantable and meaningless paradox. Regretfully it must be stated that, so far as the present volume is concerned, the writers have not given expression to this unity in any matter whatever. If this is the object we must be definite in saying that there are high-sounding words and promises, but no achievement. The volume will be of interest to that small class of people, those absurd contradictions in terms, who, like Mr. Chesterton's rhinoceros, exist but look as if they didn't, the specialists in philosophy. To these, and to amateur dabblers, the book will appeal, but to no one else.

To readers of this journal the main point of interest is that, by several writers, considerable space is given to the treatment of methodology. What is methodology? It is supposed to have something to do with scientific method, and consequently, might be expected to have some interest for men of science. As usually presented it emphatically has not. But the potential scientific interest of the subject, as it might conceivably be presented, will be sufficient excuse for devoting the remainder of the space at our disposal to the methodological aspect.

Prof. Windlebank contributes very little of interest. "Strictly speaking, methodology has no principles of its own. Its principles are to be found in pure Logic, and methodology has only to deal with their application to the different aims of the special sciences" (p. 43). Very admirable, but it does not

do it. The methodologists are like the chorus of policemen in the *Pirates of Penzance*; their song sounds well, but, in the words of Major-General Stanley, they *don't go*. Prof. Windlebank occupies eleven pages but says little more. He concludes by informing us that "*the knowledge of reality* of the empirical sciences . . . possesses *immanent truth in the agreement of the theory with the facts*" (p. 54, italics his). Most of us will be of the opinion that we were aware of this already.

Prof. Royce makes an attempt to depart from the conventional view that methodology is a division or extension of general or formal logic. He regards formal logic as a very subordinate part of methodology. The idea may be said to be in the air. Dr. Schiller and the pragmatists would certainly not repudiate it. The reviewer expressed a similar but less extreme opinion several years ago. Prof. Royce can thus make no claim to originality in the idea itself. Everything depends on the manner in which he carries it out in detail. A methodology of which formal logic is only a part should, at least, be substantial in its content. It would not be reasonable to expect any considerable detail in Prof. Royce's twenty-seven pages. And such general ideas as he has stated are so condensed in exposition as hardly to admit of summary or criticism. Very great prominence is given to Mr. Charles S. Pierce's logic of induction. He brings into strong relief the presupposition that every set of facts has *some definite constitution*. "That is, according to our presupposition, there are possible assertions to be made about these facts which are *either true or false* of each individual fact of the set in question." "'A is a man' is either true or is not true of A." On this supposition, which is said not to be self-evident, all induction and scientific inquiry is based. This is interesting and plausible, but what is meant by "fact." A "fact" concerning which nothing could be asserted as true or false would, indeed, be a curious phenomenon. If it is not self-evident that "fact" implies *definite constitution*, what is self-evident? There is the usual discussion concerning definition, classification, and stages in the growth of science. The specialist in logic will find the discussion well written, interesting, and highly controversial. It contains just those elements so dear to the formal logician.

The few methodological pages of M. Couturat contain a glaring example of pyrrhonism. All axioms other than the "common axioms which are the principles of logic" (what are they?) are merely primary and true for the particular theory under consideration. "We are guided in our choice of fundamental data by quasi-æsthetic reasons." This is pragmatism with a vengeance and, as such, well worth noting. Any one interested must refer to the original for the manner in which it is worked out.

The whole volume is an admirable compilation in its way and will greatly interest logicians. It is a pity, however, that its object should be stated to be to give expression to the fundamental unity of thought underlying the theoretical and speculative intercourse of civilised peoples. It is merely logical specialism, one more specialised science, added to the rest.

H. S. SHELTON.

Scientific Method. Its Philosophy and Practice. By F. W. WESTAWAY. [Pp. xx + 439.] (London: Blackie & Son, Ltd., 1912. Price 6s.)

THE volume is divided into four books entitled respectively: The Philosophy of Scientific Method, The Logic of Scientific Method, Famous Men of Science and Their Methods, Scientific Method in the Classroom.

The first two books call for very brief comment. They are lengthy, encyclopædic, and seemingly without any guiding principle, central idea, or original point of view. The author has evidently read carefully the works of many of the great ancient and modern philosophers, and is also well acquainted with the current text-books on logic. The views of all and sundry are duly noted and are discussed at some length. The volume cannot be regarded seriously as a contribution either to the logic or to the philosophy of science, nor can it be recommended as a text-book for those wishing to acquire a clear knowledge of the current methodology. That being so, it is to be regretted that the first two sections are not condensed to a brief introduction.

In the latter part of the work the author is on his own ground, and his treatment of the principles of pedagogics is worthy of serious consideration. The preface indicates that the volume is intended mainly for the practical teacher, and Mr. Westaway's opinions on that subject should be treated with the respect due to an expert in school routine. Mr. Westaway is an ardent advocate of heuristic methods, especially those of Prof. Armstrong. A somewhat fuller treatment of this point would have been welcome. Unfortunately the discussion is scanty and, moreover, exceedingly didactic. Whether or no and to what extent school children in the process of learning the elementary principles of science and mathematics can be put into the position of discoverers is a problem which deserves fuller and more impartial consideration than it has yet received. Every teacher of every subject will probably say that his main object is to train the pupil to use his own intelligence, but whether or no this object will be served by turning a science lesson into a peculiar ritual yclept heuristic is a controversial question.

What Mr. Westaway does not appear to realise is that the teacher to whom his remarks are addressed has no option but to proceed by the experimental method. If he attempts to adopt Prof. Armstrong's and Mr. Westaway's ideas the attempt can be nothing else but an experiment, and one, moreover, of which the standard of success is uncertain. It is so easy, on insufficient grounds, to call a fad a great discovery. But the matter dealt with is of great interest to a large professional class. It is unfortunate, therefore, that Mr. Westaway did not write a book on the practical teaching of science, a subject on which he is specially competent to speak, and that he did not discuss these current controversies clearly, fully and with the minimum of dogmatism. The exponents of the heuristic method show small disposition to be heuristic in the presentation of their own pedagogics. The author's attempt to combine in a single volume a treatise on pedagogics and an account of the philosophy of scientific method is not very successful.

H. S. S.

Spencer's Philosophy of Science. By C. LLOYD MORGAN, F.R.S. [Pp. 52.] (Oxford : Clarendon Press, 1913. Price 2s. net.)

PROF. LLOYD MORGAN, who is, or has been, a competent specialist in at least the three subjects biology, geology, and psychology, is eminently fitted to be a Herbert Spencer lecturer and to pass judgment on the work of the great synthetic philosopher. The lecture, however, is disappointing. The lecturer tries to cover too much ground and conveys no very clear impression. Moreover, he greatly overstates the importance of " the Unknowable " in Spencer's system. There are a number of interesting points, but the content of the lecture is not well indicated by the title.

H. S. S.

Astronomy. A popular handbook. By HAROLD JAKOBY, Professor of Astronomy, Columbia. [Pp. xi + 435, with 32 plates and many figures in the text.] (New York : The Macmillan Co. Price 10s. 6d. net.)

IN the preface to this volume the author states that it has been written primarily for "the ordinary reader who may desire to inform himself as to the present state of astronomical science, or to secure a simple explanation of the many phenomena constantly exhibiting themselves in the Universe about him." Such a reader will find the second of these desires amply satisfied by this book, the greater part of which is occupied with elementary and lucid explanations of some of the problems of spherical and gravitational astronomy. The explanation of the differences between sidereal, true solar, and mean solar times, of the principles of the sundial, of the methods of determination of the shape, size, and mass of the earth and of the shape and dimensions of its orbit, and of the differences between the Julian and Gregorian calendars are given. The methods by which the masses and distances of the sun, moon, and planets have been determined are described, as are also the methods by which the position of a ship at sea may be found. The phenomena of the precession of the equinoxes, and of the librations of the moon, and the causes of the production of tides, eclipses, and kindred phenomena are also expounded in a simple manner. The variety of the subjects here mentioned will sufficiently indicate the comprehensive nature of this portion of the book. Wherever possible, the derivations of the mathematical results which are assumed in the course of the arguments are given for the benefit of those readers who possess a knowledge of elementary mathematics, these elementary explanations being collected together separately in an appendix.

The portion of the book dealing with descriptive astronomy is not nearly so successful, being very sketchy, incomplete, and disjointed ; and the author would have succeeded better had he not attempted to cover so much ground. The beginner who may wish to study this part of the subject is recommended to turn to other books where it is treated in a much more satisfactory manner.

The author also intended that this volume should serve as a text-book for high schools and colleges. It does not appear to be suitable for this purpose : much is included that would be beyond the grasp of young students, whilst in the case of more advanced students, the subjects here dealt with can be studied to much greater advantage by a more free use of mathematics. The requirements of the student are so different from those of the ordinary reader that it is impossible to meet adequately the needs of both by one and the same volume.

A few incorrect or misleading statements may be mentioned. On p. 125 it is asserted that "so far as gravitational forces alone are concerned, the solar system may endure for ever." The researches of Henri Poincaré have negatived this conclusion, which Laplace and Poisson erroneously believed to follow from their mathematical investigations. On p. 181 the lunar mountains are stated to be from 1,000 to 2,000 feet high, whereas some of the large mountain rings rise to ten times this amount. Also on p. 240 the velocity of light is given as 186,000 miles per sec., whereas on p. 333 the value 183,000 miles per sec. is used.

H. S. J.

Die Physik der bewegten Materie und die Relativitätstheorie. By MAX B. WEINSTEIN. [Pp. xii + 424.] (Leipzig : Johann Ambrosius Barth, 1913. Price 17 marks ; bound, 19 marks.)

THIS book gives a careful and laborious account of the work done on electrodynamics in recent years, divided into two parts, the first presenting the pre-

Einstein treatment of the optical and electrical phenomena in moving systems ; the second the work of Einstein, Minkowski, and their disciples on the theory of relativity. Thus the author devotes considerably more than half of his work to setting out the classical work of Maxwell, Hertz, and Heaviside. His object, as stated by himself, is somewhat in the nature of a protest against the haste of some of the ultra-moderns to throw the powerful theories of the great physicists of the last generation overboard ; he warns them lest, having driven out the old gods, they be compelled to bring them back, should those who have replaced them not fulfil expectation. We think that most physicists will be able to sympathise with this point of view, but at the same time we do not consider that the presentation of the older work is sufficiently original or convenient to justify the amount of space devoted to it. A competent knowledge of the work of the Maxwell-Hertz school, and a clear realisation of the points in which it comes into conflict with experience, is, of course, a necessary preliminary to an understanding of the principle of relativity, and what it seeks to do ; but in view of the large number of excellent books on the older electrodynamics we find the present book unnecessarily diffuse.

The second part of the book deals with the modern theory of relativity, and seeks in particular to reduce the brilliant work of Minkowski to a form more easily understandable than that of the original papers. Einstein's concept of simultaneity and the fundaments of his theory are clearly exposed. The author finds an objection to the theory in the assumption which makes co-ordination of times dependent on so arbitrary a thing as the velocity of light—a difficulty which must have struck every one on their first approach to the principle. It is hard to answer the objection ; probably the best justification of the whole theory is the way in which it gives the dragging coefficient required by Fresnel's formula and the Fiseau experiment, the negative results of the Michelson-Morley and all allied experiments, and the appeal of a transformation which makes the equations invariant. Minkowski's mechanics and electrodynamics are developed in two sections, but, while adequately exposed, are not made much clearer than in the original papers.

The book has all the thoroughness of a German work of the old school, and is a formidable addition to the works on relativity.

E. N. DA C. ANDRADE.

Rays of Positive Electricity, and their Application to Chemical Analysis.
By SIR J. J. THOMSON. [Pp. vi+132.] (London: Longmans, Green & Co., 1913. Price 5s. net.)

To the various series of scientific monographs now appearing Messrs. Longmans, Green & Co. now add their "Monographs on Physics," under the joint editorship of Sir J. J. Thomson and Dr. F. Horton. The first volumes include one on positive rays by Sir J. J. Thomson, and one on the photoelectric effect by Dr. Allen, reviewed elsewhere.

The book on positive rays is not a general account of all the work which has been done on the subject, but rather an account of the recent experiments of the distinguished author, which has thrown so much light on the nature and charge of the material carriers of electricity at low pressure, together with a few selected researches of other authors whose results are of interest in this connection. It is to be welcomed as giving an authoritative summary of the methods and results of his investigation of the last seven years in this field. The general method consists in subjecting a beam of positive rays, passing through a single fine hole

in the cathode, to the joint action of codirectional electric and magnetic fields ; the trace of the deflected rays on a plane perpendicular to the undeflected rays will then be a straight line if the velocity of the rays is constant, and the ratio $\frac{e}{m}$ variable, and a paiabola if the velocity is variable, and the ratio $\frac{e}{m}$ constant, different values of the ratio giving different parabolas (e is the charge on the carrier of mass m). The trace of the rays is detected by means of a photographic plate, and from its nature Sir J. J. Thomson is able to make a series of striking deductions.

The nature of the curves on the plate depends upon the pressure of the gas. When this is not very low (relatively speaking), these are straight lines, generally only two, corresponding to the atom and molecule of hydrogen ; Wien, however, also obtained evidence of the existence of positively changed oxygen atoms. For these pressures the effect is complicated by the fact that, owing to collisions with the gas molecules, these positive rays are not positively changed all the time, but alternate this condition with the neutral and negative state, as Wien showed. The effective charge, e, thus varies according to the fraction of the time during which a carrier is charged with electricity of one kind. In Sir J. J. Thomson's recent series of experiments this effect was avoided by using very high vacua.

In these high vacua experiments, to which a large part of the book is devoted, the main curves are parabolic, to each parabola corresponding a definite value of $\frac{e}{m}$ for the rays producing it. Sir J. J. Thomson has from the different parabolic traces photographed shown the presence of atoms of various elements with positive charges of one or more units, of neutral atoms, and of negatively charged atoms in the "positive" rays in different gases. In addition he has found values of $\frac{e}{m}$ which indicate molecules of various sorts with one positive charge : the number of molecules of different kinds present in the case of a complex gas may be very large ; for instance, in the case of phosgene gas, $COCl_2$, molecules of the composition CO, Cl_2, CCl and $COCl_2$, were found. *Molecules* with a multiple positive charge have never been detected. A strong confirmation of the monatomic nature of helium, argon, and the other inert gases is afforded by the fact that, for them, only curves corresponding to single charged atoms have been observed, while for oxygen and hydrogen, for instance, diatomic molecules are easily detected. Another striking result from the photographs is that all the atoms except hydrogen can acquire multiple positive charges, which agrees with Prof. Rutherford's theory that the hydrogen atom consists of a positive nucleus and only one electron. Mercury, the heaviest atom investigated, acquires from one to eight positive charges ; the maximum number of charges possible appears to depend, not on the valency of the atom, but on the atomic weight.

As the photographs afford no indication of the relative proportions in which the different and differently charged atoms and molecules are present, Sir J. J. Thomson has measured the number of electrified particles of the various kinds present by an electrostatic method, isolating them by a parabolic slit, through which the rays of different kinds are brought to pass by altering the strength of the magnetic field. In this way estimates of the number of positively and negatively charged atoms have been made ; consideration of these results shows

that the atoms of the molecule of a compound gas are not charged with electricity of opposite signs, but each atom contains as much positive as negative electricity, a result of great importance for chemistry.

The method is a very powerful one for finding the weights of the atoms and molecules present in the tube, and has already led to the announcement of a new atom and a new molecule. A parabola for which $\frac{m}{e}$ 3 (taking $\frac{m}{e}$ 1 for the singly charged hydrogen atom) is attributed to triatomic hydrogen. A parabola for which $\frac{m}{e}=22$, which accompanies the neon parabola $\left(\frac{m}{e} \cdot 20\right)$, indicates an atom of weight 22. Mr. F. W. Aston has partially separated such a gas from neon by diffusion ; differences of density in the two components have been actually measured.

Besides the parabolic curves discussed already, there appear on the photographs at lowest pressure straight lines, which the author calls "secondaries." Their origin is the subject of an interesting theoretical discussion, in the course of which the conclusion is reached that the minimum velocity required by an electron to ionise an atom of hydrogen is 11 volts ; this is the value obtained by Lenard in 1903, one of whose students, F. Mayer, has recently redetermined it to be 11·5 volts. The discussion on p. 70 of the amount of ionisation produced by cathode rays is not very clear, as no explicit mention is made of the fact that there is a certain best velocity of the primary electron which produces the most secondary electrons, and the matter is further obscured by the misprinting of "increases" for "decreases" on p. 71. In this connection reference may be made to a paper by C. Ramsauer in the *Jahrbuch der Radioactivität*, ix. 1912, p. 515.

Enough has been said to indicate the extraordinary interest of the researches described. The book, further, contains short accounts of the retrograde and anode rays, of Stark's experiments on the Doppler effect in canal rays, and of experiments on the continuous production of helium and neon by bombardment by cathode rays, affirmed by Sir William Ramsay. Sir J. J. Thomson does not pronounce definitely in favour of any particular source of the gases so liberated.

The book unfortunately contains many oversights. The figures are not always clear ; for instance, fig. 50 is not marked with the letters given in the text, and fig. 29 likewise. There are also misprints, such as "Kulschewski" for "Kutschewski," and misplaced commas sometimes produce odd effects, as in the many cases in the index where Doppler (spelt Döppler throughout) is made to appear as part of the name of the man who has worked on his effect. These are trifles, however. The importance of the book is obvious, and a book by Sir J. J. Thomson requires no recommendation.

<div align="right">E. N. DA C. A.</div>

Practical Exercises in Heat. By E. S. A. ROBSON. Second Edition. [Pp. xii + 213.] (Macmillan & Co., 1913. Price 3s. 6d. net.)

THIS book, written by one evidently experienced in teaching, contains accounts of a number of varied experimental exercises in heat, which are described clearly yet briefly. They are none of them very difficult, all being well within the ability of first and second year men ; the range is, however, wide, and includes many interesting experiments not usually described in books of this kind, such as simple determinations of the calorific values of fuels, and the use of the resistance

thermometer. The experiments on the thermocouple are very simple and neat. We must, however, take strong exception to an experiment described as measuring the temperature of the blowpipe flame; a brass cylinder is heated in the flame, and its final temperature, measured calorimetrically, taken as being the flame temperature. This is, of course, wildly wrong; the melting point of brass is about 900° C., while the true temperature of the ordinary Bunsen flame goes from 1400° C. up to 1800° C. The student could easily convince himself that the brass is only prevented from melting by radiation losses, and other disturbing factors, by fusing thin brass and iron wires in the flame; this would be more instructive as to flame temperatures than the experiment described. With this exception we have only found trifling faults in the book, which is on the whole to be recommended. The working out of actual numerical cases is helpful to the student, and there are some useful tables at the end of the book.

E. N. da C. A.

Photoelectricity. By H. STANLEY ALLEN. [Pp. vii + 221.] (Longmans, Green & Co., 1913. Price 7s. 6d. net.)

IN 1887 Hertz observed that the passage or a spark was facilitated if ultraviolet light fell on the spark gap; and in the next year Hallwachs found that such light possessed the power of discharging plates of certain metals if they were negatively charged, but not if they were positively charged. In 1899 Lenard, and a few months later J. J. Thomson, showed that the action of the light was to set free electrons from the metal thus illuminated; this effect of light in liberating negative electricity has received the name of the photoelectric effect. (It may be noted here that Lenard's fundamental paper first appeared in the *Sitzungsberichte der Kaiserlichen Akademie in Wien*, v. 19, October 1899; it was reprinted in the *Annalen der Physik* in the following year. The paper is quoted by the latter date only in the book under review and other English books. The point is of some importance as regards priority.)

Dr. Allen undertakes to give an account of the work, very extensive in recent years, which has been carried out on the subject; and he has added chapters on the connected subjects of Phosphorescence and Photochemical Action. The chapter on phosphorescence is very welcome, as the recent work in this field has been much neglected in English text-books, and is very important for the information it affords on the mechanism of light emission. As regards the photoelectric effect itself, it is remarkable, considering the number of papers published, how little definite information has been won beyond that contained in the early papers of Hallwachs, Elster and Geitel, and, especially, Lenard. So much contradictory and indefinite work has been done of recent years that the task of arranging it in a clear and connected form is one of great difficulty; if Dr. Allen has not always succeeded in ordering the material and criticising it so as to make clear what are the most reliable results at the present time, he has, in general, given good summaries of the results of the individual experimenters. At the same time we do not think that the amount of space devoted to the different researches is always well chosen; the work of Hughes, which is not very conclusive (*e.g.* the distilled metal surfaces do not seem to give such very satisfactory results as Dr. Allen frequently states. See, for criticism on this and other points, a paper by Pohl and Pringsheim, *Phil. Mag.* December 1913), is treated at very great length, while Lenard and Ramsauer's extensive work on the photoelectric effect in gases, which is not very accessible to English

readers, and so might have been more fully described, is dismissed in a few
sentences.

The book has been written at a rather unfortunate time, since a few months
after its publication a paper has appeared which seems to show the cause of many
of the inconsistencies between different experiments, and to be likely to influence
profoundly the whole field of research. We refer to the work of Fredenhagen
and Küstner, published in the *Physikalische Zeitschrift* for January 1914, where
it is shown that pure zinc, freed from gases by scraping in a very high vacuum,
gives no photoelectric effect at all. If this work, when extended, shows that
other substances too, when absolutely free from gases, give no photoelectric
effect, then the old work will obviously have to be carefully revised.

Dr. Allen's book is useful as giving a correct account of most of the work
which has been done on the subjects he treats, while leaving criticism of it largely
to the reader. In the treatment of the photoelectric effect on water there is no
mention of Obolensky's paper (*Annalen der Physik*, iv. **39**, 1912, p. 961), which
contains the best measurements, and explains previous inconsistencies, Lenard's
latest work on phosphorescence is not touched, and one or two other papers of
some interest are neglected. A few such omissions are almost inevitable ; on the
whole the book is fairly complete, and can be recommended to those interested
in the subject as being the only account to be found in English (excepting
J. J. Thomson's famous book on the conduction of electricity in gases, which
only goes up to 1906) where the researches in this region are collected.

<div style="text-align:right">E. N. DA C. A.</div>

Definitions in Physics. By KARL EUGEN GUTHE, Professor of Physics in the
University of Michigan, and Dean of the Graduate Department. [Pp.
vii + 107.] (The Macmillan Company, 1913. Price 3s. 6d. net.)

THE whole book is taken up with a series of bright " snappy" sentences, giving
in two or three lines definitions of physical conceptions and quantities, such as
light, surface tension, electron, and so on. A few examples will make clear the
nature of the information supplied : " Interference is the destructive or reinforcing
action of different systems of waves upon each other," " Magnetism is the name
of a hypothetical substance producing attraction or repulsion between magnetic
bodies by action at a distance," " Electrolysis is the decomposition of an elec-
trolyte." There are about a hundred pages of this kind of thing, in the course
of which we are told that a rays "are *identical* with ordinary canal rays"
(reviewer's italics).

We cannot imagine any useful purpose to be served by such a book, which
would seem to encourage as part of a scientific education the parrot-like learning
of a few hundred " definitions," necessarily incomplete, generally meaningless as
they stand, and sometimes misleading, if not actually wrong.

<div style="text-align:right">E. N. DA C. A.</div>

The Chemistry of the Radio-elements. Part. II. **The Radio-elements and
the Periodic Law.** By FREDERICK SODDY, F.R.S. [Pp. 46.] (London :
Longmans, Green & Co. Price 2s. net.)

THE importance of this book is not to be gauged from its size ; for it embodies a
classification, with its resulting theories, which will probably prove to be the
greatest stride made in inorganic chemistry since Mendeléeff's time. As with
other cases of the kind, earlier workers had glimpses of the truth, but to Fajans

and to Soddy belongs the credit of the first complete statement of the unifying principle. A little over a year ago, chemists regarded the elements which had been discovered through their radio-activity as being mostly extraneous to the periodic law– chemical sports, whose behaviour seemed little likely to prove amenable to classification. But with the publication, in February of last year, by Fajans and by Soddy, of the principle set out in this book, the chemical relations of the radio-elements with each other and with ordinary elements were suddenly revealed.

The loss of an a-particle by an atom leaves a residual atom which weighs four units less, and belongs to a group two places back from the parent in the periodic classification ; whilst the loss of a β-particle leaves a residue of the same atomic weight belonging to the next higher group. The result of this is that frequently more radio-elements than one must be allotted the same space in the table, and sometimes a radio-element falls into a space already occupied by a common element. There thus arise clusters of elements of slightly different atomic weights, in each of such spaces ; and the author gives the members of such a cluster the convenient name "isotopes." Moreover, it is found that the members of an isotopic cluster cannot be separated chemically from each other, at all events by the means which have been resorted to.

It therefore becomes necessary to modify the notion that in the Periodic Table the rule is "one space, one element," and to recognise that what are ordinarily taken to be homogeneous elements may in some cases be mixtures of stable isotopes. Furthermore, the range of atomic weights within a given isotopic cluster may be great enough to overlap that of its next-door neighbour ; and so, through radio-active instability of some members of each cluster, it could happen that the order of atomic weights of the two spaces would become inverted. Irregularities of atomic weights, such as that between tellurium and iodine, thus receive a tentative explanation.

On the radio-active side, it may readily be believed that the classification is of great aid in elucidating the mechanism of transformations.

It may be thought by many chemists who have followed the experimental evidences for the theory that its upholders take rather too rigid a view of the similarity of isotopes. That the members of a cluster are strongly alike in the chemical tests to which they have been subjected, nobody would deny ; but it is a bold step from this to a statement that they are chemically identical. Further, it may be urged that such a statement restricts the admittedly great extension of our views which the general theory gives, by excluding from the category of isotopes the one case which almost any chemist would now be willing to include— the rare earths. Mr. Soddy mentions this case, but it must be said that his discussion of it is not quite satisfying. Modern methods of following rare-earth separations are extraordinarily delicate, yet the difficulties which are entailed in separation necessitate far more fractionations than have ever been used for radio-element separations ; and one might hazard the remark that if, let us say, any pair of the most closely related rare earths had been tested only by as few fractionations as have been carried out with radio-elements, they might easily have been called "inseparable." The rare earths are surely isotopes, that is, they occupy only one space in the table—indeed, Mr. Soddy seems practically to indicate it— and they are extremely alike in behaviour. Whether they owe their origin to some bygone radio-active series is an interesting matter for speculation. One would have less doubt of the absoluteness of the inseparability of isotopes were it not for the fact that adsorption-effects seem to be too lightly put aside, and both

in this book and in the original papers differences are classed as essential and definite which to many readers seem to be merely important differences of degree. The curve showing the concentrations of adsorbed substance in adsorbent and in the solution often approaches the horizontal, in which case effects similar to those obtained by Fleck, von Hevesy, and others might be accounted for.

Qualitatively at any rate, however, the experimental evidence has undoubtedly shown that isotopes are extremely similar substances ; and, although one could wish for a rather more detailed discussion of " con " as well as of " pro " than appears in either volume of this book, the author has certainly demonstrated how a space in the Periodic Table can be filled by several elements of different atomic weights, and of at least very close chemical similarity.

When other than chemical properties are discussed, the evidence as yet adduced is of course scanty, and too great stress is not laid on the apparent identity of the spectra of ionium and thorium, nor upon the new gas, Metaneon. If these and similar cases turn out to be verified, many of the objections which have been mentioned will naturally be silenced. The author's case might almost have been strengthened had he dwelt less upon some of the rather doubtful positive evidences, such, for example, as the relative volatilies of the emanations. The last adverse remark to be made is that if Uranium X_2, as a unique element, merits the special name of " Brevium," surely also Radium Emanation, no less a new chemical type, deserves its name of Niton ?

Lest it be thought that this review is written in a hostile or carping spirit, one may emphasise the sentiment of the opening paragraph, that to the writer's mind the subject of this book represents the greatest inorganic advance since Mendeléeff ; and every chemist must welcome so attractive and stimulating a scheme, and will admire the skill and ingenuity of its founders.

IRVINE MASSON.

A Dictionary of Applied Chemistry. By Sir EDWARD THORPE, C.B., LL.D., F.R.S. [Pp. viii + 830.] (London : Longmans, Green & Co., 1913. Price 45s.).

THE reviewer of chemical books in his time plays many parts. He may have to place himself by turns in the frame of mind of a university professor, a schoolboy, a manufacturer, a research chemist, or the man-in-the-street. At least three of these mental attitudes are required if one is to review properly the present work ; but, failing the requisite versatility, one may be content to look upon it as an index of the correlation between scientific research and industry as viewed by the representatives of each who contribute to this dictionary. From this standpoint it seems as if we have far to go before the correlation is nearly close enough, at any rate if the evidence here displayed is a true indication. There are noteworthy exceptions among the many articles in the volume, but in the main the trail of the serpent Rule-of-thumb is over them all.

Take, for example, an article which deals with one of the greatest industries -- the manufacture of sodium carbonate. In the whole section on the ammonia-soda process we look in vain for any curves or phase-rule diagrams, despite the fact that the process really depends for its success and further progress upon phase-rule researches no less than upon mechanical ingenuity. One might forgive such an omission in the discussion of the older Leblanc process on the ground of its evolution having taken place less systematically ; but here again some of the very clear and admirable descriptions of constructional details could have been dispensed

with for the sake of more space devoted to theory. An alkali manufacturer is not likely to turn to a dictionary for information as to the mechanical outlines of his own business; but he surely ought to be able to consult it for the purpose of ascertaining fully the why and the wherefore. Naturally, this is the expression of a personal opinion, and it is given only for what it is worth.

But the section on the contact manufacture of sulphuric acid shows that an article dealing with an industry can be scientifically interesting—that is, of great educative value and practical help—to the technical man.

This is the true purpose of a dictionary such as this—to point out to the practical man the virtues of scientific method, and to show him that it is for him to instigate research, not merely to profit by it when he finds it made to his hand. Such articles as that on triphenylmethane dyes, or in yet another field, that on soils, may be quoted as being likely to produce this effect.

The metallurgy articles hardly come up to the standard, and would be much improved by a more "advanced" discussion of physical and physico-chemical properties; indeed, in the article "Tin" there is no section on the properties of the metal at all.

The main criticism to be levelled at the work is, in fact, that despite the great influence which physico-chemical work now has upon industry, that side of the subject receives far too scanty attention; and even in the special articles there is often so sharp a line drawn between theory or practice, or rather between laboratory and works, that no obvious connection is manifest. It is remarkable that the important subject of surface-tension should be ignored, nor is to be found under "Capillarity" in Vol. I.

Several of the larger articles have already been cited as being of high value; and among the others which call for special mention are "Starch," "Water," "Thermit," "Ultramarine," and many of the sections on various vegetable extracts.

IRVINE MASSON.

The Progress of Scientific Chemistry in our own times; with biographical notices. By SIR WILLIAM A. TILDEN, F.R.S. Second Edition. [Pp. xii +366.] (Longmans, Green & Co., 1913. Price 7s. 6d. net.)

SOME fifteen years have passed since the first edition of this book was published, and consequently this, the second edition, embodies many additions. Originally the outcome of a series of "Lectures to Working Men," its eleven chapters constitute a most interesting survey of the development of chemistry during the past eighty years. Sir William Tilden is one of the comparatively few who can properly claim to be competent to pass under review so fruitful a period in chemistry; for, even apart from the wide scope of his chemical interests, the fact that a large section of this time lies within his own recollection gives him a special title to authority, and confers a sense of perspective and proportion which no younger chemist can compass. This is far from saying that the author of this work is *laudator temporis acti*; the significance which he evidently attaches to the most recent, no less than to the earlier, developments would satisfy even the most "modern" of chemists. The said modern, however, is occasionally apt to forget the debt of the past, especially of the recent past; and such a book as this does service in keeping in due prominence the great researches which last century brought forth.

In a sense, chemistry may be said to have been made during the last three generations, and thus it would be small wonder if a history of this period were

somewhat "confused feeding"; but here the sectional treatment of the multifarious developments conduces rather to the proper feeling of unity than to one of complexity. To the many students who wish for a single volume giving a clear purview over the modern foundations of chemistry, this book should prove most welcome.

I. M.

American Chemical Journal. Vol. 50, Nos. 4 and 5 (Baltimore).

Of the six papers contributed to these numbers, the longest deals with work carried out by Guy and H. C. Jones on absorption-spectra of salt solutions as measured with the radio-micrometer. Two papers are concerned with compounds between inorganic salts and organic bases; another describes the preparation of numerous organic compounds. Finally, there are two papers on certain chemical and physical results of high tension discharges. Other matter includes reviews, and reports of Royal Institution lectures by Sir J. J. Thomson and by Dr. Dobbie. The journal is now merged in that of the American Chemical Society.

The Nature of Enzyme Action. By W. M. BAYLISS, D.Sc., F.R.S. Third Edition, revised and enlarged. [Pp. vii + 180.] (London: Longmans, Green & Co. Price 5s. net.)

THE justification for the series in which this monograph takes its place is stated by the general editors to be the facility with which, in this method of publication, the text-book may keep pace with our rapidly growing knowledge of the subject. This can only be accomplished by the repeated issue of fresh editions of separate monographs; new editions which are not reprints, but have been carefully revised to take account of more modern points of view. It is from this standpoint that the present edition of the book needs to be regarded, and, as would be expected from its author, it is an eminently satisfactory example of the value of the monograph method of publication.

Attention will naturally be directed to the main alterations in this third edition. These seem to be significant of modern trends of biochemical investigation, and in particular of the influence of pressing biological problems upon the development of biochemical ideas.

The chapter upon reversibility of enzyme action, for instance, has been practically rewritten, and now includes a valuable discussion of the conflicting evidence as to the part played by enzymes in synthesis.

Recent work on the asymmetric synthesis of carbohydrates by enzyme agency has enabled the author to reaffirm with greater confidence the position he took up in the earlier edition. The whole chapter is very valuable for its insistence that in these complex phenomena the investigator should retain a clear conception of the present position of the theory of catalytic action, and that assumptions not compatible with this position should only be accepted after critical examination of the experimental evidence. This leads the author to a conservative position in relation to the so-called "synthetising enzymes" which will probably prove of more value to progress than an uncritical acceptance of these new suggestions.

It is generally anticipated that we may have to extend our ideas as to the possibilities of enzyme action, with increased knowledge, but nothing is to be gained by a rapid solution of our difficulties through ready acceptance of new properties and new names for enzyme catalysts, which merely hide our difficulties of interpretation under a cloud of words.

In this connection it is interesting to notice the growing importance of Note F.

in these editions ; this note refers to the possibility that the same enzyme may be active in different and allied reactions which it accelerates unequally. The possibilities of the biological applications of this hypothesis are fascinating, and the simplicity of the statement, one reaction, one enzyme, is more apparent than real when dealing with actual physiological problems.

J. H. P.

Metallography. By CECIL H. DESCH, D.Sc., Ph.D. Second Edition, with 14 plates and 108 diagrams. [Pp. x + 431.] (London : Longmans, Green & Co., 1913. Price 9s.)

THE first edition of Dr. Desch's book is so well known to metallographists that no very detailed review of the second edition is called for. The new features do not affect the general plan, although by bringing the book up to date and by correcting earlier errors they naturally enhance the already high value of the work. The treatment of the whole subject might well serve as a model to all scientific authors, and the reader is continually made to feel the sense of satisfaction always imparted by clear and logical exposition united with cogent criticism—factors which diversity of knowledge, as well as depth, on the part of the author, alone can produce. The correlation between metallographic studies and research in other physico-chemical fields is an intimate one, and in the writer's mind the greatest value of this book is that it forces one to realise this intimacy, and indicates very clearly the mutual benefits which metallography and the rest of physical chemistry may expect of one another. In this way, and by his systematic method of examining each part of his subject, Dr. Desch has produced a book which will certainly continue for a long time to be one of the most stimulating works of its kind, alike to workers in physical chemistry and to specialists in metallography.

I. M.

Modern Seismology. By G. W. WALKER, A.R.C.Sc., M.A., F.R.S. [Pp. xii + 88. With 13 plates and diagrams.] (London : Longmans, Green & Co., 1913. Price 5s.)

THIS work is one of the monographs on physics edited by Sir J. J. Thomson and Dr. H. Horton, of the Cavendish Laboratory. The geological aspects of earthquakes are purposely avoided, and seismographs and their records are treated from a mathematical point of view. The author has been profoundly influenced by the vigorous personality of John Milne, with whom he associates Wiechert and Galitzin as the most prominent workers in seismology. Photographs and descriptions are given of the best modern types of seismograph, and the introduction of artificial " damping " is discussed. Even to the non-mathematical, the successive triumphs over unexpected difficulties must appeal. On page 25 we learn, after a discussion of mechanical registration, that " the writing point may remain at rest anywhere within a range of $2r$, and discontinuities of the magnitude may occur in the trace." The author's experiments show, moreover, that r is not a constant, but depends on the state of the smoked surface on which registration takes place and on the amplitude of the movement. We are immediately reassured by the statement that with care the value of r may be reduced on Wiechert instruments to " a few tenth millimetres."

The student will not find much in the book about the nature of earthquake-waves ; he is supposed to have assimilated this in previous reading. The author's familiarity with the appearance and interpretation of seismographic records leads him to be very concise, even when he discusses their interpretation. A few

practical notes are given in Chapter V. on the installation of seismographs, and we are then asked to consider the characters of a record resulting from a shock initiated in a solid isotropic earth. Though such an earth fails to satisfy the actual readings of the instruments, we are not allowed to comfort ourselves with the idea that these readings as yet give us reliable information about earth-shells and variations of density below the surface.

The most marked recent advance appears to be Galitzin's determination of the epicentre of a shock from observations at one station only (p. 64). The science of seismology, as distinct from seismoscopy, is so modern that Mr. Walker, watching the work of his colleagues in this field of delicate measurement, and observing earthquakes critically on his own account at Eskdalemuir, can look forward confidently to successive editions of his treatise.

The Petrology of the Sedimentary Rocks. A description of the Sediments and their Metamorphic Derivatives. By F. H. HATCH, Ph.D., and R. H. RASTALL. With an Appendix on the Systematic Examination of loose Detrital Sediments. By T. CROOK. [Pp. xiii + 425. With 60 text-figures.] (London: G. Allen & Co., 1913. Price 7s. 6d. net.)

THIS book is divided into two portions, of which the first deals with unaltered sedimentary rocks, and the second with the changes such deposits may undergo subsequent to their deposition.

The first portion is by far the smaller, and treats in a general and brief manner the processes of formation, and the classification of stratified rocks which are arranged under the headings Fragmental, Chemical, and Organic. There is no doubt that for purposes of a text-book it is desirable to draw rather hard and fast lines ; but the division between chemical and organic deposits cannot be definite, and there can never be a strictly logical separation of the two groups. The authors have shown a great tendency to concentrate on the second portion of the work and to treat the metamorphic rocks with conspicuous partiality ; not only this, but metamorphism has been extended to the widest limits placed upon the term by trans-Atlantic workers, and has been made to embrace all rocks except those which have suffered practically no change of any description. This is a departure from the system usually adopted in this country, and is not to be recommended.

The elementary student and also the more advanced worker will find this book of considerable use, for collected between its covers is a store of information which previously was scattered through varied and often obscure publications. The worker, however, who turns to this book for detailed descriptions of any particular type of sedimentary deposit will be disappointed, for, unlike the mode of treatment adopted for the igneous rocks in the first volume, the sediments are discussed in general with special reference to their mode of origin and subsequent changes. It is true that specific examples are given in many cases, but these examples are too few in number ; and at the same time it is to be regretted that so many have been drawn from distant foreign localities when there are equally good examples which might have been quoted from Britain.

A most useful Appendix has been drawn up by Mr. Crook on the methods of examination of loose detrital sediments. It is an admirably clear account of the methods usually adopted for the separation of mineral constituents from each other, and it includes data for the determination of the commoner mineral species present in sedimentary deposits. From these data the author might perhaps

have omitted the values of the bi-refringence, for although of prime importance in the case of cleavage flakes bounded by parallel planes it is of little value for irregular grains of which the thickness cannot be determined. The author has placed in deserved prominence the determinative methods of Schroder van du Kolk and Becke, and has added a list of most useful oils which can be used to ascertain the mean refractive index of the minerals he mentions. It is doubtful, however, whether a solution of sulphur in methylene iodide can be made to attain such a high refractive index as 1·83 and retain any degree of stability. The usual figure is nearer 1·79.

The Petrology of the Igneous Rocks. By F. H. HATCH, Ph.D. [Pp. xxiv+454] Seventh Edition. (London : G. Allen & Co, Ltd., 1914. Price 7s. 6d. net.)

THE seventh edition of this useful textbook diffeis mainly from the older editions in the presence of chapters on the Pyroclastic Rocks and the Metamorphic Derivatives of the Igneous Rocks, which, although treated very briefly, are well done, and in keeping with the rest of the book. The classification of Igneous Rocks adopted by Hatch is, however, still shaped by the old qualitative views, in spite of the recent onset of quantitative treatment. It is based mainly on silica percentage, followed, at least in the case of plutonic rocks, by a subdivision into alkalic, monzonitic, and calc-alkalic series. It is difficult to see why the ultrabasic rocks should be excluded from this scheme (p. 160) if it is adequate to the needs of students and petiographers. A rather elaborate quantitative treatment is accorded to the acid plutonic rocks, but it is denied to the more basic. Whilst the volcanic rocks are subdivided on the same basis as the plutonic, the hypabyssal types are apparently not considered as susceptible to this treatment, and are classified into five vaguely defined families. This classification is a patchwork consisting of oddly contrasted compartments in which qualitative and quantitative treatment is alternately adopted. While there is, no doubt, still some advantage to be gained by the student in continuing with the older qualitative classification, it is time the newer quantitative ideas were appearing in the textbooks. The description of the rocks and the account of their distribution is in general excellently done. When Scottish Carboniferous *basalts* are mentioned in several places it is difficult to understand the repetition of the ancient error that *andesites* occur amongst them (p. 413). The terms "texture" and " structure," which now possess well-defined and different significations, are used as though they were interchangeable. The book is free from typographical errors, but the word "spilosite" is used where " spilite" is obviously meant (p. 29).

Text-Book of Paleontology. Edited by CHARLES R. EASTMAN, A.M., Ph.D., Prof. of Paleontology, Pittsburg. Adapted from the German of KARL A. VON ZITTEL. Second Edition, vol. i. [Pp. xi+839, illustrated.] (London Macmillan & Co., Ltd., 1913. Price 25s. net.)

ALMOST the only fault we have to find with this admirable volume is the spelling of the title, an atrocious Americanism, which, together with " Paleozoic " in the text, is enough to make a classically educated Englishman lose his temper. It is, moreover, an insult to the memory of the great German palæontologist upon whose work the present and previous editions are based.

The present volume, which deals solely with the invertebrates, is considerably larger that its predecessor in the first edition (1899), containing 839, against 706,

pages of text, and about 1600, in place of 1476, illustrations: the increase being especially noticeable in the case of the sections on Echinodermata and Arthropoda, which respectively show an augmentation of 38 and 45 pages.

A great change, too, has taken place in the list of specialists responsible for the various sections of this volume: only three out of the twelve names in the first edition reappearing in the second, namely, those of Messrs. J. M. Clarke, W. H. Dall, and C. Schuchert. In the present edition the number of collaborating specialists is no less than seventeen: the new ones being Messrs. R. S. Bassler, W. T. Calman, A. H. and H. L. Clark, J. A. Cushman, A. Handlirsch, R. T. Jackson, A. Petrunkevitch, P. E. Raymond, R. Ruedemann, J. P. Smith, F. Springer, T. W. Vaughan, and C. D. Walcott. A better and more representative list it would be difficult to bring together; each specialist being eminent in his own particular department.

Such a sweeping change in the staff, coupled with the increase in the bulk of the volume, implies of course equally radical changes in the text; so that, as the editor remarks in his preface, the work can no longer be properly styled Zittel's Text-Book, as it is in effect a composite production, although still modelled on the lines of the famous German original. Apart from the importation of new blood, such additions and alterations were inevitable, as invertebrate palæontology has not been standing still during the first dozen years of the present century; and, as a matter of fact—to quote the editor's own words "many parts of the work have been entirely rewritten, others have been emended, rearranged, and enlarged, and the classification in various places has been very considerably altered."

The main groups, however, stand practically as they were in the first edition, the only alteration being that the Echinodermata and the Vermes have changed places, the latter coming first, instead of second, in the present edition.

In regard to these (seven) main groups, the noticeable feature is the inclusion (as in the first edition) of the Porifera (sponges) in the Coelenterata; and since this arrangement differs from that adopted by the majority of zoological writers, it would have been well, we think, if the reasons for the departure from the usual practice had been stated in detail, instead of the reader being left to find them out as best he may. It of course involves a considerable change in the usual definition of the Coelenterata - a change which is not, in our opinion, on the side of simplicity and clearness.

Extreme technicality, it need scarcely be mentioned, is the leading character of the work, which is intended solely for more or less advanced students, and for palæontologists desirous of information with regard to groups which do not form the subject of their special studies. It is not, however, to palæontologists alone that the work should appeal, for it contains much valuable information with regard to certain existing species, notably the nautilus; and if it be thereby the means of inducing zoologists—in the restricted sense of that term to devote more attention (in some cases we may say to devote any) to palæontology than is the practice with many, it will have done good service in helping to place biology on a broader and more philosophical basis.

The excellence and number of the illustrations form an especially valuable feature of the volume; among those worthy of special commendation being the figures of Palæozoic insects, crustaceans, arachnids, and other arthropods, which will come as a revelation to those who have not hitherto devoted attention to this part of the subject. Most wonderful of all these Palæozoic insects are the giant dragon-flies of the Upper Carboniferous, which are regarded as forming a group - the

Protodonata—intermediate between the still more primitive Palæodictyoptera of the Carboniferous and the modern Odonata, or dragon-flies.

In this connection reference may be made to a marked defect in the book, namely that the index does not include groups of higher rank than genera, and that when mention is made in the text of groups other than those under consideration no reference is made to the pages where they are respectively described. In this particular instance, for example, the group Palæodictyoptera is mentioned on page 809, but we have to search through the fourteen preceding pages before there is any possibility of finding out what insects it represents. And such waste of time is trying to the temper ! Moreover, is it not too absurd to spell such names as Palæodictyoptera with a diphthong in the second syllable and Palæontology and Palæozoic without it ?

Reverting to the giant dragon-flies of the Carboniferous, it is mentioned that in *Meganeura monyi*, the largest of them all, the wing-expanse is no less than 75 centimetres ; but it would have been well if some reference had been made to recent speculations with regard to the physical conditions necessary to enable such monsters to fly, which, like the giant pterodactyles of a later epoch, they could not apparently have done if they lived under conditions of atmospheric pressure similar to those existing at the present day.

In the Introduction, which contains an excellent summary of the stratigraphical sequence of rocks and a review of ancient and modern theories with regard to the origin, evolution, and extinction of species, attention may be particularly directed to the following thoughtful passage : " For the extinction of many plants . . . and animals . . . of former periods no adequate explanation has yet been found. Changes in external conditions, especially such as regards the distribution of land and water, climatal conditions, saltness of the water, volcanic eruptions, paucity of food-supply, the encroachments of natural enemies, and diseases, may have led to the extinction of certain forms, but such suggestions signally fail to account for the disappearance of an entire species or particular groups of organisms. Oftentimes extinction seems to have been caused merely by superannuation. Long-lived forms belong for the most part to persistent types whose range of species is limited. Their reproductive functions have declined, and, like an individual in its senescence, they evince the symptoms of decrepitude."

Palæontology, we may observe in conclusion, has been decried as an obsolete and unnecessary science, which ought to be merged in zoology and botany. But there are many and cogent reasons against such a view, not the least of these being a volume like the one before us, which is very nearly the ideal of what a manual of palæontology should be, and which displays before the eyes of the reader a cinematographic sketch of the past history of a portion of the animal kingdom, the vividness and compactness of which would be utterly and completely lost if its contents were amalgamated with a volume on recent zoology.

R. L.

Problems of Genetics. By WILLIAM BATESON, M.A., F.R.S. [Pp. ix+258, illustrated.] (Yale University Press. London: Humphrey Milford, Oxford University Press, 1913. Price 17s. net.)

CERTAINLY it would be a misfortune to the advance of science were Darwinism established as an orthodoxy against which a biologist should write only at the peril of his reputation, and were the writings of Darwin accredited with a plenary inspiration. It would be even more paralysing were the principles expounded by

hat painstaking and luminous genius to be accepted as the final expression and interpretation of natural knowledge, deductions from which were to be regarded as cogent in themselves, and proper refutations of the results of new observers. If there were any approach to this state of affairs, I should offer Mr. Bateson the humble tribute of my sympathy, and even if I did not agree with him, I should gladly put my back against such small portion of his as it might cover. But it is not so. Mr. Bateson, in girding at Darwin, is expanding his wings to the bland and buoyant air of popular approval, and although I do not doubt that it is not his objective, he has become an idol of the market-place. The irony of the position is that those who applaud Mr. Bateson on the rumour that he is an opponent or refuter of Darwin would take little comfort were they at the pains to examine for themselves the direction in which Mr. Bateson would lead them. He believes that the fancies of the living world came about by some evolutionary process, a proposition which was first made credible by Darwin. Whether species have come into existence by the summation of minute variations, a view for which Darwin thought there was just a balance of evidence, and Wallace thought greatly preponderating evidence, or by big jumps, as Mr. Bateson thinks, is a problem of great interest and great importance, but its solution in the sense of Mr. Bateson would lessen not increase the difficulties in accepting natural selection as the fundamental principle of evolution. In my opinion Mr. Bateson is rash in departing from Darwin's caution in refusing to assert that characters are useful because we cannot understand their utility; he does not allow enough for the possible correlation of useless characters with useful characters, and he is going far beyond the book if he thinks it a vital part of Darwin's theory to suppose that every specific character is useful. But even if he were to succeed in ejecting every notion of utility from our conception of the evolutionary process, his disillusioned admirers would find themselves further than ever from Paley, further from teleology, further from a mystical immanence of design. The analysis of organisms into unit characters or factors, the interpretation of the phenomena of variation and heredity as combinations and disintegrations of given unit factors according to numerical law, would make the living world more congruous with the inorganic world, and would not lighten the task of those who propose to interpret it in terms of mind.

These preliminary remarks relate rather to the attitude than to the substance of Mr. Bateson's lectures, for the greater part of the volume is a luminous and quite reasonably impartial account of many of the problems that are still perplexing biologists. These fortunately exist in every branch of biology; it would be a dull world if we understood it all. In his introductory chapter Mr. Bateson relates the problem of species in a fashion which insists on the reality of specific distinctions apart from what has been called their "selection value." He is very severe on systematics, contrasting those who are "engaged in the actual work of naming and cataloguing animals" with biologists. He points out that almost always the collections are arranged in such a way that the phenomena of variation are masked, that the causes of variation are overlooked or confused, and that it is only by a minute study of the original labels of specimens and by redistributing them according to locality and date that their natural relations can be traced. He might have added that the modern museum system of attaching to each specific name a "type" specimen duly registered and labelled, by which the species must in future stand or fall, may be convenient to the systematist, but has helped to divorce systematic work from any true understanding of the natural facts. It is conceivable that the vast labour of systematists in museums may not

be thrown away, but it has been recognised for long that their work will have to be followed by some attempt to delimit what have been called "master species," the real units into which the forms of life are thrown, and that their named species and museum types will at the most serve as index numbers. But at the least it may be said that the large series of individuals, with localities and dates carefully marked, that are now being collected in museums, will be of great service when the work of systematists comes to be translated into science.

The second chapter opens with a statement so astonishing that it is difficult to qualify it with any other term than the term "perverse." "Twenty years ago," declares Mr. Bateson, "in describing the facts of variation, argument was necessary to show that these phenomena had a special value in the sciences of zoology and botany." I cannot conceive how this proposition could be justified, and it is an introduction entirely unnecessary for the extremely interesting discussion to which it leads. Mr. Bateson offers as a preliminary classification of the facts of variation, the distinction between those which are the results of changes in the mode of division, and those which relate to differentiation in the substances divided. He suggests that the first set of variations are possibly in the last resort dependent on the second, and offers valuable comments on the nature of the two processes. He is inclined to think, in this differing from perhaps a majority of modern writers, that substantive variations, depending on differentiation of the substances divided, are more easy to understand than meristic variations. The former may be due to some kind of chemical process; the latter, and, indeed, the nature of division itself, so far remain nothing but observed facts of life. Two chapters are occupied with a clear description, illustrated by many examples, of the different kinds of merism and segmentation, and of the recovery of symmetry in dividing parts. In Chapter IV. substantive variations are discussed, and the attempt is made to correlate them with the unit factors of Mendelian analysis. The suggestion is developed that recessive characters are due to the omission of a factor and dominant characters to the addition of a factor, but this distinction is abandoned in an appendix, and we are left with the tentative picture that all substantive variation is due to the loss of a pre-existing factor—a conception that does not appear to carry the argument on to any very useful plane. In Chapter V. there is a very fair account of the failure of Mendelian analysis to account for the phenomena of mutation in *Oenothera*.

In three interesting and detailed chapters the relations between geographical distribution and variation are discussed, with, however, the result of showing that Mendelian methods are as yet no more satisfactory than any other methods in explaining the relation of geographical races to species. It is obvious, as Mr. Bateson suggests, that no great advance can be made in this direction until extensive breeding experiments have been undertaken. In Chapters IX. and X., under the title "Adaptation," Mr. Bateson discusses recent evidence for the inheritance of acquired characters, and dismisses it partly on the ground that it is unconfirmed, but even more cogently because in the alleged cases the normal course of inheritance under undisturbed conditions is not sufficiently known. The utmost length to which Mr. Bateson thinks the evidence can be stretched is to suppose that in some parthenogenetic forms, variations, produced in response to special conditions, recur in one or two generations after the removal of those conditions, and that violent maltreatment may in rare cases so affect the germ cells contained in the parents as to bring about in the offspring, resulting from the fertilisation of these germ cells, an arrest of development similar to that which their parents underwent. Examination of the present condition of knowledge as

to the sterility of hybrids leads to similar negative conclusions, and the best hope
that Mr. Bateson has to bring us is the expression of his conviction that the
prospect of permanent progress is greater if science retreats from the speculative
position which he thinks it has occupied. He states his belief that new light will
most probably come from the pursuit of genetic research. Those who think that
a little speculation is the salt of experimental work, can at least comfort themselves
with the reflection that there seems no dissociation of any radical nature between
speculation and genetic research on Mendelian lines.

<div align="right">P. C. M.</div>

A Possible Physical Aspect of the Trichromatic Vision Theory. By C.
TIMIRIAZEFF, F.M.R.S. [Pp. 12.] (Moscow, 1913.)

THIS pamphlet is an ingenious attempt to associate Edridge-Green's Theory of
Vision with the Trichromatic Theory. The author suggests that the distribution
of the red sensation corresponds to the perception of amplitudes of the vibrations
that the green sensation curve corresponds to the absorption curve of the visual
purple, having its maximum in the green, and sloping to the limits of the spectrum:
the third curve having its maximum in the violet is attributed to the perception
of the oscillation frequencies. It is difficult to suppose that the sensation of red
can be only caused by the perception of amplitude any more than a treble note
should become a bass one when struck very violently. The author in support of
this view alludes to the decrease of the red sensation with the decrease of the
intensity of light, and the converse effect. This pamphlet will be interesting to
those who incline to the Trichromatic Theory, but to those who claim that the
Trichromatic Theory is quite untenable, and that the objections to it have never
been answered, it will only be regarded as an ingenious speculation. So many
of the so-called facts of colour-vision are merely speculations based upon the
theory, and do not bear any relation to the actual facts. The terms red-blindness
and green-blindness convey no meaning to us, as different varieties are classed
under the same heading. A theory of colour-vision should be able to account
for the facts as given, for instance, in Professor Starling's *Text-book of
Physiology*.

Irritability. A physiological analysis of the general effect of stimuli in living
substance. The Silliman Lectures delivered at Yale University in 1911-12.
By MAX VERWORN, M.D., Ph.D., Professor at Bonn Physiological Institute.
With Diagrams and Illustrations. [Pp. xii + 264.] (New Haven: Yale
University Press. London: Henry Frowde, Oxford University Press,
1913. Price 15s. net.)

PROF. MAX VERWORN is no mean successor to those who have preceded him in
the Silliman lectureship, the names of whom include Sir J. J. Thomson, Professors
Sherrington, Bateson, and Svante Arrhenius. The present volume will add to the
very high reputation which Prof. Verworn already enjoys both as an investigator
and writer. The subject which forms the title of the lecture deals with a property
of the living substance which is perhaps its most fundamental characteristic, and
the author has made it peculiarly his own, for the volume deals with his researches
which have covered a period of twenty years.

　　Those acquainted with Verworn's *General Physiology* will know that the author
was one of the earliest to recognise the importance of a study of the subject from
the comparative standpoint; the muscle-nerve preparation from the frog's leg is

still the sheet anchor of those who investigate the phenomenon of irritability, but fresh light is shed upon the problem by employing other kinds of excitable protoplasm such as are found in the protozoa. Prof. Verworn emphasises the importance of this branch of work in the book just published.

His first chapter deals with the history of the subject from the days of Francis Glisson onward, and his second with the general principles of scientific research, especially in relation to the conception of the word cause. It is not until we reach Chapter III. that the author really launches out into his subject proper, and in this and succeeding lectures deals in turn with the varieties of stimuli and the manner of response. We have, for instance, a discussion of the Weber-Fechner law which mainly operates in sensory phenomena, and the "all or nothing" law which he regards as the rule in motor responses. It is pointed out that stimulation as a rule leads to katabolic effects, the intakes of food being practically the only stimulus which leads to anabolism. The processes of induction are of necessity included with those of irritability. The importance of a due supply of oxygen is largely dwelt upon, for it is upon an interference with this that the phenomena of "refractory period" and of fatigue depend. The concluding chapters deal with the "interference" of stimuli, and with inhibition which is largely due to interference, and with the very important question of depression of irritability as especially illustrated during narcosis.

Such a hasty summary of the main contents of the book is sufficient to illustrate the important nature of the themes it treats of, and one hopes it will be sufficient to induce those interested in physiological advance to purchase it. It is, however, only right to warn intending purchasers that they are not to expect light and easy reading. It is not suited to beginners, for it presupposes a groundwork of physiological knowledge. To the advanced student or professed physiologist, especially if he is a rapid thinker, some of the sections, moreover, may prove rather "irritating," for they labour points which are pretty obvious.

The translation has been admirably carried out by Frau Verworn, and in a book published in the United States one naturally forgives such Americanisms as center and acetat. But the words functionation, oxyclable, and excitate do not strike one as happy; they are not English and one doubts whether they are even American.

<div align="right">W. D. H.</div>

Applied Mechanics for Engineers. By J. DUNCAN, Wh.Ex., M.I.Mech.E. [Pp. xv + 718.] (London: Macmillan & Co., 1913. Price 8s. 6d. net.)

THIS book deals in two parts with Materials and Structures, and Machines and Hydraulics. There is, of course, no logical reason for not dealing with heat engines and other branches of engineering, when treating with mechanics for engineers. But no one can complain that enough has not been crowded into the seven hundred odd pages as it is. It may be said at once that the book will be found very useful for students who have been properly taught, and who wish to possess in a concise form a summary of the material dealt with in the usual engineering examinations. They will be further helped by the numerous examples, particularly as answers to these questions are given at the end of the book. All that can be done by a good compiler and an excellent printer has been achieved. The formulæ and tables are admirably clear, and the illustrations throughout are excellent and carefully drawn. The student will find that the numerous proofs of different formulæ are stated in a concise form, and the cross references are conveniently given. It is quite necessary for a student to understand that he cannot

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hope from this book alone to study and understand the very large number of problems that are dealt with unless he has had a good technical and mathematical training. Thus the intimation in the preface as to the amount of mathematics required appears to be misleading, and there are very few students who could understand the differential and integral calculus from the few remarks given in the first chapter. In fact, this chapter is only to be justified by the point of view of reference. To take another instance, the whirling of shafts, which is really a difficult subject, should at least contain a reference to Prof. Dunkerley's original investigations in the *Phil. Trans.* to enable the student to understand the subject properly, since there are variations in whirling which are dealt with in that paper which might easily be called for by an examiner as well as the simple cases given by the writer. Aided by the instruction of a good teacher, who can refer the student to other books for further information on subjects such as the Ellipse of Stress and various other problems, the book might be safely recommended to students of engineering.

BOOKS RECEIVED

(Publishers are requested to notify prices)

Chemistry and its Borderland. By Alfred W. Stewart, D.Sc., Lecturer on Organic Chemistry in the Queen's University of Belfast, Formerly 1851 Exhibition Research Scholar and Carnegie Research Fellow. With 11 Illustrations and 2 Plates. Longmans, Green & Co., 39, Paternoster Row, London, New York, Bombay, and Calcutta, 1914. (Pp. xi, 313.) Price 5s. net.

Photo-Chemistry. By S. E. Sheppard, D.Sc. (Lond.), Formerly 1851 Exhibition Research Scholar of University College, London. With Illustrations and Figures. Longmans, Green & Co., 39, Paternoster Row, London, New York, Bombay, and Calcutta, 1914. (Pp. ix, 461.) Price 12s. 6d.

Quantitative Analysis in Practice. An Introductory Course designed for Colleges and Universities. By John Waddell, B.A. (Dalhousie University), B.Sc. (Lond.), Ph.D. (Heidelberg), D.Sc. (Edinburgh), Formerly Assistant to the Professor of Chemistry in Edinburgh, Assistant Professor of Chemistry School of Mining (Queen's University), Kingston, Canada. London : J. & A. Churchill, 7, Great Marlborough Street, 1913. (Pp. vi, 162.) Price 4s. 6d. net.

The Great Scourge and How to End it. By Christabel Pankhurst, LL.B. London : E. Pankhurst, Lincoln's Inn House, Kingsway, W.C., 1913. (Pp. xi, 155.) Price 1s.

A Course of Practical Work in the Chemistry of the Garden. For Teachers and Students of Horticulture, Gardening, and Rural Science. By D. R. Edwardes-Ker, B.A. (Oxon), B.Sc. (Lond.), Head of the Chemical Department and Lecturer in Agricultural Chemistry at the South-Eastern Agricultural College (University of London), Wye, Kent ; Joint Author of " Practical Agricultural Chemistry." London : John Murray, Albemarle Street, W., 1914. (Pp. 40.) Price 1s. 6d. net.

Controlled Natural Selection and Value Marking. By J. C. Mottram, M.B. (Lond.). Longmans, Green & Co., 39, Paternoster Row, London, New York, Bombay, and Calcutta, 1914. (Pp. vii, 130.) Price 3s. 6d. net.

The Universe and the Mayonnaise, and Other Stories for Children. By T. Brailsford Robertson. Illustrated by K. Clausen. London : John Lane, the Bodley Head. New York : John Lane Company. Toronto : Bell & Cockburn, 1914. (Pp. 125.)

Health Preservation in West Africa. By J. Charles Ryan, L.R.C.P.I., L.M., L.R.C.S.I., L.M., Diplomate in Tropical Medicine, University, Liverpool, Late M.O. West African Medical Staff. With Introduction by Sir Ronald Ross, K.C.B., F.R.S., Nobel Laureate, M.D., D.P.H., F.R.C.S., D.Sc., LL.D. London : John Bale, Sons & Danielsson, Ltd., Oxford House, 83–91, Great Titchfield Street, Oxford Street, W., 1914. (Pp. xv, 96.)

Studies in Water Supply. By A. C. Houston, B.Sc., M.B., C.M., Director of Water Examination, Metropolitan Water Board. Macmillan & Co., Ltd., St. Martin's Street, London, 1913. (Pp. ix, 203.) Price 5s. net.

Artificial Parthenogenesis and Fertilisation. By Jacques Loeb, Member of the Rockefeller Institute for Medical Research. Originally translated from the German by W. O. Redman King, B.A., Assistant Lecturer in Zoology at the University of Leeds, England, Supplemented and Revised by the Author. The University of Chicago Press, Chicago, Illinois. Agents : Cambridge University Press, London. (Pp. x, 312.) Price 10s. net.

A Way of Life. An Address to Yale Students, Sunday Evening, April 20, 1913. By William Osler. London : Constable & Co., Ltd., 1913. (Pp. 61.)

Some Main Issues. A Collection of Essays. By G. Walter Steeves, M.D. London : Chapman & Hall, Ltd., 1913. (Pp. iii, 109.) Price 3s. 6d. net.

Ancient Egypt. Quarterly Journal, Part I. Editor, Prof. Flinders Petrie, F.R.S., F.B.S. Macmillan & Co., London and New York, and British School of Archæology in Egypt. University College, London. (Pp. 48, Illustrated.) Price 2s. net.

A School Statics. By G. W. Brewster, M.A., Senior Mathematical Master at Oundle School, and C. J. L. Wagstaff, M.A., Headmaster, Haberdashers' Hampstead School. Cambridge : W. Heffer & Sons, Ltd., 1913. (Pp. viii, 248.) Price 3s. net.

Modern Methods of Water Purification. By John Don, F.I.C., A.M.I.Mech.E., and John Chisholm, A.M.I.Mech.E., Engineer and Manager of the Airdrie, Coatbridge and District Waterworks. With 106 Illustrations. Second Revised and Enlarged Edition. London : Edward Arnold, 1913. (Pp. xvii 398.) Price 15s. net.

Physical Chemistry : Its Bearing on Biology and Medicine. By James C. Philip, M.A., Ph.D , D.Sc., Assistant Professor in the Department of Chemistry, Imperial College of Science and Technology. Second Edition. London : Edward Arnold, 1913. (Pp. vi, 326.) Price 7s. 6d. net.

Heredity and Sex : Columbia University Lecture. By Thomas Hunt Morgan, Ph.D., Professor of Experimental Zoology in Columbia University. Humphrey Milford, Oxford University Press, London, E.C., and at Toronto, Melbourne, and Bombay. (Pp. ix, 282.) Price 7s. 6d. net.

Flies and Mosquitoes as Carriers of Disease. By Wm. Paul Gerhard, C.I., Member American Society Mechanical Engineers, Consulting Engineer, Doctor of Engineering. New York, 1911. Published by the Author, 39, Strong Place, Brooklyn, New York. (Pp. 14.) Price 25 cents.
  The sub-title of this paper is "What Farmers can do to Assist in the Campaign against Flies and Mosquitoes." The work is well designed for instruction in this particular, as regards the United States, and farmers even in this country often have to deal with mosquitoes in the summer, especially in certain parts. The author does not appear to have read all the large literature either on malaria or on yellow fever ; but the instructions for practical work will be useful.

Journal of Ecology. Vol. I. No. 4, 1913. Edited or the British Ecological Society by Frank Cavers, Cambridge University Press. (Pp. 64.) Price 5s.

NOTES

The Sale of Honours

The feast of unreason called party politics is the last kind of banquet which men of science should attend; but nevertheless men of science have definite duties to perform towards the State—it should be their part to throw the cold light of reason upon the welter of clashing interests. As a matter of fact, however, they as a body take almost no part whatever in public affairs. The light of reason remains unthrown; and in the darkness we hear only the howls of the combat between the interested people who are trying to rob each other and the time-servers who are pretending to lead them. There are, however, some signs of awakening interest among the more intelligent people in the country—combined with a rapidly increasing sense of resentment against the politicians of both parties. It is beginning to be seen that these people are sacrificing the interests of the whole empire in the pursuit of the game which they play—to their own profit and to the loss of the nation. Mr. John Galsworthy, the dramatist, has performed a public duty by calling attention in the *Times* of February 28 to the "heartlessness of Parliament" he might have said "inefficiency." He complains that a large number of important reforms remain quite ignored by the body which is supposed to govern us; and we could easily add to his list many, and many more important, matters which have been fruitlessly calling for attention during the last century or more—such as the perfectioning of the Common Law, the encouragement of the arts and sciences, and the removal of innumerable abuses which are, if anything, favoured by the party politicians. But the things which are most effectually rousing even the most brainless to the evils of party government are, first, the state to which the parties are bringing Ireland, and, secondly, the infamous abuse to which Lord Selborne called attention in the House of Lords on February 24 —the public sale of honours to persons who purchase them by subscribing funds to the parties—a thing which we should

expect to find more easily in Turkey or China than in a country which thinks that it possesses the hegemony of the world. In other words, the great honours of the State, which should be reserved only for the highest services to the world or to the State, are given for perpetuating a disease of government which does not really belong to our Constitution at all, and which every person accustomed to correct reasoning must look upon with dislike and contempt. The world is beginning to perceive that the next great reform which it must undertake is the expulsion of the party politician from public affairs.

The Royal Society

List of recommendations by the Council for the Fellowship in 1914: Dr. E. J. Allen, Mr. R. Assheton, Mr. G. T. Bennett, Prof. R. H. Biffen, Dr. A. E. Boycott, Mr. Clive Cuthbertson, Dr. H. H. Dale, Prof. A. S. Eddington, Prof. E. J. Garwood, Mr. T. H. Havelock, Dr. T. M. Lowry, Prof. D. Noël Paton, Mr. S. Ruhemann, Dr. S. W. J. Smith, and Dr. T. E. Stanton.

The British Association

The Committee for Radiotelegraphic Investigation have issued a circular asking for assistance in connection with the forthcoming eclipse. Communications should be addressed to the Hon. Secretary, Dr. W. Eccles, University College, London, W.C.

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